

**Changing social scientific research practices
negotiating creative methods**

by

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Abstract

In recent decades social scientists have started to use qualitative creative methods¹ more and more, because of epistemological and methodological developments on the one hand and demands of innovation by governmental funding agencies on the other. In my thesis I look at the research practices of social scientists who use these qualitative creative methods and answer the following main research question:

How are practices and approaches from the arts (specifically visual lens-based arts, poetry, performance and narrative) negotiated in social scientific research practice?

This question has been divided into the following three sub-questions:

- *How do social scientists negotiate the use of creative methods with other members of their research community?*
- *How do social scientists negotiate the use of creative methods into their own research practices?*
- *And how do creative methods emerge in the process?*

Using Lave and Wenger's approach to communities of practice (1991; Wenger, 1998) and Ingold and Hallam's (2007) conceptualisation of improvisation for my theoretical framework, I look at these practices as constantly emerging and changing, but at the same time determined by those same practices. Based on ongoing conversations with postgraduate research students, interviews with experienced researchers, participant observation at conferences and videos of my participants' presentations, I conclude that the use of creative methods is always embedded within existing research practices. When this is not the case, either participants themselves or other academics experience the creative methods as problematic or even as non-academic. In those cases boundary-work (the in- and exclusion of what is deemed academic) is performed more fiercely, making it difficult, if not impossible for creative methods to be truly innovative in the sense that it means a break with previous practices. Instead, we see small shifts in participants' academic practices and how creative methods are taken up in these practices. This means improvisation is a more apt term to describe how creative methods are making their way into social scientific research practices/into the social sciences. As such this conclusion has consequences for the way we think about learning

¹ I use the term qualitative creative methods to refer to methods that have visual lens-based arts, poetry, performance or narrative components in the production of their qualitative data and, or the dissemination of their research in a qualitative way.

methods, the production of knowledge, innovative methods and (inter)disciplinarity.

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Chapter One: Introduction

The social sciences have devoted a lot of attention to research methods over the last decades (Hammersley, 2011, p. 17; Puustinen, Garrington & Wiles, 2014). More specifically, attention to qualitative *creative* methods has grown in disciplines such as anthropology, sociology, media and communication studies, pedagogy and education, human geography, health and sport studies (Beck, Belliveau, Lea & Wager, 2011; van Romondt Vis, 2007; Wiles, Pain & Crow, 2010; see also the journals *Qualitative Research*, *Qualitative Inquiry* and *FQS*). I elaborate on what qualitative creative methods entails below in the *Creative Methods* section, but for now it suffices to say that I am referring to methods that have visual lens-based arts, poetry, performance or narrative components in the production of their qualitative data and/or the dissemination of their research in a qualitative way.

The growth of attention is evident in the number of publications, conferences and training schemes to be found in the UK and elsewhere. For example, there are journals that focus almost exclusively on creative methods, such as *Qualitative Inquiry*, *Cultural Studies* ←→ *Critical Methodologies* (both published by Sage) and *FQS: Forum Qualitative Social Research* (an open access online journal). Methods textbooks have increasingly added, albeit reluctantly by some (Silverman, 2007), chapters on visual, performative and narrative approaches (e.g. Delamont, 2012; DeLyser, Herbert, Aitken, Crang, & McDowell, 2010; Denzin & Lincoln, 2005; Denzin & Lincoln, 2010; Glesne, 2011; Leavy, 2014). More importantly, there are textbooks dealing specifically with creative approaches (Cahnmann-Taylor & Siegesmund, 2008; Hesse-Biber & Leavy, 2008; Janesick, 2011; Knowles & Cole, 2008; Leavy, 2009; Margolis & Pauwels, 2011; Prendergast, Leggo & Sameshima, 2009; Savin-Baden & Howell-Major, 2010). Conferences, too, have special strands for creative methods (Silverman, 2007, p. 132), in some cases it is even their main theme.² These developments are mostly driven by the work of individual researchers using creative methods, but there is also a governmental initiative, the National Centre for Research Methods

² Examples of conferences with special strands for creative methods are the Annual Conference at the Wales Institute of Social & Economic Research, Data & Methods (WISERD) on 25 and 26 June 2013 and the biannual ESRC Research Methods Festival, organised for the sixth time in 2014. A few recent examples of conferences with creative methods as their main theme are the Qualitative Ethnographic Research Network Conference at the University of Exeter (QERN, 10 May 2013); the eighth annual conference of Methodological Innovations at Plymouth University (4-5 December 2013); the twelfth International Visual Sociology Association (IVSA) annual conference (26-28 June 2014); the third International Visual Methods Conference (2-6 September 2013); the second Pedare Conference at Falmouth University (12 September 2013); the fifth International Symposium on Poetic Inquiry at the University of British Columbia (8-10 October, 2015); the fourteenth annual Qualitative Methods Conference (formerly Advances in Qualitative Methods (AQM) Conference) in Melbourne, Australia (28-30 April 2015); and the eleventh International Congress of Qualitative Inquiry at the University of Illinois (20- 23 May 2015).

(NCRM). The NCRM was founded by the Economic and Social Research Council (ESRC) in 2004, with one of their aims being to maintain the UK's leading position in the social sciences "within the contexts of global competition and the knowledge economy" by providing an infrastructure to develop innovative quantitative and qualitative research methodology (Taylor & Coffey, 2008, p. 7). Since then, they have sponsored several projects involving qualitative methods, in general, and creative methods, in particular. In addition, the NCRM have held consultations concerning research needs in research methods (Bardsley & Wiles, 2006; Luff, Wiles & Sturgis, 2015; Wiles, Bardsley & Powell, 2009, pp. 9-10) and made calls for networks for methodological innovation and methodological innovation projects (NCRM, 2011), which all included what I am here calling 'creative methods'.

There are various reasons the attention for and use of these methods has grown which I only briefly mention here, but describe more in-depth in the *Creative Methods* section below. Epistemological and methodological developments in the social sciences – such as the crisis of representation, the crisis of legitimation and embodied, sensory and emotional ways of knowing the social world – have required other forms of conducting research (Denzin & Lincoln, 2003, pp. 25-29). In addition, a more general academic demand for innovation from funding bodies and researchers themselves (NCRM, 2011; Taylor & Coffey, 2008, p. 7; Wiles, Bardsley & Powell, 2009, pp. 9-10) has encouraged the use of creative methods.

These developments have inspired many researchers to adopt more sensory and performative ways of conducting research to engage with those issues. Indeed, much has been written about the advantages of those methods over more realist approaches on certain accounts (Gergen & Gergen 2012; Görlich, 2015; McIntyre, 2003). That said, many researchers are clear that these methods *add* to the palette of social science research methods, but are not a replacement of other methods (Edwards & Weller, 2012, p. 203; Richardson, 2006, p. 5; Sparkes, 2009a).

Despite the excitement, there is also apprehension about creative methods. On the one hand, these methods supposedly provide social scientists with 'better' or at least different ways of understanding and representing the social world (van Romondt Vis, 2007). There is a lot of discussion about creative methods and their place in the social sciences (e.g. Gergen & Jones, 2012), what kind of knowledge these methods produce, how it is produced (e.g. Buckingham, 2009; van Romondt Vis, 2007; 2010) and what can be done with that type of knowledge. Some feel these methods are a bandwagon to jump on (Becher & Trowler, 2001, p. 96). As one of my participants, a professor in sociology, said: "There is a rush to the visual." Yet, visual and other creative methods might not always be suitable for the specific

topic of the research (Pink, 2007, p. 43). In addition, some of the skills involved in creative methods, such as performance, raise doubts about whether or not the different disciplinary skills can be brought together (see, for instance, Lafrenière & Cox, 2013; Piirto, 2002). Despite funding bodies' encouragement of interdisciplinarity in research projects, there are all kinds of issues that make interdisciplinary work hard to do. And as another one of my participants, also a professor in sociology, said: "We are not good at it." Research practices are often taken up by other disciplines without genuinely engaging with the discipline's knowledge (Schoenberger, 2001) because being open to other disciplines and their historical development can be hard (Pink, 2003, p. 191). Schoenberger (2001, pp. 366-367) points out several difficulties in doing interdisciplinary work. She says disciplines often speak a 'different' language. Disciplinary definitions of terms slightly vary, as do the way disciplines approach topics. Furthermore, what counts as data or evidence and how that is transformed into knowledge can differ across disciplines. How these methods are applied and how they can be assessed are thus important questions to understand how research is done. Yet, despite the growth of creative methods within the social sciences, much less attention has been given to how these methods have been taken up in practice and how social scientists have appropriated these methods into their research practices.

Therefore, in this thesis, I look at how creative methods are applied in practice, the challenges and opportunities that taking up these methods poses for researchers, both novice and experienced, and what doing so means for learning methods, the production of knowledge and the development of methods and interdisciplinarity. This has led to the following main question:

How are creative methods (specifically, visual lens-based arts, poetry, performance and narrative) negotiated in social scientific research practice?

My sub-questions are:

- 1. How do social scientists negotiate the use of creative methods with other members of their research communities?*
- 2. How do social scientists negotiate the use of creative methods into their own research practices?*
- 3. And how do creative methods emerge in the process?*

Answering these questions allows me to add to the literature on knowledge production, to

contribute to the development and teaching of research methods and what that means for interdisciplinarity and, crucially, to look critically at the innovation discourse currently associated with creative research methods. In the remainder of this introduction, I explain more about the main topic of this research, creative methods, and I return to other issues so far briefly raised. I start with explaining my understanding of creative methods by discussing my choice for this particular term over others, the rise of such methods and the current field. I end by giving an outline of the structure of this thesis.

Creative Methods

‘Creative methods’ is one way of labelling what I examine in this thesis, but there are others. Some terms refer to the supposedly new status within the social sciences, for example, calling them “emergent” (Hesse-Biber & Leavy, 2008) or “innovative” methods (Pain, 2009; Taylor & Coffey, 2008; Wiles, Pain & Crow, 2010; Xenitidou & Gilbert, 2009, pp. 60-65). Initially, I planned to use ‘innovative methods’, because I often heard academics use the term when they referred to methods using visual lens-based, poetry, performative or narrative elements. However, I soon realised that this was a wholly inappropriate term, as I argue in Chapter Two’s *Academic Practices and Innovation* section, mainly because it denies the connections these methods have with previous methodological practices.

Other terms emphasise the epistemological background of the methods, labelling them, for example, as “art-based” or “arts-informed” research. Cole and Knowles (2008, p. 59) define arts-informed research as: “a mode and form of qualitative research in the social sciences that is influenced by, but not based in, the arts broadly conceived.” This definition is inclusive in terms of different types of arts and the role they play in the research (as a form of data, representation or the practice itself as a way of knowledge production). It thus might seem appropriate to use for my research, but there are a number of reasons why I have decided not to. For one, arts-informed research has strong ties with educational research. Using this term would obscure some of the other roots that creative methods have, such as in anthropology (see, for instance Prosser, 2008; Richardson, 1992; 1994). In addition, despite its broad definition, the term ‘arts-informed research’ already implies an interpretive methodology and a participatory action research approach which is aimed at making connections with non-academic communities (Finley, 2003). In fact, in the editorial to a special issue on arts-based research, Finley (2014) emphasised the specific emancipatory and participatory aspects of arts-based research. However, creative methods are not always

applied in an emancipatory and participatory way. For my research, this was a crucial point: considering the different ways in which researchers apply creative methods necessarily meant that I had to be open to researchers who applied them differently. Furthermore, I feel that using ‘arts-*informed* research’ can become quite confusing because the term is very similar to ‘art-*based* research’, though these terms quite differ in their focus. The latter is a more specific strand within arts-informed research, which “can be defined as the systematic use of the artistic process, the actual making of artistic expressions in all of the different forms of the arts, as a primary way of understanding and examining experience by both researchers and the people they involve in their studies” (McNiff, 2008, p. 29). McNiff goes on to more sharply distinguish it from arts-informed research where “the arts may play a significant role but are essentially used as data for investigations that take place within academic disciplines that utilize more traditional scientific, verbal, and mathematic descriptions and analyses of phenomena” (ibid.). Another reason I feel the term ‘arts-informed research’ can be misleading is because it is not always about art (Clover, 2011, p. 22). Finally, most of my participants did not commonly refer to their work as arts-based or arts-informed research.

Another term I encountered was ‘creative analytic practices’ (CAP) (Richardson, 2003; B. Smith, 2009), which includes all ethnographic writing that “has moved outside conventional social scientific writing” (Richardson, 2003, p. 509). For my research, this term unfortunately did not suffice because it does not include methods of data-gathering, whereas I *do* include those in my research.

‘Creative methods’ is a much less defined term than arts-based research, but still refers to methods that employ creativity and the arts (Prosser & Loxley, 2008; Wiles, Pain & Crow, 2010). This means I can look at the methods as they are being applied, rather than how they should supposedly be used, and see how they are connected to earlier practices. Even so, the choice to label the methods in my thesis as ‘creative’ could of course be debated. The word ‘creative’ is hard to define (Kara, 2015, pp. 5-16; Gauntlett, 2007; Tarr, 2014, p. 6), and it is questionable whether these methods are indeed creative (Buckingham, 2009). However, I find that for this particular research, these shortcomings are outweighed by the fact that the term does not prescribe a certain methodology.

So in this study, creative methods entail social scientific qualitative methods which encompass practices and approaches from the arts, specifically from visual lens-based arts, poetry, performance and narrative. There are other creative methods, such as quilt-making (Ball, 2008; Rippin, 2003), dancing (Blumenfeld-Jones, 1995; 2008) and working with Lego (Gauntlett, 2007), but these have not been as widely used as visual lens-based arts, poetry,

performance and narrative, as my earlier literature study showed (van Romondt Vis, 2007). In the following two sections, I explain what I understand those methods to be in the context of this research. I do so by, first, looking at some of the roots from which these methods developed and, second, outlining the visual lens-based, poetry, performance and narrative methods.

The Rise of Creative Methods

Researchers started to look for other ways of doing and disseminating research partly as a response to the crisis of representation (Schwandt, 1997, p. 21) and the crisis of legitimation (Denzin & Lincoln, 2003, pp. 28-29). The crisis of representation concerned power relations between the researcher and participants within the research process. This relationship presented the researcher with various problems, such as the influence she had in producing the research both in the field and in writing-up (Denzin & Lincoln, 2003, pp. 25-29; Schwandt, 1997, p. 21). In writing up the research, the researcher constructed a version of the social world she had studied, which was presented as an objective depiction. Certain rhetorical strategies, such as the use of a passive voice, helped the researcher create this objective, scientific and authoritative text. This process was now called into question and led to what Gergen and Gergen (2012, pp. 23-29) called the loss of privileged language and the realisation that social scientists did not objectively observe the world, but rather constructed it. “[T]he products of ethnographic research [could thus no longer] be legitimately perceived as in any way *representing* the separate reality of another society” (Davies, 2008, p. 15, italics added).

If researchers could “no longer directly capture lived experience” (Denzin & Lincoln, 2003, p. 28) and the written-up research was a construction produced by the researcher (Clifford & Marcus, 1986; Van Maanen, 2011), the next issue concerned how to evaluate research if there was no longer an objective reality to measure it by. Which criteria could be used to judge research with, if validity, generalisability and reliability were no longer considered relevant (ibid.)? This became known as the crisis of legitimation (ibid.).

Creative methods offered ways to cope with these crises. Their use in disseminating research such as poetry, performance and narrative in a postmodern way was one way to draw attention to the constructedness of the research (Denzin & Lincoln, 2003, p. 3) because such texts deliberately and critically called into question their own authority (Davies, 2008, p. 16) and the difference between fact and fiction (Kara, 2015, p. 126). Dramatic performances and

poetry, for example, were unequivocally constructions conveying the experience of the participant (e.g. Glesne, 1997, p. 214; Goldstein, 2001; Gray, 2000; Jones, 2002; Mienczakowski, 1995; Paget, 1990; Pifer, 1999). Or, narrative texts used a variety of literary techniques (Denzin & Lincoln, 2003, p. 3; O'Reilly, 2005, p. 217), such as applying different writing styles to emphasise that no “single authority, method, or paradigm” was favoured (Denzin & Lincoln, 2003, p. 39). Examples of such postmodern texts are *a Thrice-Told Tale* (Wolf, 1992), *Troubling the Angels* (Lather & Smithies, 1997) and *Nine Poems* (Richardson, 1994). Deconstructing authority was not always a reason to use creative methods. Some used them because such texts helped convey “other truths and different perspectives that were not available in the non-fiction realm (Vickers, 2010, p. 563). Gergen and Gergen (2012, pp. 15-19) claim that performative social sciences provided a more vivid and emotional representation of how social life can be, which potentially bridges the distance between the written-up research and the messiness of daily life (p. 17).

Another particular concern in the crisis of representation was the relationship of the researcher with the participants during and after the research. Researchers felt that participants should get a more prominent role in the research process in order to counterbalance the power of the researcher. Some researchers even went further and said that research needed to be useful or emancipatory for the participants in some way³ (Fals-Borda, 2005; Freire, 1970; Maguire, 1987; Prosser, 2008; Reason & Bradbury, 2006). In an effort to achieve this, more participatory and creative methods were employed. Some of the arguments for participants using visual and performative methods are questionable. For example, they are said to address the participants more on their own terms (e.g. López, Eng, Randall-David & Robinson, 2005; McIntyre, 2003, p. 48). Or they are believed to provide participants with a more authentic voice because creative methods are better at drawing out what participants actually feel (Brodie, 2004; Gray, 2003; Jones, 2002; Pifer, 1999). Nevertheless, they can be more fun (Darbyshire, MacDougall & Schiller, 2005; Einarsdottir, 2005, p. 527; Riley & Manias, 2004, p. 401), and they allow the participants to express themselves differently (López et al., 2005; McIntyre, 2003; Riley & Manias, 2004). This can lead to other, more accessible forms of research disseminations for non-academics and, as such, reach a wider audience (Gray, 2000; Leavy, 2013). Instead of having to read a complex research article (Richardson, 1998), which

³The power relation between researchers and their participants is obviously much more complex than this simple juxtaposition, and has been written about extensively (e.g. Ballamingie & Johnson, 2011; Martínez Guillem, 2013; Vähäsantanen & Saarinen, 2013; van Romondt Vis, 2010).

is often not freely accessible,⁴ the audience can engage with a poem, photo exhibition or performance, allowing for other connections with audiences than traditional lecturing affords (Douglas, 2012, p. 530; Gergen & Gergen, 2012, p. 161). Additionally, such formats have the potential to enlarge the impact of the research, which is increasingly important in the race for research funding (Colley, 2014; Hammersley, 2011, p. 10; LSE Public Policy Group, 2011; RCUK, 2014).

The use of creative methods and the materials that are produced with it – e.g. photographs, film, poetry and performance – also showed that creative methods could convey other ways of knowing, such as sensory, embodied and emotional (e.g. Brodie, 2004; Conquergood, 1991; Goldstein, 2001). Embodied activities, such as sports, are portrayed well through methods that can convey a sensory experience (Owton, 2013; Sparkes, 2009a). Researchers claim that poetry, performance, narrative and visual methods are all ways to *show* rather than *tell* about the lives, experiences, feelings and emotions of participants (Kara, 2013; Owton, 2013, p. 602) and for which language might not be the best option (Gergen & Gergen, 2012, pp. 25-26). Other forms of dissemination can make us understand things in ways we did not ‘see’ before (pp. 27-29). As such, they are seen as ways to approach the social world differently and as possibly providing answers to complex and demanding issues (Darbyshire, MacDougall & Schiller, 2005; McIntyre, 2003; Pifer, 1999).

Sparkes (2009a) cautions though that such methods should not be seen to replace more traditional written texts, but rather that they provide a different and additional understanding of the research topic at hand. Douglas and Carless (2010) argue that the choice for a particular method involves a choice for a particular kind of knowledge, where the realist tale and ethnographic fiction convey different meanings derived from the research. The researcher needs to ask herself how the research is best conveyed (Saldaña, 1999, p. 61), whereby the aims of the research and its dissemination are leading in the choice between methods.

In conclusion, then, creative methods attempt to achieve two main things. One is to try to acknowledge the power relations in the various aspects of the research process: the relationship with the participants, the authoritative voice in the dissemination and the exclusion of a non-academic audience. The other is the acknowledgement of different kinds of knowledge, such as embodied or sensory, and being able to convey them through other means of disseminating the research, such as photography or poetry. These two goals provide the entry point to the next section, whereby I discuss the creative methods, which are the

⁴ In recent years, however, British and Dutch universities have made considerable steps towards an open access system.

focus of this research.

Creative Methods in this Research

My aim in this thesis is to look at how creative methods are negotiated into social science research practice. In this section I offer *my* understanding of creative methods at the time of the study, specifically visual lens-based arts, poetry, performance and narrative. It is not my intention to give a complete history or overview of all the methods, but rather to discuss a couple examples to help the reader understand how I perceived creative methods and concomitantly the way my participants took up these methods in practice and, thus, how creative methods were emerging in that process. I structure this section similarly to the conclusion of the previous section, discussing the different methods according to how they acknowledge the power relations in the research process and the different kinds of knowledge produced. Furthermore, I look at the different methods separately. However, this separation is sometimes hard to maintain because some of the methods' roots are similar (see, for instance Smith, 2007, p. 394). As such, researchers do combine them, as seen, for example, in the coupling of poetic inquiry with visual methods (Owton, 2013), visual methods with narrative (Harper, 2006) and poems with narrative (Owton, 2012; Sparkes, 2012).

Visual Lens-based Methods

In recent decades, the field of visual methods has grown tremendously (Pauwels, 2011, p. 3; Pink, 2012, p. 3; Rose & Tolia-Kelly, 2012, p. 1) and it encompasses many more methods than the creative methods I look at. The ones I focus on in this thesis are those where either the researcher or the participants produce visual materials specifically for the research. That excludes the methods where pre-existing images are analysed, such as films and television. I also exclude methods of analysis like semiotics, iconography and discourse analysis (Rose, 2012; van Leeuwen & Jewitt, 2001). Of course my participants sometimes made use of these methods to analyse the visuals that they or the participants produced, but they are not my focus. Another restriction is that I look at lens-based visual methods, but there are other creative visual methods, such as creating comics (Sousanis, 2015; Stephens Griffin, 2014) and childrens' drawing as well (Bak & Piko, 2007; Diem-Wille, 2001; Rawlins, 2006).

One way visual methods acknowledge the power relations in the research process is through handing over the photo or video camera to the participants. Photography and film have for a long time been used in anthropology (Bateson & Mead, 1942), though one of the

first researchers to give the video camera to participants were probably Worth and Adair in 1966. They taught their Navajo informants the basics of operating a video camera and asked them to make a film about whatever was important to them (Aitken & Wingate, 1993, p. 66; Holm, 2008, p. 329; Pauwels, 2001, p. 8). Since then, many researchers have followed suit, especially when technological developments made the use of photography and video easier and cheaper, both in fieldwork and in dissemination of the research (Erickson, 2011; Jewitt, 2012, p. 2). This collaborative way of working has been given different names, such as “native image production”, “cultural self-portrayal” and “respondent-generated imagery” (Pauwels, 2011, p. 8). The photographs or videos produced are obviously still researcher-initiated, and it is sometimes questionable how much power the participant actually has in this process. Some research done under the heading of ‘photovoice’ shows that this method does not seem to subvert the power relations, but rather provide the participants with more work.

There are projects, though, where it is apparent that the strict division between researcher and participants is no longer tenable. A case in point, *Voices and Images* is a project in which the Association of Maya Ixil Women – New Dawn in Chajul, Guatemala collaborated with Lykes, an academic from Boston College (Lykes, Beristain & Pérez-Armiñan, 2007). The Mayan women initiated the project, and were active contributors throughout the whole research process. Because they did not all speak the same language, they chose to use a visual method to partly work around this issue (Lykes, 2006, p. 271) and to overcome language barriers. The power relations were not only acknowledged in the production of the data, but also in the dissemination of the work. One of the journal articles, for instance, was co-authored by the participants (Lykes, Mateo, Anay, Caba, Ruiz & Williams, 1999), and the participants also helped produce a book (Women of PhotoVoice/ADMI & Lykes, 2000) and a website⁵. These forms of dissemination were not only achieved through collaboration between the academic and the participants, but were also ways to reach audiences other than academic.

Just one other example of the many researchers using visual methods in collaboration with their participants are Haaken and O’Neill (2013; 2014). Haaken and O’Neill (2014, p. 80) worked together with ten women asylum seekers using photography, film and drawing to investigate how “this group of women interprets community safety and threats to well-being in the context of their experiences of exile, displacement and transition.” They particularly looked at how this manifested itself in the everyday experiences. The women asylum seekers

⁵ See www2.bc.edu/brinton-lykes/voices.htm

produced a map of places they felt at home at and places they did not. The researchers and the women used the map to walk through the town following this map, taking photographs of these places. A volunteer at the refugee centre had joined the group and filmed parts of the walk. This project led to an academic publication (2014), but also to an exhibition and a video (2013). In the video the viewer can only partly see the women, since they wanted to remain anonymous, because of their precarious situation. Filming the women from behind, ensures their anonymity, but also helps to convey a sense of distance, because the viewer cannot make eye contact. Additionally, it gives the viewer the sense that she is following the women, as she is constantly being behind them. This creates a specific sensory experience, conveying other kinds of knowledge about these women's lives.

Both these projects aim “to reduce the gap between the concepts and models of researchers and those of individuals and communities by giving participants control of the camera and the process of making their experiences visible” (Jewitt, 2012, p. 3). This enables the participants to create their own meanings. These projects also clearly try to break down power relations through addressing non-academic audiences as well. In those disseminations is also more room for different kinds of knowledge such as the embodied and sensory, as the viewer is able to see – sometimes only partly because of anonymity – and hear the participants, places, stories. Furthermore, the data in these projects often entail not only the photographs or video, but also the process of production and thus takes a broader understanding of the data produced (ibid.). In this understanding the photographs or video is problematized. Prosser (2006, p. 2), for instance, says that

“A photograph does not show how things look. It is an image produced by a mechanical device, at a very specific moment, in a particular context by a person working within a set of personal parameters”.

This means that the visual is not self-evident, but a construction in which “culture, personal biography, positionality, politics, aesthetics and so on” come together (Prosser & Loxley, 2008, p. 31). In such an approach the visual is not seen as “ancillary [and] illustrative [but] rather (...) constitutive of anthropological knowledge” (Taylor, 1996, p. 66).

Poetry

Many consider Laurel Richardson (1992; 1994) to be one of the first to use poetry in the

social sciences (Görlich, 2015). Richardson used poetry, but also other forms, such as drama and fiction, to which I come back in the *Narrative* section below, to explore the limits of anthropological writing and to question those limits. Since then it has been steadily taken up under a variety of names. Prendergast (2009, pp. xx-xxi) collected a list of forty different names for research that used some form of poetry in social science dissemination. She also found that there were three main categories into which this work could be divided: literature-voiced poems, researcher-voiced poems and participant-voiced poems (p. xxii). The first refers to poetry “written from or in response to works of literature/theory” (ibid.). According to Prendergast, this type of poetry is produced the least and personally I have only come across one example by Prendergast (2006) herself, so I will not discuss this type further. The second category is poetry based on “field notes, journal entries, or reflective/creative/autobiographical/autoethnographical writing as the data source” (ibid.). The final one refers to poetry based on the participant’s words “interview transcripts or solicited directly from participants, sometimes in an action research model where the poems are co-created with the researcher” (ibid.). Both of these forms are ways to acknowledge power relations and to convey different kinds of knowledge (Richardson, 1994).

One very important reason poetry is used as a form of dissemination by researchers is to do justice to experiences of the participants. Researchers often mention that the transcripts of a particular participant compelled them to write poetry, because their stories were in a way already poems through their rich language, word use and rhythm (Hones, 1998; Kara, 2015, p. 131; MacNeil, 2000; Sparkes & Douglas, 2007, p. 174). Written prose, especially in a formal academic style is considered inappropriate to convey all the nuances and emotions the participants portrayed in either interviews, field observations. (Kara 2015, p. 121-136).

In addition, researchers felt that the topics demanded a more sensitive treatment, such as bereavement (Clark, 2014; Owton, 2011; 2012; Terry, 2012), mental or physical illness (Clarke, Febraro, Hatzipantelis, & Nelson, 2005; Kendall & Murray, 2005; Naidu, 2011; Poindexter, 2002; Souter, 2005) or issues concerning racial and gender identity (Drummond, 2011). Poetry is a way to rebel against the stifling rules and practice ways of writing that allow for the complexity, emotional, embodied experiences of both participants and researchers.

Another way in which the power relations are acknowledged is the form of poetry. The words and layout are used in such a way that they show the reader that this is a construct. Research poetry is sometimes embedded in an article in which the author discusses the research and explains how the poems were constructed from the data (Glesne, 1997; Görlich,

2015). Glesne (1997), for instance, is distinct in that she prioritises the participant's words, defining poetic representation "as the creation of poemlike compositions from the words of interviewees" (p. 202) and indicates that "[t]he process [of poetic transcription] involves word reduction while illuminating the wholeness and interconnectedness of thoughts" (p. 206). She (1997, pp. 205-206) explains how she first started with coding her research into general themes. After she had generated those general themes, the process diverged from her normal writing up practice where she would continue "coding, categorizing, and ordering of large data clumps." (p. 206). Instead, she would re-read all the words from her participant she had themed together, and reflected on what the essence of this theme was. When she started writing, she only used the words from her participant and rather than trying to convey a linear story, she tried to re-present an essence of her participant through different and contradictory aspects of the interviews. (ibid.)

In contrast, there are also standalone poems (Owton, 2011), which offer no information on the content in which the poetry was constructed. In between are those poems that are accompanied by a short abstract, which provides some contextual information about the research on which the poem was based (Owton, 2012). Sometimes this information is put in the poem itself. Faulkner (2005), in a poem aptly called *The Study*, describes where she did her research, who her participants were and what kind of method she used for gathering and analysis.

The Study

I. Design and Methods.

Narratives show you thoughts-in-context¹
like cartoon bubbles or atavistic gestures flung at crazy cars,

they tell you more than enough about someone,
identity wrapped up in everyday conversations²

the ideal and real, self and society,³ seamless
saran wrapped produce packaged for others,

quotidian stories revel in thoughts-in-context
tell you a lot about someone just before last call.

Procedures.

Went to the right places—
Pennsylvania, New York and New Jersey,
gay list serves and Jewish lectures, local communities,
urban and college towns, homes, diners, places of comfort—

with the right equipment,
asked if they had friends,⁴
 who would tell different stories
 who cry and rage and laugh like that,
with the right attitude.⁵

Interview Guide.

Yes.
Focused,
yet flexible.⁶

Participants.

There were thirty-
one, aged 19 to 73,
with mode = 37,
median = 44,
and mean (*M*) = 42.7.

Image 1

The words Faulkner used, how she combined them and laid them out on the page are a good example of how poetry emphasises the constructed nature of the research. This form of poetry in the poem by Faulkner is called free verse, which according to Kara (2015, p. 131), is the most common form, but other traditional poetic forms, such as, haiku and elegy (Lahman, Rodriguez, Richard, Geist, Schendel & Graglia, 2011; Prendergast, Gouzouasis, Leggo & Irwin, 2009) are also used.

The choice of words and how to structure them is also a way to convey other forms of knowledge. By using repetition and rhythm certain meanings are emphasised (Kara, 2015, p. 131). To repeat the word panic three times, maybe even using capital letters and an exclamation mark, evokes a sense of panic much better than saying, ‘she felt a sense of panic’. This makes poetry capable of “[capturing] the subjective and affective aspects of human life” (Görlich, 2015, p. 522) as well as the emotional one (p. 524).

Sparkes and Douglas (2007, p. 175) explain how Douglas first repeatedly listened to the interview recordings to look for the emphases and accents her participant placed. They continue:

Gradually, as she listened to the tapes alongside the transcript, Kitrina [Douglas] sensed how the phrases and lines fell into grouped stanzas, or verses, that focused on important themes and events in Leanne’s life. Furthermore, similar to Poindexter (2002), Kitrina searched for unambiguous phrases, strong statements, eloquent expressions, wording that appealed to her, and portions of the narrative that she felt strongly captured Leanne as she had come to know her through the study. This was

particularly so when these phrases expressed the feelings and emotions of certain moments in her life and golfing career. Particular attention was given to Leanne's repetitions and how these formed key themes in her story. (ibid.)

Conveying other knowledge, thus already starts in the analysis phase, because creating poetry from the interviews demands the reader approaches the material differently. This subsequently becomes manifest in the poem. Rather than examining the interviews for themes Douglas looked for unambiguous phrases, strong statements, eloquent expressions to use in order to capture who Leanne was to Douglas. Conveying this other kind of knowledge also enables the researchers to do justice to the voices of research participants.

Performance

Performative methods include research that is disseminated through theatre, but of course the recital of poetry at conferences or the performance of songs also fall under this heading. Because I have already addressed poetry and I have not come across the performance of songs very often, I focus mainly on theatre-based methods here for clarity.

The development of performance in the social sciences has been greatly influenced by the collaboration between Turner, an anthropologist and Schechner, a theatre director (Jones, 2006, p. 339). This collaboration resulted in Turner's book *From Ritual to Theatre* (1982), which focussed on how performance, learning and the body connected in acquiring and conveying knowledge. Another influence was Boal's book (1970) *Theatre of the Oppressed*. In this book he developed a specific way of using theatre in which communities were invited to reflect on issues that *they* found worthwhile and worthy of showing other people rather than elites deciding what stories needed to be told, which were then performed by those same elites. Performance is thus unmistakably rooted in acknowledging the power relations and the different kinds of knowledge.

How a performance acknowledges those depends on the form of the performance. Beck, Belliveau, Lea and Wager (2011) created a spectrum of research-based theatre, offering a way to discuss the various types of work that fall under the header of performance. On the one hand there is the research continuum which focuses on the kind of material that is used as a basis for the performance (pp. 690-691). This ranges from the fieldnotes and interviews with participants to second hand sources such as newspaper articles and biographies. On the other hand there is the performance continuum, which focuses on the intent and audience of the

performance (pp. 691-695). Here, the question is which form best tells the story of the participants to this particular audience (Saldaña, 1999, p. 61)? An audience of peers at an academic conference will expect something else than a general audience, where the former might be more interested in aspects like transparency of analysis and the latter in an aesthetically pleasing performance (Beck et al. 2011, p. 691). Of course, researchers can decide to go against these expectations. Depending on the type of research one has done and the aim of the performance, there are different ways in which the research can be performed.

Donmoyer and Yennie-Donmoyer (1998, p. 398), for instance, use reader's theatre⁶, which they define as:

[A] staged presentation of a piece of data or selected pieces of data which are thematically linked. (...) Staging is simple. The performers hold scripts, and any "acting out" of a piece is limited and highly stylized. (...) [S]tylization and certain conventions are used for the express purpose of deemphasizing realism”.

For instance, in discussing the literature on a specific topic, each author is represented by a different person on stage. Each person will deliver the lines of his or her author. This creates a performed dialogue between the different authors showing how it constructed from different voices.

Saldaña (2003, p. 218) discusses another form.

Ethnotheatre employs traditional craft and artistic techniques of formal theatre production to mount a live performance event of research participants' experiences and/or researchers' interpretations of data for an audience. An ethnodrama, the script, consists of analyzed and dramatized significant selections from interview transcripts, field notes, journal entries, or other written artifacts. Characters in ethnodrama are generally the research participants portrayed by actors, but the actual researchers and participants themselves may be cast members.

This type of performance is much more elaborate and requires more skills to actually stage it. As such, the conversion of those research materials into an ethnodrama needs to be a collaboration between social scientists and playwrights (Gray, 2003; Gray et al. 2000) or

⁶This term can be written as reader's theatre as well as readers theatre.

someone with a background in theatre and social sciences as well (Goldstein 2001; 2002; Saldaña 2003). Moreover, the production can be a collaboration with the participants as well (Gray, 2003; Gray et al. 2000). Unlike the reader's theatre, the ethnotheatre can *show* how "participants facially react, walk, gesture, pose, dress, vocally inflect, and interact with others. These non-verbal cues reveal much about characters – and real people" (Saldaña, 1999, p. 67). It can counterbalance the research disseminations that are devoid of emotions and "integrate mind and body" (Hesse-Biber & Leavy, 2006, pp. xxi-xxiv). As Sangha Slade, Mirchandani, Maitra and Shan (2012, p. 287) state "We hoped to convey some of the passion, emotion, and tension that emerged during the interviews as women often gave us, at times, shocking accounts of their working conditions". As such, the performance is also interesting for non-academic audiences. Gray (2000) and Mienczakowski (1995) have actually toured with their research based performances in local community centres or regional care centres. Their audiences consisted of general practitioners, health professionals, service users, high school students and the general public.

What is noteworthy is that performances can also acknowledge the power relations and other kinds of knowledge through articles in research journals. Most of the articles referenced in this section give at least short excerpts of spoken words in the performance accompanied by stage directions. The following is an example from the performance written by Sangha et al. (2012, p. 288). Their research was on the lived experiences of precarious workers:

Scene 1: Falsity in a Call Center

Characters: Namisha, Paul (Manager).

Setting: The cubicle of an employee at an outbound call center.

Scene: Namisha is worried. She is having trouble meeting the quota for the day.

Namisha: [Looking at her watch] Only two more hours! If I don't meet the quota I will be sent home soon!

[She picks up her headset and starts making calls.]

Namisha: Hello?

Respondent 1: Hello?

Namisha: [Sounding very enthusiastic] Hi my name is Namisha, and I'd like to talk to you about. . .

[Caller hangs up. She makes another call.]

This is clearly a technical way of writing down the scene, thereby showing that this is construction. Nonetheless, the reader understands how to read it and can envision a cubicle at an outbound call centre with Namisha in it. She can see her being worried and starting to make the calls and, thus, making a more sensory and emotional connection with the reader.

Narrative

The methods that I call narrative in this thesis visibly originated in the crisis of representation. As already mentioned in the section *The Rise of Creative Methods*, such text use various literary techniques (Denzin & Lincoln, 2003, p. 3; O'Reilly, 2005, p. 217) to acknowledge the power relations and other forms of knowledge.

One way in which narrative acknowledges power relations is by showing that the academic text is a construction through using non-academic styles of writing. Wolf (1992), Richardson (1997; 2006) and Lather and Smithies (1997) even use multiple writing styles. In *a Thrice-Told Tale* Wolf (1992) wrote three texts on the basis of the same research in Taiwan. The first was a fictional story, the second consisted Wolf's field notes and the final was an academic journal article. Each text was followed by her own comments on those texts. The combination of the texts showed there were different perspectives possible within one research project. The alternation between styles reminds the reader that this text is not naturally so. It is crafted by someone. In addition, each text conveyed a different kind of knowledge. Likewise, Richardson (1997; 2006) wrote, what she calls, a pleated text in which autobiographic, dramatic, poetic and narrative alternated with sociological styles of writing. Finally, Lather and Smithies (1997) not only used different writing styles, but also mix them on the same page, using the layout as a means to show how the text is a construction.

Writing in a non-academic style is also a way to deal with the power relation between researcher and participant, most notably the issue of exploiting the participants' stories (Wolf, 1992, p. 2), doing research *on* others. Leavy (2011; 2012) and Scarlett (2014) used fiction to tell the stories of women they had heard over the years, mostly in non-research settings. This made it problematic for them to write about specific people. Auto-ethnographic writing evidently deals with this by using oneself as a subject rather than others. As Richardson (2006, p. 4) states: "I did unto myself as I had unto others". Additionally, auto-ethnographic writing troubles what is considered as an appropriate and most notably objective source of knowledge.

These other writing styles are also a way to engage the audience – academic or other – because as Cully (2008) says qualitative research reports are often boring. Leavy (2012, p. 519) argues that “traditional academic writing” was not the best option to reach and relate to an audience of young women – her target audience.

Narrative can also be used to convey other kinds of knowledge, such as, embodied, sensory or emotional. Sparkes (2009a, p. 21), for instance, clearly shows how the vignettes he wrote convey sensory knowledge:

It's busy in the gym tonight. I can tell long before entering the main room. The sound of the RAP music assaults my ears as I climb the stairs. Loud, fast, aggressive, pumping – *thump- thump-thump-thump*. Some heavy weights hit the rubber floor. I feel the vibrations in the soles of my feet on the stairs. A metal bar slams into the squat rack emitting a piercing jangle – *sshpaanngg!* Sounds like some of the ‘big boys’ are in there.

In this specific excerpt the reader is alerted to the sounds and touch of the experience of going into the gym, but in the remainder of the vignette Sparkes refers to the other senses as well. The reader can almost see, feel and hear the gym herself. This is what Cully (2008, p. 427) calls a dramatic or scenic method, which *shows* the reader what happened. This is in contrast to the summary methods, which *tells* the reader what happened. The dramatic way of writing addresses the reader more directly and is thus able to appeal to the reader's senses and emotions. As such, it is also often used, like poetry, to convey emotional knowledge (Wyatt, 2012).

Conclusion

It has not been my intention to give an complete overview of all the research done with creative methods. That would be a thesis in itself. Rather, I have discussed examples of research to give the reader a sense of how *I* understand creative methods. With this discussion, I have shown how visual methods, poetry, performance and narrative methods acknowledge the power structures in the research and how they convey different kinds of knowledge. Inevitably this means they have to go against standard practices and they use creative methods “influenced by the arts broadly conceived” (Cole & Knowles, 2008, p. 59) The examples that I have discussed very explicitly use creative methods and as such give a

good idea of what I consider creative methods to be. There are, however, many more research projects that use elements of these methods in more subtle ways. There is no hard distinction between them, but rather a continuum on which the use of these methods can be placed.

Moreover, not all researchers who use creative methods, do so because they want to acknowledge the power relations between them and their participants, or because they want to convey different kinds of knowledge. The use of creative methods has also, quite simply, become popular as my participants told me. How these methods are taken up in practice, depends on the background of the particular researcher and the context in which he or she does her research, the so-called community of practice, which I explain in Chapter Two's *Communities of Practice* section. Nonetheless, the examples in this section give a reference point of how I understood the participants' negotiations in taking up these methods.

Are Creative Methods Innovative Methods?

There has been a lot of discussion about and research on innovation in research methods (Travers, 2009; Wiles, Pain & Crow, 2010). Although 'innovation' and 'innovative methods' are problematic terms, which I elaborate on in Chapter Two's *Academic Practices and Innovation* section, creative methods are often categorised as such (Pain, 2009; Taylor & Coffey, 2008; Wiles, Pain & Crow, 2010; Xenitidou & Gilbert, 2009, pp. 60-65), which is why the discussion on innovative methods is relevant to my work. In the research on innovative methods we come across questions that ask where we see innovation within social scientific research methods; what innovation within research methods is; whether these creative methods are innovative; and what kind of innovations we need within social science research methods. The problem with questions like these is that they produce debate over what innovation is and whether or not creative methods deserve to be called innovative. And all of this assumes, in the first place, that it is clear what those methods are! Another problem with these questions is that they understand methods as static, which they are not. This misunderstanding is exactly the problem in answering those questions. Furthermore, as I elaborate in Chapter Two, an important goal within academia, more broadly, is for research to always produce *new* knowledge, meaning that 'innovation' is expected to be perpetual.

In contrast to the studies on innovative methods referenced above, I am looking at methods in development, as constantly emerging (Ingold, 2010a; 2010b; Ingold & Hallam, 2007; Wenger, 1998). This provides a better understanding of how people take up creative methods and how they improvise while integrating such methods in their own research practice (Ingold & Hallam, 2007). By looking at this learning process as constantly

developing but at the same time embedded within a specific context, I develop a more sophisticated understanding of such processes. In addition, my research helps to examine the concepts of innovation and interdisciplinarity and question their relevance as drivers for research. So rather than aiming to work in innovative or interdisciplinary ways, the goal should be to research the topic well. This means the research question should drive the research. That drive could potentially lead a researcher to take innovative or interdisciplinary approaches to the topic, but they are means, not goals in themselves.

Structure of the Thesis

In this introduction, I outlined the topic of my thesis and presented my research concerns: namely, creative methods and their development through negotiations in the research practices of social scientists. Further, I argued that it is relevant to research them because there has been little attention to the way these methods have been taken up in practice, the challenges and opportunities taking up these methods poses for researchers – both novice and experienced – and what doing so means for learning methods, the production of knowledge. Finally, I gave an outline of my understanding of these methods.

In Chapter Two: *Practice, Improvisation and Innovation*, I develop the theoretical framework of the thesis. I first discuss elements of Lave and Wenger's theory of 'communities of practice' (Lave & Wenger, 1991; Wenger, 1998) to understand how researchers learn and engage with specific research practices. Both change and continuation are important elements of Lave and Wenger's theory, and I develop these elements further with Ingold and Hallam's (2007) concept of improvisation and Ingold's (e.g. 2010a; 2010b) elaboration of a constantly emerging and unfolding reality. Communities of practice and practices themselves are thus ever changing. Then I move on to critique the lack of accounting for power within Lave and Wenger's theory of 'communities of practice' (Lave & Wenger, 1991; Wenger, 1998). Power is always and inevitably present in negotiations and, in discussing the concept of boundary-work (Gieryn, 1983; 1995; 1999), I explain further how power plays a role in the way academic practices evolve. This is followed by a discussion of who is able to perform boundary-work and thus to exercise power, for which I use Bourdieu's (1975) discussion of symbolic power in the scientific field as a starting point. Innovation is the next main element I discuss in the theoretical framework. I end with discussing the assumptions within qualitative social sciences of how research is 'supposed to be done'. This theoretical framework leads me to argue that the change in practices happens in small shifts. Change is incremental rather than revolutionary. This means that a concept such as innovation

is not useful and even incorrect in relation to academic practices, in general, and the creative methods my participants use, in particular. Of course there are times when developments ensue very quickly one after the other, but even then, connections are always made to earlier practices. More importantly, it means slow but profound shifts in epistemologies, methods and knowledge production can be overlooked if everyday practices are neglected in research focussing on change in academic research practices.

In the Chapter Three: *Methods*, I deal with my methodological approach and the role of reflexivity. I use the methodology-as-autobiography approach (Hammersley, 2011, pp. 25-30) in which *how* the method was applied in this specific study by this particular researcher contextualises *what* was done. In this approach, reflexivity plays an important role. The chapter therefore starts with an extensive discussion of this concept. Reflexivity has become part of good research practice (Lumsden, 2013, p. 4), but it can be unclear what is meant by it (O'Reilly, 2012, pp. 521-522). I therefore look at different conceptualisations of reflexivity over time and conclude that I understand reflexivity to be relational and on-going. Such an understanding of reflexivity is in line with the overall argument of the thesis: practices are always connected to earlier practices and they emerge in an on-going relation to other research. This treatment of reflexivity is then put into practice by giving an account of my own academic background, thereby giving the reader an understanding of how my perception of creative methods developed and how it was grounded in my personal education. This will help the reader understand how I interpreted my participants' research practices. This is then followed by a discussion of how I conducted my research in terms of the general approach and, more specifically, the construction of the field, my participants, the methods of data-gathering, transcription and analysis. The final part of this chapter consists of the on-going research ethics in the project, which deals with issues such as anonymisation, the relationship with my participants and the co-construction of knowledge in this thesis.

The following three chapters present the findings of my research. In Chapter Four: *Making, Breaking and Negotiating Boundaries*, I discuss how my participants negotiated their use of creative methods with others. I use the concept of boundary-work (Gieryn, 1983) to describe and analyse four examples in which the use of creative methods was contested, showing the diversity of ways in which boundary-work was performed and who could perform it; the way power relations developed; and how this shaped the way the creative methods emerged. These examples highlight that only little shifts could be made as creative methods were always connected to 'traditional' practices.

Chapter Five: *Creative Methods and Negotiating Personal Practices* describes and

analyses how my participants negotiated creative methods into their own personal practices. Although my participants always had to negotiate their practices with each new research project, it was clearer in those moments when they perceived their theoretical knowledge or established practices as being inappropriate to the situation (Roe & Greenhough, 2014, p. 51; Wenger, 1998, p. 53). The focus of this chapter is on those moments, when practices become visible. The moment when practices became visible showed the discrepancy between my participants' theoretical and methodological expectations and the methods in practice. This discrepancy required my participants to negotiate their practices in order to accommodate the creative methods and simultaneously adjust the creative methods to fit into their practices. It also showed the assumptions – the so-called reifications – on which my participants' practices were based. Again, this highlights how only little shifts were possible in changing practices. In contrast to my findings in Chapter Four, the changes in practices were not limited by others through boundary-work, but rather by my participants' own ideas about what research entailed.

In Chapter Six: *Creative Methods and Presentations*, I look more closely at the way the postgraduate research students presented their research. I start by discussing the limited literature on conference presentations, from which I distilled a standard structure for a presentation and the rhetorical strategies that are commonly used in introductions. Then I describe my postgraduate research participants' presentations and look at how they presented the creative methods they used in their research. The content, design and wording of the presentation, along with the setup of the room and the context of the conference (e.g. call for papers, organisation, delegates), and compare all of that to the standard structure I outlined at the beginning of the chapter. As I found, three of the five presentations clearly followed this structure, and the creative methods were embedded just as other qualitative methods would be. Furthermore, the presentations contained many references to academia. The other two presentations differed more from the standard structure, but they, too, contained elements of that structure and used elements from other academic sources in their presentations. Nonetheless, all of them pushed the boundaries in academic presentations. Another interesting point was how my participants dealt with anonymity, reflecting the discussion on anonymity in the social sciences, in general, and visual methods, in particular. Whereas mainstream social sciences tends to work with a principle-based ethics in which all participants need to be protected and thus anonymised, creative methods have helped open up this discussion through the way they gather and disseminate their data. Anonymity is not a given, but part of an on-going reflection on the research process.

Finally, in Chapter Seven: *Conclusion*, I remind the reader of the research questions and present summaries of each chapter, linking them to the research questions. I then answer these questions, compelling me to conclude that creative methods are used, appropriated and developed within a context of communities of practice. Within these communities of practice, there are certain ways of doing things – which Wenger (1998, pp. 57-62) calls reifications – that determine how methods can be understood, applied and developed.⁷ My findings allow me to argue that profound changes in research practices are not possible in a short timespan. Further, I argue that the emergence of creative methods and the negotiations my participants made were always relational: to other practices, to earlier research, to other researchers. This meant that the practices of the participants, the negotiations they engaged in and the emerging of the creative methods were better explained through improvisations than innovations. Such an understanding of how research methods and practices develop has implications for the teaching and learning of methods and interdisciplinary research. If researchers always improvise from and relate to earlier practices, then the way they engage with and learn methods is always influenced by those earlier practices. This also applies to interdisciplinary research, where the engagement with the other method is always done from one's own practices. In order to successfully conduct interdisciplinary research it is thus crucial to realise what the different practices of researchers are. Finally, this shows that the call for innovation in methods, understood as a break from previous practices, is thus wholly infeasible and inappropriate.

⁷ I use 'determine' here, as I do throughout the thesis, in the sense that Williams (1980, p. 32) has discussed it – namely, “as setting limits, exerting pressures”, but not as “an external cause which totally predicts or prefigures, indeed totally controls a subsequent activity”.

Chapter Two: Practice, Improvisation and Innovation

Introduction

This chapter outlines the theoretical framework through which I understand how creative methods are taken up by social scientists in their methodological practices. I start with a discussion of practices, addressing questions such as: How do social scientists learn specific research practices, and how do they engage with them? Here I draw on elements of what is known as ‘practice theory’, most notably from the work of Lave and Wenger (1991; Lave, 1991; Wenger, 1998) on ‘communities of practice’. To understand how change and stability in these practices are so possible, I draw on the concept of improvisation (Ingold & Hallam, 2007) and Ingold’s (e.g. 2010a; 2010b) elaboration of the constantly emerging and unfolding reality that shapes communities of practice. Additionally, I point out that the concept of power is not sufficiently elaborated in Lave and Wenger’s theory of communities of practice. I then move on to boundary-work (Gieryn, 1983; 1995; 1999), a concept that addresses the discursive practice of accepting or rejecting research in the academic community, which helps explain how power plays a role in the way academic practices evolve. This is followed by a discussion that concerns who wields power and how. For this, Bourdieu’s (1975) concept of symbolic power in the scientific field offers a starting point. Finally, I discuss the concept of innovation, which is often applied to the creative methods my participants use. Innovation entails a break with the past, leaving creative methods to be seen as radically different from earlier methods. By contrast, I argue that change in practices (including those that are methodological) happens in small shifts; change is incremental rather than revolutionary. As such, I argue that it is inappropriate to use words such as ‘innovative’ and ‘innovation’ to describe the creative methods my participants use.

Communities of Practice

The various theories on practices each have their own approaches, some differing only slightly from one other (Postill, 2010, p. 6; Schatzki, 2001, p. 2). Still, Schatzki (2001, pp. 1-2) identifies shared general similarities – notably, aim, which, broadly speaking, is to overcome the structure-agency opposition. Practices are generally understood as an array of human or non-human activities (p. 2). That is, “phenomena such as knowledge, meaning, human activity, science, power, language, social institutions, and historical transformations occur within and are aspects or components of the *field of practice*” (p. 2, italics in the original). Another point that most theories agree on is that practices are embodied and

materially mediated. Moreover, they are “organized around shared practical understandings” (ibid.), and they “acknowledge the dependence of activity on shared skills or understandings” (p. 3). The disagreements concern what constitutes an activity and how it and others are connected. Science and technology scholars, for one, would include nonhuman activities. The conception of embodiment also differs, and there is no consensus about whether “anything beyond shared understandings is necessary to explain practices” (p. 3).

Work by Lave and Wenger (1991; Lave, 1991; Wenger, 1998) is particularly useful to focus on because of its emphasis on communities that share a goal and how they, in trying to achieve that goal, develop and maintain practices. As such, they become communities of practice. In my thesis, this would apply to academics who produce knowledge and the research methods and dissemination practices they undertake to do so. The element of learning is also important in Lave and Wenger’s approach. Sharing a goal and learning how to achieve it are important to my work, as I explain in the following discussion of communities of practice.

Communities of practice is a concept that was developed by Wenger and Lave (1991; Lave, 1991; Wenger, 1998) to explain how people learn (Wenger, 1998, p. ii). They view learning as a part of everyday life and not restricted to certain phases or places in it. Learning is not just done at school or other educational institutes, but rather takes place everywhere all the time. Learning is inherent to human life because people are constantly engaged in pursuing all kinds of goals, from building a career to taking up a hobby. These pursuits are done in interaction with others. Even seemingly individualistic activities, such as fishing or conducting social scientific research, are still learnt and pursued in relation to others. In fact, something can be defined as ‘fishing’ or ‘conducting research’ because it relates to others doing those activities in that way. Over time, participants define these pursuits, and the repeated interactions of a pursuit become practices. These practices “reflect both the pursuit of our enterprises and the attendant social relations. These practices are thus the property of a kind of community created over time by the sustained pursuit of a shared enterprise” (Wenger, 1998, p. 45). However, practices are not set in stone; changes, big or small, happen all the time as conditions change or people apply practices in varying ways. Nonetheless, within such activities and interactions – and their accompanying definitions – there is enough similarity and continuity for us to discern particular practices. Without them, there would be no such thing as a practice. Thus, there is a tension between perpetual change (however small) and continuity within practices. Take preparing food. Throughout the centuries, the utensils and the ingredients used to make meals have changed. By implication, our techniques to

prepare food have changed too. The times at which food is served have varied throughout the years and there are variations across countries on these points. Still, we recognise the activities surrounding food preparation as a practice.

Following others (see e.g. Postill, 2010; Rouse 2007; Schatzki, 2001), I find it helpful to think of Wittgenstein's discussion of language in his book *Philosophical Investigations* (1958). In paragraphs 65 to 71, he explains how all the things called 'language' fall under the same heading of 'language' without intrinsically having a core element in common. Instead of this one commonality, Wittgenstein (1958) argues all these things called 'language' have a *family resemblance* to one another. Illustrating with the example of games, he writes: "Look for example at board-games, with their multifarious relationships. Now pass to card-games; here you find many correspondences with the first group, but many common features drop out, and others appear. When we pass next to ball-games, much that is common is retained, but much is lost" (pp. 31-32). Still, all these things are labelled as 'games'. So "the result of this examination is: we see a complicated network of similarities overlapping and criss-crossing" (p. 32) and, in so doing, creating the family resemblance. This is the same for practices (disciplinary and other), where change and continuity not only occur over time, but also across locations. In his PhD thesis, de Wilde (1992), looks at the identity of sociology in Germany and the US between 1870 and 1930. In trying to demarcate the field, he finds no core element in sociology, but rather that it is the constant ongoingness of it that makes it sociology (see also Lynch & Bogen, 1997). De Wilde (1992, p. 12) writes: "[Academic research is practised] within the context of specific traditions such as a set of characteristic examples, operating procedures, reasoning patterns and manners of interaction.⁸" He goes on to explain the discipline's identity is to be found neither in the object of investigation's nature nor its approach to the research problems. Rather, it is to be found in its own history, the ongoingness of specific traditions, including the small changes happening over time (de Wilde, 1992, p. 12). In their book *Academic Tribes and Territories*, Becher and Trowler (2001) have a similar problem defining the term 'discipline'. Definitions are either too specific to one discipline, thereby excluding all others, or too general, thereby failing to apply to any disciplines whatsoever. Yet, we understand what a discipline is and have no trouble using the term in everyday language. Similarly, this lack of a core element and challenge to define applies to communities of practice. The practices slowly change over time, as do the people who are engaged in them, but we still call certain groups of people engaged in specific

⁸ Translated from Dutch by the author.

behaviour communities of practice.

Wenger defines a community of practice as one in which participants “*mutually engage in a joint enterprise about which they have formed a shared repertoire*” (Wenger, 1998, pp. 72-73, italics added). One such enterprise, I argue here, is conducting social scientific research, and part of that practice is shaped by research methods. At universities, which are institutes of learning, there are clearly observable times during which the practice of social scientific research is explicitly taught. Students read literature, go to lectures and take exams to test their knowledge of a subject. These activities exemplify how the knowledge that is taught has been produced through a process Wenger (1998, pp. 57-62) calls ‘*reification*’. The students also do research in which they discuss, adapt and put into practice the knowledge they have learnt. Here knowledge is gained through what he calls ‘*participation*’ (pp. 55-57). I now discuss these two terms.

Reification is the process whereby something abstract is considered as a material or concrete thing (Merriam-Webster Online Dictionary, accessed 21 October 2013). For instance, democracies or societies are not ‘real’ in the sense that they can be touched. Nonetheless, we do talk about them as able to act, collapse, react and so on (Wenger, 1998, p. 58). Likewise, research is a reified concept that can show, justify and report on any number of things, for example, the effects of policies, ‘the’ media or migration. Although research can be very many different things and can entail many different practices that are constantly changing, we do have an idea of what it means. Like Wittgenstein’s games (1958), research entails many different things though they share a family resemblance. The meaning that is attached to the concept of research is a reification. These reifications come to academics most importantly through textbooks.⁹ Textbooks contain definitions of research, show how it is done and explain specific forms which all amount to an idea of research. This is the process of reification.

Textbooks provide coherent, if unsophisticated, accounts of the history, methods, and aims of the discipline. (...) This "core" is a social (and commercial) construct, but, like other such constructs, it is not illusory; it is part of the material production of standard disciplinary practices. (...) [S]ociology textbooks portray a discipline with a common ancestry, a unified field of investigation, and a general scientific methodology. Year in and year out, the textbooks reiterate the facts of the discipline (Lynch & Bogen, 1997,

⁹ Journal articles and books contain and add to reifications, but they can negotiate, critique and change them too. The same goes for lecturing.

p. 484).

Textbooks also provide academics with the shared repertoire¹⁰ (the way of speaking about doing this kind of research, using the right concepts, asking the right questions) (Kuhn, 1970; Johnston; 2000; Lynch & Bogen, 1997; Richardson, 2004; Stambaugh & Trank, 2010). Although handbooks within the social sciences scope quite different topics in varying ways (Keith & Ender, 2004; Johnston, 2000), they are still seen as forms of authority, presenting “a coherent, thematically integrated view of a discipline” (Stambaugh & Trank, 2010, p. 664). They are, moreover, important tools for shaping academic disciplines by teaching the next generation of students about the disciplines (Stambaugh & Trank, 2010, p. 663). “[Textbooks] are part of a stable, highly standardized, widely disseminated disciplinary pedagogy. As such, they are intrinsically important to the constitution and maintenance of the discipline” (Lynch & Bogen, 1997, p. 482). In these textbooks, the process of doing research is reduced to certain clearly identifiable steps, to guidelines that describe how one should go about doing research. Deduced from previous experiences that have proven successful, they therefore help students get started, to understand what the joint enterprise is and learn the shared repertoire in order to do the research themselves.

This, however, is not the end of the learning process because the students are also expected to be able to appropriate this knowledge into the practical context of their own research. Learning is not just about the reception of factual knowledge (Lave & Wenger, 1991), but also about using that knowledge in action. This is what Wenger calls ‘*participation*’. It refers to the doing of the practice – in my thesis, of the research – in relation to and with other people who also participate in that practice (Wenger, 1998, p. 55).

Methods, in their abstract form, do not perfectly match the concrete circumstances of the actual research, thus only giving students guidelines. As O’Reilly (2005, p. 4) and Pink (2007, pp. 4-6) note in the introductions of their books, respectively, *Ethnographic Methods* and *Doing Visual Ethnography*, these works are not recipe books; they do, however, provide guidelines for doing research, leaving readers to learn and apply them as they see fit. Bryman (2008, p. 27) puts it as follows: “All social research is a coming-together of the ideal and the feasible.” This can be a struggle for students as is nicely illustrated by the following quote from Boyd (2008, p. 26). She recounts two similar events I have often experienced myself,

¹⁰ These guidelines are never set up and maintained by a single institution that must be obeyed by everyone the way the constitution or laws are. As such, there is no overt power. Nonetheless, the guidelines do exert power as I explain at the end of this chapter in discussing how research is ‘supposed’ to be done.

both as student and lecturer.

As an undergraduate, I once (foolishly) asked my professor how long the assigned paper had to be. “Long enough to touch the ground” was what he told me. Needless to say, this response did not satisfy my desire to know the “correct” answer that would confirm that I was being a “good” student. (...) In a graduate school qualitative methods course, I asked my advisor how I would know when I was finished collecting data. He offered the same Dumbledore smile as the previous professor before responding, “When you stop learning new things without expanding the scope of your question.” Once again, I asked a question of the wise and received a koan¹¹ in response. While I have not reached methodological enlightenment, I have begun to appreciate the brilliance of these answers.

It is important to note that although appropriating methods becomes easier as the researcher becomes more experienced, the doubts do not disappear, especially when taking up new practices, such as creative methods. Indeed, as Ingold (2010a; 2010b) argues, every situation and thus every research project brings things together in a new way, which means the students will have to improvise. This is because

no system of codes, rules and norms can anticipate every possible circumstance. At best it can provide general guidelines or rules of thumb whose very power lies in their vagueness or non-specificity. The gap between these non-specific guidelines and the specific conditions of a world that is never the same from one moment to the next not only opens up a space for improvisation, but also *demand*s it, if people are to respond to these conditions with judgement and precision (Ingold & Hallam, 2007, p. 2, italics added).

Similarly, Roe and Greenhough (2014, pp. 46-47) suggest that while habits seem to continue unchanged, small modifications (which they also call improvisations) are made in interaction with the context all the time. The same goes for methodological habits. Holland, Lachicotte, Skinner and Cain (1998, pp. 17-18) argue that these little improvisations “using the cultural resources available” are the openings to change. Wenger, too, says there is no exact fit

¹¹ According to the online Oxford Dictionary, a koan is “[a] paradoxical anecdote or riddle without a solution, used in Zen Buddhism to demonstrate the inadequacy of logical reasoning and provoke enlightenment”.

between the rules and procedures and their practical application. This necessitates their meaning to be reinterpreted in each and every situation. Furthermore, there is always room to newly interpret them. For Wenger (1998, pp. 58-62), the rules, laws, procedures and other reifications are never endpoints, but rather part of an on-going *negotiation of meaning*.

The negotiation of meaning is a process that is shaped by multiple elements and that affects these elements. As a result, this negotiation constantly changes the situations to which it gives meaning and affects all participants. In this process, negotiating meanings entails both interpretation and action. In fact, this perspective does not imply a fundamental distinction between interpreting and acting, doing and thinking, or understanding and responding. All are part of the ongoing process of negotiating meaning. This [productive] process always generates new circumstances for further negotiation and further meanings. It constantly produces new relations with and in the world. The meaningfulness of our engagement in the world is not a state of affairs, but a continual process of renewed negotiation (Wenger, 1998, pp. 53-54).

Likewise, Ingold and Hallam (2007) see practice unfold along lines that continually come into being, in an ever on-going process (Ingold, 2010a, p. 3) in which improvisation is an important element. For improvisation to take place, the codes, rules, norms or reified practices have to be known. Improvising does not mean doing random things; it means taking what you have learnt and appropriating it. So, in conducting research, students have to negotiate and renegotiate the meaning of what they have learnt about the joint enterprise known as ‘doing research’. Their improvisation is an interpretation in which they slightly shift the meaning of what research is and the reified practices of methods. This new meaning is never completely original or independent from its past, but at the same time never exactly the same as before (Wenger, 1998, pp. 52-53; Ingold & Hallam, 2007, p. 11).

All that we can do and say may refer to what has been done and said in the past, and yet we produce again a new situation, an impression, an experience: we produce meanings that extend, redirect, dismiss, reinterpret, modify or confirm – in a word, negotiate anew – the histories of meanings of which they are part. In this sense, living is a constant process of *negotiation of meaning* (Wenger, 1998, pp. 52-53, italics in original).

Practices, thus, are constantly evolving, but the changes that are made are always in relation to earlier work, to what was done before. Wenger in the above quote says we “may refer to what has been done”, but I would say that in the case of research, connections to the past *must* be made; otherwise, research risks being classified as something else – something that is not research. The process of negotiation and improvisation therefore is determined by traditions, by reifications. At the same time, that new knowledge be produced is something demanded of research. Becher and Trowler (2001, p. 23) argue that the particular ways academic communities of practice have organised their professional lives are important reifications providing coherence and stability to practices, while at the same time academia is “an environment in which progress and development, not stability, is the normal expectation” (p. 100). The tension between change and stability is an important aspect of academic practices, observable throughout the thesis in the way that creative methods emerge and how my participants negotiate creative methods into their practices. What’s more, academics are part of multiple communities of practice. In part, this is due to the nature of their research interests, which often consists of different and likely changing topics; but also, within one topic, there are different levels of communities of practice. For instance, a sociologist is a member of her sociology department at her university, but also of the British Sociological Association and the International Sociological Association. Each level’s importance will differ for the researcher, but exchanges between those communities of practice will demand that she negotiate the different reifications, which will lead to change.

Students at the beginning of their academic career often only participate in the community of practice of the department in which they do their course. This community of practice is thus important in their understanding of the joint enterprise and how they engage with reifications and practices. While the students are appropriating the methods through their formal learning, the interaction with other students and researchers is of huge importance. It is here where they mutually engage in the joint enterprise, where they learn and discuss the shared repertoire and where they acquire knowledge about how to do research that is not to be found in handbooks. The students, in a way, do participant observation during their learning process by being around other academics (senior or otherwise), seeing their working routines, listening to them, engaging in casual conversation. But also, getting a sense of the informal hierarchies teaches the students how research can be done and is valued in practice. As Löfgren (2014, p. 74) points out, such working routines are often thought of as personal ways of doing things, but are actually influenced by these subtle cultural processes of learning; they are fraught with power. What further becomes clear is that social skills are important in

becoming part of a community of practice. Holmes and Woodhams (2013) show how social skills, such as small talk and knowing what kind of jokes to make and when, are important in becoming accepted members of a community. This shows that being a member of a community entails more than just knowing the reifications and how to apply them. In the discussion on power that follows, I describe how social skills are also important for securing research funding and enabling collaboration between researchers.

To reiterate, learning and participation is not a one-way process. People bring different educational backgrounds, experiences, epistemological perspectives and personal characteristics to the table, which means they interpret the meaning of the practice differently. It also means that participation does not have to be collaborative; “it can be equal, conflictual, harmonious, intimate, political, competitive, cooperative” (Wenger, 1998, p. 56). Think, for instance, of critical questions asked by supervisors or the comments accompanying rejected papers. Through participation, which entails improvisation and negotiation, practice then changes. As a consequence, what is considered the joint enterprise and how it is talked about, *the shared repertoire*, also changes. This is reflected in new reifications, such as new editions of handbooks, changed curricula and other ways of teaching, that find their way back into participation through the appropriation of these new reifications.

In the literature on academic research communities and their practices, the role reifications play in maintaining stability – and thus how they exert power – is quickly recognisable. Descriptions by Becher and Trowler (2001) and Barnes (1982) emphasise how they are forces to maintain a certain status quo. These traditions exert a certain power, forcing continuity through their existence.

Alongside these structural features of disciplinary communities, exercising an even more powerful integrating force, are their more explicitly cultural elements: their traditions, customs and practices, transmitted knowledge, beliefs, morals and rules of conduct, as well as their linguistic and symbolic forms of communication and the meanings that they share. To be admitted to membership of a particular sector of the academic profession involves not only a sufficient level of technical proficiency in one’s intellectual trade but also a proper measure of loyalty to one’s collegial group and of adherence to its norms (Becher & Trowler, 2001, p. 47).

[M]echanisms of socialisation and knowledge transmission, procedures for displaying the range of accepted meanings and representations, methods of ratifying acceptable

innovations and giving them the stamp of legitimacy [are needed]. All of these must be kept operative by the members of the cultures themselves, if its concepts and representations are to be kept in existence. When there is a continuing form of culture, there must be sources of cognitive authority and control (Barnes, 1982, p. 9).

Both Becher and Trowler and Barnes thus highlight the importance of reifications in the joint enterprise of communities of practice. Reifications are a starting point for negotiation and improvisation, thereby enabling participation in those communities.

Negotiations and Power

Following Becher and Trowler (2001) and Barnes (1982), I deem it useful to pay more attention to power in the process of practices, which I have so far only alluded to. Despite Wenger's (1998, p. 56) classification of participation as potentially conflictual or competitive and Lave and Wenger's (1991) focus on the unequal distribution of resources and power, the work lacks a more thorough investigation of power as a concept (Contu & Willmott, 2003). According to Contu and Willmott (2003, p. 287), the definition of communities of practices emphasises coherence and consensus. The word 'community' itself reflects an understanding of them as necessarily harmonious¹² (ibid.). Moreover, Lave and Wenger (1991, p. 292) do not follow up their critical theoretical standpoints with similarly critical interpretation or discussion of empirical studies. Contu and Willmott fear that emphasising the harmonious side of communities of practice runs the risk that they become understood as a tool for effective learning (i.e. complying with what those in charge say), which in their field of organisational studies is a clear threat. Within academia – and specifically the social sciences – the threat is subtler, I would argue. Change is an ambivalent but nonetheless definite component of research practice. Academic researchers are expected to explore new topics, and the goal is to contribute new knowledge.¹³ Students are taught certain theories, but are at the same time encouraged to be critical of them and, as already mentioned, are expected to appropriate those theories or methods in their own research. Additionally, this can be discerned in the criteria for academic pieces of work. A case in point, my PhD thesis must “make a significant original contribution to, and show a critical appreciation of, existing

¹² Although I agree, I believe any word would evoke similar debates of being either too harmonious or too antagonistic.

¹³ This has not always been the case. In the fifteenth and sixteenth centuries, universities focused primarily on conveying existing knowledge rather than producing new knowledge. In the seventeenth and eighteenth centuries, the focus started to shift towards intellectual innovation (Burke, 2000, p. 33; de Wilde, 1992, p. 7).

knowledge in the subject” (Loughborough University, 2014b). Another illustration, for an article review I was asked to do, the first of four review criteria asks: “Does the paper make a significant and original contribution to its subject area?” These are just two examples of academic practices that show how change – in the form of an original contribution – is valued. It is embedded in academic practice as the joint enterprise (Wenger, 1998, pp. 72-73). Deviating slightly from practices in the academic community is thus encouraged, but the ways in which academics are allowed to do this are highly specific.

The arbitration of what is and is not allowed is what Gieryn (1983, p. 782; 1995; 1999) calls ‘boundary-work’, or ‘the demarcation problem’, as it is called in philosophy of science.¹⁴ Gieryn draws attention to the negotiation of what is considered as academic practice and how academics discursively distinguish between academic and non-academic activities. In line with Wenger, he sees no essential characteristics for academic and non-academic work and, as such, no real differences between them. Rather, the distinction is based on social conventions (Gieryn, 1983, p. 792), which are negotiated between different actors, who exercise more or less power to – even if only temporarily – consolidate their meaning. This often happens when “scientific credibility is on the line” (Nielsen, 2008, p. 173) or between adjacent disciplinary fields (Schoenberger, 2001). In his own work, Gieryn (1983; 1999) looks at historical cases in which science was demarcated from work that was considered non-scientific. Others have taken the concept further, looking at contemporary cases of boundary-work, but also at distinctions within and between disciplines (as suggested by Gieryn, 1983, p. 792), including the social sciences (e.g. Friman, 2010; Nielsen, 2008; Saukko, Reed, Britten & Hogarth, 2010). One should keep in mind that, like the negotiation of meaning, boundary-work is constantly being done, even in times of relatively stable ideas of what is and is not academic. According to Albert, Laberge and Hodges (2009, p. 185), Gieryn has sometimes framed “the struggle for scientific authority as dichotomous; that is, groups who compete for scientific authority either succeed or fail in representing their work as scientific”. They also feel the concept encourages us to think of two camps, one on each side of the boundary (ibid.). Furthermore, Gieryn’s historical cases make it tempting to talk in terms of settled boundaries. It is important to stress that boundary-work is a rhetorical style, done discursively. Gieryn does not refer to fixed boundaries. Rather, he shows how scientists draw these boundaries anew each time they distinguish their work as different from what they perceive as non-scientific work. This way it is also possible to speak of boundary-work when

¹⁴ The concept of boundary-work is mostly attributed to him, but as Gieryn (e.g. 1995, p. 441) himself acknowledges, others have worked on it before.

boundaries are made fuzzy or when they are broken down. Calling the latter anti-boundary-work, as Nielsen (2008) does, is therefore not really necessary because Gieryn (1983) already referred to such an appropriation of the concept. Nevertheless, the differentiation of the terms illustrates clearly the different aim of the boundary-worker and, in so doing, provides more clarity. Boundary-work is thus a constant, on-going discursive practice in which academics on the basis of social conventions – or reifications, in the terminology of communities of practice – accept or reject research as academic, thereby consolidating, blurring or breaking down those boundaries on which the acceptance or rejection is based in the first place.

Another important point is that the boundaries need not be between science and non-science, but rather can be built up or broken down between or within disciplines (Albert, Laberge & Hodges, 2009). I would say that in academic practice, there is a constant discursive renegotiation of what academia means among different people from different disciplines, but also among parties external to academia, such as governments, journalists and companies.

Boundary-work is thus an on-going process of inclusion and exclusion, and researchers struggle more or less to have their work accepted. As said earlier, the practices themselves exert power because of their reified nature in determining what is possible. The next questions to ask then are: Who can draw the boundaries? And who can challenge the boundaries others have drawn? In these negotiations, not all participants – who comprise academics, but also governmental organisations, the industry and the general public – get an equal say in establishing boundaries. So how is power exercised in negotiating these boundaries as well as in negotiating practices, in general?

Power is an elusive concept. Power is not something that can be owned or that is stable; it is established anew in every negotiation. However, clearly some people have more leverage than others. According to Bourdieu's article *The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason* (1975, p. 23), people with more *scientific authority* tend to have more *symbolic power* to resist or push through certain definitions. What are scientific authority and symbolic power then? Before answering this question, it is useful to briefly address the different kinds of capital that Bourdieu (1989) conceptualises. In general, he distinguishes between three – economic, cultural and social – but in the context of the sciences, he also uses symbolic and scientific capital. All these different kinds of capital are important in the conduct of research and the power relations involved.

Economic capital is, quite simply, money or resources that are directly transferable into money (Bourdieu, 1989, p. 122). Cultural capital refers to the knowledge someone has,

and is recognisable in educational qualifications. It can be transformed into economic capital under certain conditions (pp. 122-123), such as work. Cultural capital can be attained through education, but also through upbringing. It is, moreover, personal in the sense that, unlike economic capital, it cannot be transferred to someone else. Rather, it is an investment of personal labour and time, recognisable through diplomas and titles – institutional acknowledgements. Acknowledgements from highly esteemed institutes are ostensibly indicators of better or more cultural capital. Social capital refers to the network someone has, which involves a person's social skills, but also family ties (p. 132). This works as a guarantee for a given individual, making it easier (or harder) for him or her to pursue life goals. To illustrate, the son of a notorious bank robber might find it harder to get a job than the son of a famous surgeon because a surgeon is a better guarantee than a bank robber. Of course, this is a simplistic example, but it shows how social capital can work.

The different kinds of capital are linked, but their relation is not fixed. Economic capital can pay for a proper education, which provides cultural capital as well as social capital, which can then be converted back into economic capital. However, economic capital is not enough to buy cultural and social capital. As stated, capitals are also gained through upbringing and family ties.

Symbolic and scientific capital, then, are closely related to cultural capital, but they are not the same (Bourdieu, 2004, pp. 55-56). Symbolic capital is the capital an individual possesses according to the peers who have the ability to judge that capital. Cultural capital certainly plays a role, but is valued through the perception of peers (p. 55). Scientific capital (or scientific authority, which Bourdieu seems to use interchangeably) is symbolic capital specifically in the sciences. Bourdieu (2004, pp. 55-56, italics in the original) puts it as follows:

[A] scientist's symbolic weight tends to vary with the distinctive value of his contributions and the *originality* that his competitor-peers recognize in his distinctive contribution. (...) The position a particular agent occupies in the structure of the distribution of this capital, as perceived by the agents endowed with the capacity to perceive and appreciate it, is one of the principles of the symbolic capital imparted to that agent, inasmuch as it is generally linked to his contribution to the progress of research and his distinctive value.

Scientific authority thus comes with having the approval of others in the field who

acknowledge a researcher as a legitimate producer of knowledge (Bourdieu, 1975, p. 23). The more approval a researcher has received throughout the years for his or her products – in my thesis, research done according to agreed-upon practices – the more symbolic power he or she has “to constitute the given” (Bourdieu, 1991, p. 170). This power is certainly not a given; the worth of symbolic capital is under constant renegotiation, for instance, through the work of others. This means researchers must keep on producing new research that takes the latest developments (and negotiations) into account.

Furthermore, Bourdieu (1975, p. 23) says the approval of researchers’ symbolic capital is necessarily given by the researchers’ competitors – in my case, academic peers. Only they have sufficient knowledge to understand the work and to recognise its scientific authority. Simultaneously, they are vying for the approval of others in their discipline. I agree, but argue that the picture is slightly more complicated; I would add that the approval-granting researchers within the discipline are not a set group, and that the researchers themselves do not operate in a single discipline. They often participate in several communities of practice. Bourdieu (1975, p. 21) himself says the analysis of scientific practices always must be two-sided. It can never just be political or ‘purely’ intellectual, meaning that scientific authority is not just based on cultural capital, but also on economic and social capital. He explains how all these capitals are important in convincing others of the ‘right’ definition of science in the struggle for resources. Bourdieu’s work has been criticised, though, for being too narrow and excluding non-scientists (Sismondo, 2011, p. 93).

Latour (1999) argues that the development of science cannot be analysed separately from politics and society, because they are inextricably connected (see also Hammersley, 2011, pp. 2-4). In the chapter *Science’s Blood Flow*, Latour (1999) discusses how science related to society, giving as a case in point how in the run-up to the Second World War, the French government, the French National Centre for Scientific Research (CNRS), a French scientist and a Belgian company were *all* part of generating the knowledge necessary to produce an atom bomb. He asserts that the scientific production of knowledge is always connected to and comes in interaction with political, societal and economical goals. Latour (1999, p. 80) writes: “The notion of a science isolated from the rest of society will become as meaningless as the idea of a system of arteries disconnected from the system of veins.” Similarly, Lynch and Bogen (1997, p. 483) note that the context in which scientific research is done “is not an external ‘factor’ impinging on the laboratory, nor is it a surrounding set of

institutions; it is a matrix for producing facts and artifacts.¹⁵”

These connections to the non-academic show there are also stakeholders outside of academia who judge research's worth and thereby determine the symbolic capital of a particular researcher. A contemporary example in the UK is the 'pathways to impact' component in applications for the Research Council UK (RCUK), showing how economic and societal impacts (Hammersley, 2011, p. 10; RCUK, 2014) have become important in the Research Excellence Framework (REF). The London School of Economics has even published a handbook on impact, providing a discussion of what it is, how it can be achieved both inside and outside of the academy and how to measure it (LSE Public Policy Group, 2011). In addition, governments or companies might not necessarily have the symbolic capital to judge or generate research, but their economic capital is crucial for funding it. Social capital is similarly important, for instance, when collaborating on research teams or convincing boards of directors, funders or department heads of the worth of specific research. Economic and social capital must therefore be taken into consideration when thinking about how research is conducted and how the research process is influenced by different stakeholders, each with his or her own kind of capital.

Thus far, I can conclude that what is considered as academic knowledge and who is able to judge that is much more complex than academics simply judging the quality of colleagues' work. As Somekh (2007) says in the afterword to the book *Knowledge Production: Research Work in Interesting Times*, what is considered knowledge has always been politically contested, but now the traditional bastions of knowledge production are challenged and delegitimised on that function (p. 198). The legitimacy of academic scientific knowledge is questioned more and more, and is therefore no longer an indisputable source of power. According to van Zoonen (2012, p. 56), this delegitimation is also the consequence of critical theory “[having since] identified ‘knowledge’ as an instrument of power that needs to be contested” (see also Hammersley, 2011, pp. 5-6). The so-called crisis of representation (Schwandt, 1997, p. 21) is one of many ways knowledge, its construction and authority have been challenged. This means that symbolic power as Bourdieu (1991, p. 170) discusses it – to “constitute the given” – or as Loveman (2005, p. 1655) describes it – “the ability to make appear as natural, inevitable, and thus apolitical, that which is a product of historical struggle and human invention” – is under pressure in the social sciences. Still, there are many practices

¹⁵ This development – that science is no longer purely done within academic institutions, but in interaction with industry and government – has been theorised and criticised. Examples are 'mode 2 knowledge production' (Gibbons, Limoges, Nowotny, Schwartzman, Scott & Trow, 1994) and the 'triple helix' (Etzkowitz & Leydesdorff, 2000).

in academia where the power to include or exclude researchers and their work is present. Mulkay (1972), for instance, describes how the reward system outlined by Merton (1973; Glover & Strawbridge, 1985, pp. 34-35) enables academics to structure the production of knowledge (their joint enterprise) and the role power, discipline and conformity play in it. Boundary-work is an important part of that system (although Mulkay does not label it as such), for instance, in the socialisation of new members and the subsequent disciplining of members through a well-maintained organisation of the negotiation of meaning. As such, Mulkay's discussion of the reward system is also a nice description of an academic community of practice.

To reiterate, in the production of knowledge, it is considered important for academics to produce new knowledge and to make an original contribution, for example, in the form of journal articles and book chapters (Burke, 2000, pp. 33, 114; Glover & Strawbridge, 1985, p. 35). The only direct reward academics get for these contributions to knowledge is *recognition* (Glover & Strawbridge, 1985, pp. 34-35; Mulkay, 1972, p. 26). Of course, because the allocation of funds or hiring of staff is partly based on the researcher's publications, the hope is that recognition leads to other rewards such as promotions, funding for research and tenure (Mulkay, 1972, p. 29).

Bourdieu (1975) says that researchers will work on those issues that are generally regarded as the most important ones in order to get the most rewards because, according to him, every choice within academia is aimed at maximising scientific profit. However, when more academics are researching these issues, these rewards will decrease. As a result, he claims some will move on to less-researched issues (pp. 22-23). Mulkay (1972, p. 34) argues that to make sure their contribution is original, academics research unexplored areas with many unanswered questions. This provides many more opportunities to make an original contribution than would exploring a much-studied area where only the harder-to-solve problems remain. Another reason, Mulkay argues, is that the norms in these under-researched areas have not been fully established, leaving more room for trying out new ideas. In Lave and Wenger's terminology, there are fewer reifications for researchers to work with. Resistance to new problems, research techniques or possible answers is much less likely to occur, Mulkay contends, when consensus has not been established yet. This portrays academics as calculating, rational individuals whose only goal is scientific authority for the sake of it. Bourdieu (1975) also assumes all academics agree on what the most important issues are. Finally, both he and Mulkay (1972) mistakenly assume that moving into less investigated areas will make it easier for an academic to get published and gain recognition,

funding and tenure. As should be clear from my discussion of communities of practice and boundary-work, this is not as easy as Bourdieu (1975) and Mulkay (1972) make it sound. Mulkay (1972, p. 18) acknowledges that “[o]riginality is only appreciated in so far as it contributes to what we already agree upon, deem as true” and that “[r]adical departure from current cognitive and/or technical norms, like other forms of social deviance, usually provokes attempts at social control” (p. 16). Innovation has to be embedded within accepted academic practices (p. 11), for instance, in peer review, which means in some cases the assessment criteria must be adjusted (Sparkes 2009, p. 304).

To explain fully why conformity to academic practices is so important would be beyond the scope of this chapter, but one noteworthy reason is the reward system. As explained above, funding, tenure and promotion are based on this system, which means that those who want to qualify for these rewards need to adhere to the rules of the system. To make sure members of the academic communities of practice conform to the rules of their own communities, that same reward system provides an explanation. First, newcomers are socialised into a specific field of academia. Students, for instance, learn the body of established knowledge, the kind of questions they can pose and the proper ways to research and answer those questions (Barnes, 1982, pp. 16-17; Mulkay, 1972, p. 19). They learn about the practice and what the joint enterprise is, though do not have much power to negotiate its meaning. If students show they know well enough what the joint enterprise is, they are rewarded through obtaining their degree. If they do not become sufficiently accustomed with the habits and customs of the academic community, they fail the course. If they continue working in academia, they will need to obtain the aforementioned recognition (which will, they hope, ultimately lead to symbolic power). In order to achieve that recognition, they will first need to share their work, such as abstracts for conferences or papers for journals, with other academics. This is what Mulkay (1972, p. 23) refers to as social exchange. Through the process of peer review, their work will then be rejected, accepted with or without changes. Conformity can be exercised through rewarding those works that fit within the cognitive and technical norms by, for instance, granting publication in a journal, preferably a highly ranked one. Another reward is a positive citation in a publication. With enough positive citations, findings become part of accredited factual knowledge (Gilbert, 1976).

The reward system is one set of practices in which power is exercised, but it is important to note that established researchers do not influence the outcomes of the reward system alone. Lecturers might accept or reject students’ or researchers’ papers, although there are financial pressures to have students succeed. Researchers can send their work to other

journals after it is rejected and they can make connections in other fields. Historical configurations, the different kinds of capital and the reifications are constantly negotiated and together help determine the way power is perceived and played out in such situations.

Academic Practices and Innovation

There is a complex interaction between traditions, practices and the drive for original and new knowledge. In between, academics and other stakeholders are constantly negotiating their own goals, positions and research. In such an environment, I argue, only small changes can be made to practices. Over time, they might lead to bigger changes, but this will take a long while. Nonetheless, funding bodies nowadays continuously call for research and their methods to be innovative. Taylor and Coffey (2008, p. 4) indicate how innovation is an implicit criterion for research proposals, as funding bodies increasingly ask for methodological innovation. The methods my participants use are often referred to as ‘innovative’ (the 8th annual conference Methodological Innovations at Plymouth University; NCRM Research Methods Festival; Pain, 2009; Taylor & Coffey, 2008; Wiles, Pain & Crow, 2010; Xenitidou & Gilbert, 2009, pp. 60-65). According to Taylor and Coffey (2008, p. 523; 2009), it is almost standard practice to claim some form of methodological innovation in research reporting.¹⁶ Rowan (2007, pp. 113-117) reminds us that ‘innovation’ has now become such a pervasive term both within and outside of academia that its meaning has become hollow. What, though, is innovation?

Here the problem of innovation as a concept arises. To define innovation is a tricky business. One important problem is that it is a relational term, needing to be understood in a specific context. According to the Merriam-Webster Online Dictionary (accessed 23 February 2011), innovation means “the introduction of something new, a new idea, method or device”. Innovation seems to imply a break with the past, but as Wenger (1998) says, meanings are constantly renegotiated, though are never entirely new. The question, then, is how much an idea, method or device needs to differ from previous ones to be labelled ‘innovative’. I have just established that new ideas are always embedded within established and familiar ideas; otherwise, they are not very well received (Mulkay, 1972; Wenger, 1998). They are seen as mere idiosyncrasies if they do not “make sense within a more widely inhabited universe of meaning and accord with its communicative conventions” (Ingold & Hallam, 2007, pp. 6-7). As said, within academia, ideas and methods need to be built on previous work. Moreover,

¹⁶ Forbes (2003) reports on a similar overuse of the word ‘cutting-edge’.

ideas or methods can be innovations within certain disciplines, but standard practice in others. Several authors have tried to come up with levels or categories of innovation to circumvent these problems (Dodgson & Gann, 2010, pp. 13-14; Wiles, Pain & Crow 2010, p. 14), but, in my view, classification does not eliminate the problem of determining the origin or originality of the ideas. Williams and Vogt (2011, pp. 1-2) in the introduction to the *Sage Handbook of Innovation in Social Research Methods*, start by acknowledging this issue and then apply several disclaimers to the term. They, like Dodgson and Gann (2010) and Wiles, Pain and Crow (2010), point out that whether a development is understood as an innovation is context-dependant. That, I believe, makes the term ‘innovation’ virtually meaningless. Xenitidou and Gilbert (2012, pp. 1-3) also discuss the difficulty in defining innovation in relation to methods, saying it might be better to talk about developments rather than innovation.

Another issue is that research can only, if at all, be judged as innovative after it is completed. Writing more generally about innovation, Ingold and Hallam (2007, pp. 2-3) explain how innovation is a backward reading of the process of creation – which in my thesis would be of doing research.

The difference between improvisation and innovation, then, is not that the one works within established convention while the other breaks with it, but that the former characterizes creativity by way of its processes, the latter by way of its products. To read creativity as innovation is, if you will, to read it backwards, in terms of its results, instead of forwards, in terms of movements that give rise to them.

What is different about improvisation is that it is seen as forward-going. Ingold and Hallam (2007, p. 1, italics in the original) call it “*generative*, in the sense that it gives rise to the phenomenal forms of culture as experienced by those who live by them or in accord with them”. Crucially, Ingold and Hallam (2007), like Wenger and Lave (1991), emphasise the process rather than the products. Indeed, those products, such as academic studies, are never finished. They are not the end, but rather a moment in an on-going process, in an on-going production of knowledge. The same goes for the development of creative methods and the people who use them. Rather than seeing them as innovative methods, as the NCRM does, and trying to categorise them according to levels of innovation, I see creative methods as methods in which researchers improvise (as in all other methods) or, in Wenger and Lave’s terms, negotiate their way through. “To improvise is to follow the ways of the world, as they unfold, rather than to connect up, in reverse, a series of points already traversed” (Ingold,

2010a, p. 10). Connecting up these points, is what researchers do in papers, but in everyday academic practice, they improvise.

Research Reifications

In the remainder of this thesis, I look at the practices of my participants and how these relate to the reifications of how research is ‘supposed to be done’. I answer questions such as: How did my participants and others within their communities apply those reifications in doing their research? How did my participants improvise from those reifications? On the basis of which reifications is the research of my participants accepted or rejected? It is therefore important to know a bit more about those reifications, about the way research is ‘supposed to be done’. It should be clear from the discussion of reifications in this chapter, these are not fixed and they are interpretable in different ways. Further, we must remember that communities of practice can operate at different levels, such as the international, national or departmental. The focus and application of, for instance, cultural sociology on an international level is quite different from the national context in the UK or the departmental one at Loughborough University. In addition, there might be slight differences between the reifications of various community of practice in terms of how research should be done. This is especially pertinent when researchers are engaged in multiple communities of practice, which they almost always are, because research topics often pertain to different fields. Moreover, as already stated, these reifications are constantly changing (de Wilde, 1992; Holland, Lachicotte, Skinner & Cain, 1998; Roe & Greenhough, 2014; Wenger, 1998). This means that my discussion of how research is ‘supposed to be done’ is inevitably incomplete and subject to change. Nonetheless, it provides a context in which I can start discussing the practices that I analyse throughout the thesis.

An implicit reification is that the further along one is in one’s academic career, the less one should rely on textbooks. They may be a handy way into a subject, but a serious researcher should ultimately read the original article, book or chapter (Seale, 2004, pp. 2, 6). In a chapter I wrote for my first year report, according to one of my supervisors, I relied too much on textbooks.

You rely quite heavily on Delanty and Strydom, which is a textbook really. At this level you should be referring to the original works, demonstrating you have read Hammersley’s work (for example) itself, not second hand. And really try to avoid

referring to undergraduate textbooks such as Bryman's. By all means, use these to find information but then follow up their references for yourself. By the way, Denzin has written a lot on this topic you need to cover too.

For this section, though, textbooks are excellent precisely because they simplify and present “a coherent, thematically integrated view of a discipline” (Stambaugh & Trank, 2010, p. 664), as I discussed above in the discussion of reifications. So here I do rely on, amongst others, Bryman's (2008) *Social Research Methods* and Seale's *The Quality of Qualitative Research* (1999).

In the appendices of his book, Seale (1999, p. 189) offers an overview of “criteria for the evaluation of qualitative research papers”, which provide insight into how research should be done. For instance, research should be done systematically (pp. 190-191). The choices for the methods of participant recruitment, collection and analysis should be substantiated (p. 189). The research should also have sufficient depth through enough arguments substantiated by adequate data (pp. 190-191). “Are the results credible and appropriate?” Seale asks (p. 192). Furthermore, the research must be clearly contextualised, allowing the researcher to reflect on her use of the research methods, own position and relationship to the participants (pp. 190-191). Finally, the research needs to have been conducted ethically (p. 192).

Bryman (2008, p. xxix) offers another angle, providing valuable insights into how we are supposed to do research in discussing “Why it is important to study methods?” He says, for instance, that learning about methods teaches a student the variety of methods, how to properly apply them, when it is appropriate to apply which method, the different stages of the overall research process and how they connect with one another (ibid.). Seale (1999, p. 189) echoes this when he asks: “[A]re the methods appropriate for the question asked?” This points to thinking through the research process and returns in the writing of a proposal and research design. Planning the research forces the researcher to think through the process of possible choices (Bryman, 2008, pp. 66-76). It also allows the researcher to argue and defend the choices made (p. 664). Furthermore, training in research methods enables the student to be critical and to judge other research and the assumptions on which it is based (p. xxviii). It enables the student to see what is good research and what is not (p. xxix). It is these reifications that students, as well as my participants, are socialised into.

Research, furthermore, needs to connect to literature, theory and ontological and epistemological ideas concerning the research topic (Bryman, 2008, pp. 4-5; Seale, 1999, p. 189). This means that research disseminations often start with a theoretical framework and

then connect findings with theory in the analysis and the conclusion. This connection is helpful, moreover, in establishing the research's contribution to highlighting gaps in the literature and how it may contribute to filling them. It shows how the researcher is making an original contribution (Bryman, 2008, p. 74; Burke, 2000, p. 33; de Wilde, 1992, p. 7). It shows that the researcher is aware of what has been done before in the field and that the work is not a repetition of other people's work. This is directly linked to the question of why research should be done in the first place, the answer of which is providing new knowledge. As mentioned, this has not always been the case, though over the last centuries, universities have evolved from knowledge-conveying to knowledge-producing institutes (Burke, 2000, p. 33; de Wilde, 1992, p. 7). This means researchers need to add to the established body of work.

Another important practice is referencing. Because academic work is based on the reward system (Mulkay, 1972), it is important to give credit to other researchers' work, and plagiarism is an absolute academic sin (Berg & Lune, 2012, pp. 387-390; Bryman, 2008, pp.

107-109; Department of Social Sciences, 2013, p. 39). Becher and Trowler (2001, p. 77) argue that the importance of the researcher's reputation makes it essential to safeguard one's research accomplishments. "Academic work places a high value on the originality of the work that is presented in any kind of output" (Bryman, 2008, p. 107). Another reason for citing is that referencing work shows "you are aware of the historical development of your subject, (...) [it] shows that you recognize that your own research builds on the work of others" (p. 101). In addition, it shows the writer's knowledge of the subject, or lack thereof, and helps substantiate the argument (ibid.). Becher and Trowler (2001, p. 114) also say that referencing others' work helps "to reinforce the statements you put forward", but even more, "it suggests that you are keeping good company". Gilbert (1977, p. 116) nicely describes the importance of citations.

Thus one purpose of giving references is to provide justifications for the positions adopted in a paper. Another purpose is to demonstrate the novelty of one's results. This is often achieved by reviewing the current state of knowledge in an Introduction, and then showing or implying that the findings reported constitute an advance. Yet another purpose is to indicate, usually in a conclusion, how these findings illuminate or solve problems which arise from other, cited work, and thus to demonstrate the importance of the author's research.

Learning these skills comes back in many teaching forms. Undergraduates are taught in their

first year how to reference and why this is important. In my research Master's, part of the weekly coursework was to hand in one page on the literature we had to read for that week containing the following elements: central argument, summary, question (CASQ). The question at the end especially forced us to bring into practice the elements Bryman discusses. In the PhD club of my former department, PhD students were required to meet every two weeks to discuss a written piece by one of their fellow students. This could be an article, a chapter for the thesis or a proposal for new research. All the other students had to read it and prepare comments to improve the written work. These are but three examples showing such academic practices and how to apply them to one's own and others' research.

Evaluation Criteria

Other important indicators of how research should be done are found in the research evaluation criteria. This initially developed from the written communications between scientists that got published in a sort of newsletter (for a short history of the development of the journal article, see Gilbert, 2008, pp. 487-489; 1976, p. 286), allowing other scientists to respond to with critical comments (Gilbert, 2008, pp. 487-488) and reaching a wider audience. To defend their research from potential criticism, scientists tried to show they had abided by specific standards of conduct (p. 489), which had to be described in a way so others could replicate the research and get the same results. Criteria to evaluate research on started to emerge.

Such criteria, however, can be problematic for qualitative research. Thus, in the twentieth century the debate began concerning appropriate criteria for judging qualitative research and the difference between ideal and actual applications thereof.¹⁷ The criteria discussed do give an idea of how research can be assessed and, by implication, how it should be done. In fact, the discussion surrounding these criteria is a nice example of how reifications – the criteria that make it to handbooks of qualitative research – are negotiated and contested by the participants in the community of qualitative research practice (Hammersley, 2007, pp. 288-289).

Part of the debate concerns which criteria are appropriate for qualitative research: post-positivistic or interpretivist. Like all concepts, theories and approaches within academia,

¹⁷ See, for instance, Hammersley (2007), J. Smith (2009) and Smith and Deemer (2003). For a discussion specifically on creative methods, see Sparkes (2009). Hammersley and J. Smith (with various co-authors), in particular, have debated this issue over the years. Although they have not come to an agreement, they have certainly fleshed out the discussion.

these two are often used as shorthand for a whole range of different viewpoints (Hollis, 1994, pp. 41-42). In the debate on assessment criteria, the positivistic approach generally refers to the logical positivism of the Vienna Circle. This is an empirical approach that, in short, assumes existence of an objective reality, which we can know and understand through research. “Knowledge is grounded in particular observations and can extend to general beliefs only in so far as experience can confirm them” (p. 43). This means that research and its findings have a direct correspondence with the reality out there. Post-positivistic approaches acknowledge that research methods will always be faulty, but researchers still need to do their best to capture reality as accurately as possible by being as objective as possible. In both approaches, research is assessed on correspondence or lack thereof. Approaches subsumed under the heading of interpretivism acknowledge the role of the researcher in meaning-making. These approaches do not assume there is an objective reality out there (or at least not a reality we can objectively know), but that reality is constructed through the interpretations, understandings and experiences of researchers (and all other people) (Hughes, 1980, p. 63). Knowledge of the world is thus always subjective.

There is a tendency to align qualitative research with interpretive epistemological approaches and quantitative research with post-positivistic epistemological approaches, but the picture is not black and white. It is very well possible to do interviews in a positivist way, meaning that the answers given by the participant are seen as accurate reflections of their thoughts, experiences and feelings. The same goes for survey data, which can be used in an interpretivist way, meaning that the results are seen as one way of representing the research topic. In addition, there are many different interpretations of these epistemological approaches. This makes the debate on the relevant assessment criteria no easier. Nonetheless, quantitative methods were, and often still are, judged on the basis of post-positivistic criteria, but these are less applicable to qualitative research. There have been attempts to redefine the criteria so they suit qualitative research, but others, such as Guba and Lincoln (1989), have opted for new criteria altogether.

Despite creative methods having emerged mainly from interpretive epistemologies, it is important to discuss the two sets of criteria for my study. Both inform the reifications of the different communities of practice my participants are engaged in and, as such, can be used in discussions on how to do research. This will become clear in the remainder of the thesis, as both participants and other academics use criteria from either set to say something about the way research should be done.

The main two post-positivist criteria are internal and external reliability. The former

refers to “the degree to which other researchers applying similar constructs would match these to [the] data in the same way as [the] original researchers” (Seale, 1999, p. 140). In that sense, it resembles ‘inter-rater’ reliability (ibid.). According to Seale (1999, p. 148), this can be achieved through using concrete ad verbatim terms to describe concepts, multiple researchers, participant researchers, peer examination and the mechanical recording of data. External reliability refers to the replicability of entire studies (pp. 140-147). “[W]ould other researchers studying the same or similar settings generate the similar findings?” (p. 140). External reliability can be achieved by identifying the researcher’s subject position in the field – note that a different subject position (e.g. gender, ethnicity) in subsequent research might produce different results; identifying who offered the data; identifying the social situations in which the data was offered (in replications, similar participants in similar situations should then provide similar results); giving a full account of the theories and ideas that informed the research, including the coding schemes; giving a detailed account of all the methods used (p. 141).

The interpretivist criteria are those suggested by Guba and Lincoln (1989, pp. 233-250). Their two main criteria are trustworthiness and authenticity. Although, to my knowledge, these have not made it into the qualitative researcher’s vocabulary, they do clearly address some of the ways in which research is supposed to be done. Trustworthiness is then subdivided into credibility, transferability, dependability and confirmability. Credibility means the research account should be believable, something which can be achieved through prolonged engagement, persistent observation, peer debriefing, negative case analysis, progressive subjectivity (which I believe to refer to reflexivity) and member checks (pp. 236-241). Transferability means that the research findings should be transferable to other contexts and cases. This can be achieved through ‘thick description’ (pp. 241-242). The third sub-criteria, dependability, refers to the consistency of the research process. Researchers are required to keep a log of their work in order for them and others to see whether their work has been consistent (p. 242). Finally, confirmability is again related to reflexivity in a more ‘traditional’ sense, as the researcher is required to put aside her personal values and inclinations as much as possible (pp. 242-243). Authenticity comprises different sorts thereof such as fairness (meaning that different realities are represented) (pp. 245-247); ontological authenticity (meaning that more sophisticated understandings of the phenomenon being studied are developed) (p. 248); educative authenticity (meaning that the research has helped respondents to appreciate others’ viewpoint) (pp. 248-249); catalytic authenticity (meaning that the research encourages some form of action) (pp. 249-250); and tactical authenticity

(meaning that the research empower members to act) (p. 250).

The sub-criteria under the heading of trustworthiness are clearly inspired by the way that ethnography should be done. O'Reilly (2005, p. 3, italics added), for instance, provides a minimal definition that states:

ethnography is iterative-inductive research, drawing on a family of methods, *involving sustained contact* with human agents within the contexts of their daily lives (and cultures); watching what happens, listening to what is said, asking questions, and *producing a richly written account* that respects the irreducibility of human experience, that *acknowledges the role of theory as well as the researcher's own role*, and that views humans as part object/part subject.

The way research is 'supposed to be done' is thus shaped by different practices, which are linked to epistemology, the drive for new knowledge and the reward system. As referred to throughout this section, there are continuous debates about these practices. Just like theories and methods, they offer guidelines for how to go about doing research, but it is in their application –during which improvisations are likely made – that the practice takes its specific shape (Ingold & Hallam, 2007, pp. 2-3) For example, the debate on post-positivistic and interpretivist criteria is one in which the reifications are contested because their applicability in practice has become more and more difficult.

Conclusion

In this chapter, I laid out the theoretical framework with which I explain how creative methods are negotiated into qualitative social scientific research practices. I have drawn on practice theory, largely on the concept of communities of practices by Lave and Wenger (1991; Lave, 1991; Wenger 1998) to explain how the practice of conducting research is learnt by students and how it is subsequently maintained over time. I understand this practice as ever on-going and emerging (Ingold, 2010a; 2010b), one in which both change and stability are important. In the practice of research, there is an inherent tension between the *maintenance* of the practice and the production of *new and original* knowledge. On the one hand, in each new individual research, reifications of how research is supposed to be done are appropriated through negotiations and improvisations to fit the individual situation. This makes for small changes in the practice. On the other hand, through boundary-work (Gieryn, 1983; 1995;

1999) and the reward system (Mulkay, 1972), which are both performed through peer review, researchers decide whether or not these changes are in line with the reifications of their community of practice. Power is thus an important element in these practices. However, Lave and Wenger (1991; Lave, 1991; Wenger, 1998) have not paid much attention to the role of power in communities of practice, which is why I discussed Bourdieu's concepts of capital, symbolic power and scientific authority to explain further how power can be exerted and by whom. I also stressed that power does not lie necessarily with elites, but rather that there are various ways to subvert and negotiate power.

Doing research is thus an inherently social activity in which the researchers continuously engage with the other members of their community of practice, the reifications of their communities of practice and the interpretations of those reifications by the other members of the community. In addition, researchers are often members of multiple communities of practice with their own reifications. In such an environment, researchers are thus constantly in engagement with others and their research, making the practice of conducting research ever emerging (Ingold, 2010a; 2010b), improvisational (Ingold & Hallam, 2007) and relational.

The way Hammersely describes the process of producing knowledge fits quite nicely with the framework of communities of practice and my view on the development of knowledge production as an improvisational, rather than an innovative, one. As illustrated in the following quote, it is a process in which the reifications of how research is supposed to be done are constantly reaffirmed and changed.

[I]t is essential to recognise that the process of producing academic knowledge is a collective one. I argue in this book that such knowledge is not discovered or constructed by individual researchers, each working on her or his own, but rather is generated through dialectical processes within research communities: through discussion, both oral and written, that is designed to come to conclusions about what is true, what is false and what is currently uncertain. The work of any individual researcher takes place against this background, and necessarily engages with it.

This collective character of enquiry places additional obligations on researchers, as regards how they present their work, how they respond to criticism and how they treat the work of colleagues (Hammersley, 2011, p. 10).

Taking cues from this, I have begun to show that changes in practices happen through improvisation rather than innovation. Whereas an innovation is a break with the past, an improvisation is connected to it. From my discussion on communities of practice, boundary-work, the reward system and power, I conclude that only small shifts are possible in practices and that change is thus incremental rather than revolutionary. As such, the concept of innovation is not useful. It may even be incorrect in relation to academic practices, in general, and the creative methods of my participants, in particular, because connections are always made to earlier practices. Yet, change is constantly occurring, and it means that slow but profound shifts in epistemologies, methods and knowledge production can happen almost invisibly. Such developments can thus be overlooked if researchers neglect to attend to everyday research practices!

In the remainder of the chapter, I addressed the reifications of doing research. I mainly drew on the textbooks of Bryman (2008) and Seale (1999) to show how social scientific research is 'supposed to be done'. As I pointed out, these reifications are subject to change through negotiations and improvisations done within the social scientific communities of practice. Nonetheless, they help me discuss how my participants conduct their research and how their peers respond to that.

Chapter Three: Methods

Introduction

In this chapter I describe my methodological approach and the specific methods I used to address my research objectives. The main question I sought to answer asked how creative methods are negotiated in social scientific research practice. Three ensuing sub-questions addressed how social scientists negotiate the use of creative methods with other members of their research community; how social scientists negotiate the use of creative methods into their own research practices; and how creative methods emerge in the process. To answer these questions, I took an ethnographic approach in which my participants' experiences, thoughts and practices were made central. I outline this approach in the following sections.

Throughout this chapter I adopt what Hammersley (2011, pp. 25-30) calls the methodology-as-autobiography approach, where the emphasis is more on *how* the methods have been applied by the researcher and the subsequent issues that has entailed, rather than *what* methods have been applied. Hammersley (ibid.) explains this as including more factual information such as what methods have been used, the number of participants and how they were recruited, but this information is not taken to self-evidently explain how the research was done. Methodology-as-autobiography also entails reporting on problems faced in the field, specifically in relation to participants, and how the researcher dealt with those problems (p. 26). Moreover, it shows how the research in practice deviates from the descriptions in textbooks (ibid.) An argument for this approach is thus linked to what I discussed in Chapter Two's *Communities of Practice* section: the discrepancy between the reifications in methods textbooks and the 'actual' practices in the field. Whereas the textbooks can only offer methodological guidelines, a report on conducted research can give insight into how researchers improvise from those textbook reifications. Textbook guidelines are ideals unlike the messiness of the 'actual' research (pp. 26-27). Finally, this approach considers the fact that a researcher is a social being – therefore being part and parcel of the way the research is conducted (pp. 27-28) – and in this way inevitably affects the process. An example of the methodology-as-autobiography approach is found in *Journeys through Ethnography*, edited by Lareau and Schultz (1996). The book's various contributions are all accounts of difficulties researchers had in conducting their studies and how they dealt with them, thus showing how their research was affected by their individual personalities and the messiness of the field.

As part of knowledge production, it is therefore important to address the process of how the research was done. This means paying extra attention to the researcher's academic

background, manner of applying the methods and role in the research. Here, reflexivity is crucial. That topic is one I will discuss before moving on to outline my own academic background. This is followed by an explanation of why I took an ethnographic approach, what I understand it to be and how it relates to my theoretical framework. I then introduce my two groups of participants: experienced researchers and postgraduate research students. While I explain my reasons for working with those groups, I discuss the methods I have used with each of them (in-depth interviews, on-going conversations, filming of presentations, participant observation, gathering of documents), followed by how I approached transcription and analysis. Finally, I devote attention to the ethical considerations that shaped my approach in dealing with the participants and the research more generally.

Reflexivity

Reflexivity is not necessarily a straightforward part of doing research. Moreover, it is not always clear what is meant by it (O'Reilly, 2012, pp. 521-522). An explanation of how I understand it and how it played a role in my research is therefore in order, but first I discuss reflexivity more generally.

One way of thinking about reflexivity is as a way to eliminate bias and increase objectivity. This can be done by critically looking at the researcher's influence on the research and trying to decrease or even eliminate it (Davies, 2008, pp. 4, 11; Hekman, 1986). To compensate for that influence, the research has to be conducted as objectively and consistently as possible. However, as May and Perry (2011, p. 7) note, the "simple separation between knower and known" has been thoroughly critiqued. Reflexivity developed alongside the crises of representation and legitimation, as discussed in the first section of Chapter One, and the literary turn in which questions about power and the authority of research and researchers were important drivers for new theorisation of epistemology and knowledge production. Reflecting on one's research began incorporating the critique that knowledge always came from somewhere and that it is not possible to evaluate knowledge production from some independent position outside of social conventions (Barnes, 1982, p. 50). "[O]bservations are filtered through our own experience", as O'Reilly (2009, p. 191) rightly points out, and "to provide the detached voice of authority" (ibid.) would ignore that. No researcher is completely isolated from what she researches. Even when researchers only observe events without interfering with them, they still exert influence because they produce the research. Without the researcher, there is no research (O'Reilly, 2005, p. 222). Reflexivity

thus acknowledged that the identity of the researcher was paramount in understanding how the researcher understood her research topic, and it became seen as part of good practice (Lumsden, 2013, p. 4).

However, this reflexive turn, as these developments have been called (O'Reilly, 2005, pp. 210-217) also gave reason for concern. Reflexivity sometimes became a way of academically legitimating talking about oneself as it inherently involves "turning back on oneself, a process of self-reference" (Davies, 2008, p. 4). This can be detrimental for research (p. 7) should it wind up becoming an infinite spiral of reflecting on reflections (Pels, 2000) without a specific purpose or goal. Another related problem concerns the questioning of established procedures. May and Perry (2011, p. 38) warn against taking this too far, as it can lead to the radical conclusion that all knowledge is relative. Moreover, reflection takes place from the researcher's position. Still, Davies (2008, p. 4) doubts whether the researcher can see all "the ways in which the products of research are affected by the personnel and process of doing research. These effects are to be found in all phases of the research process from initial selection of topic to final reporting of results". Rose (1997) explains how reflexivity envisioned as a way to situate knowledge is an almost impossible task. The researcher, from her own specific position, reflects on her own research, but it is impossible to know where this position is because it would entail an all-encompassing reflection (Rose, 1997, p. 309). There is a limit to consciously knowing the reasons of what we do and why (May & Perry, 2011, pp. 36-37). Just as reflexivity shows that our knowledge is partial, we must keep in mind that our reflexivity is itself partial; similarly, as our knowledge is situated, so are our reflections. Rose (1997, p. 313) says that "[i]f the process of reflexivity changes what is being reflected upon, then there is no 'transparent' self waiting to be revealed". This makes reflecting on the research process a difficult task. What's more, Adkins (2002) argues that reflexivity actually re-inscribes positions of authority, allowing certain people (i.e. researchers) to speak even more authoritatively because they do so reflexively.

Being reflexive therefore does not automatically rid the research of the problems it was meant to solve, namely of power and positionality of the researcher. In fact, it might even worsen them. This does not mean that academics should abandon reflexivity altogether, but rather realise that just as reflexivity has not made research more objective, it also does not clarify where research or the researcher is situated. The question then becomes about how researchers can move past these issues while taking into account the valuable insights reflexivity offers (Davies, 2008, p. 5). The goal, then, becomes to write oneself *into* the text without writing merely *about* oneself (Fine, Weis, Weseen & Wong, 2003, p. 170).

For me, reflexivity starts with acknowledging that the researcher is an integral part of *the process* of doing research. Research is co-constructed; it is not the researcher exerting a one-way influence on the whole. The research influences the researcher as well. Research is a social process, which is based on certain conventional practices (May & Perry, 2011, p. 38) embedded within “social structures and social interaction” (Glover & Strawbridge, 1985, p. 7). Knowledge is thus socially produced, with May and Perry (2011) arguing that the researcher must reflect on how content, character and context together produce knowledge. This shows reflexivity is not just rational, but rather also: relational, being done in relation to content, character and context; dialogical, being done in dialogue with others, even if only in our heads; and emotional because emotions are part and parcel of how we relate to ourselves and others (Burkitt, 2012, pp. 459, 471). This means reflexivity is an on-going process shaped by the researcher’s thoughts and feelings. Reflecting is done in dialogue with others, academics as well as laypeople, in relation to the context in which researcher and research are located.

And so, to reflect on our role in the research means to look at the research process rather than trying to determine our position from which we see, understand and reflect on the world (Rose, 1997). It requires paying attention to the research’s relationships with others, especially the participants. These on-going research relationships help co-construct the research and its findings. Pink (2007, p. 24) words this nicely:

If the researcher is the channel through which all ethnographic knowledge is produced and represented, then the only way reality and representation can interpenetrate in ethnographic work is through the ethnographer’s textual constructions of ‘ethnographic fictions’. Rather than existing objectively and being accessible and recordable through scientific methods, reality is subjective and is known only as it is experienced by individuals. By focusing on how ethnographic knowledge about how individuals experience reality is produced, through the intersubjectivity between researchers and their research contexts, we may arrive at a closer understanding of the worlds that other people live in. It is not solely the subjectivity of the researcher that may shade his or her understanding of reality, but the relationship between the subjectivities of researcher and informants that produces a negotiated version of reality.

For me, the production of knowledge was inevitably connected to the communities of practice

I participated in and the relationships I built in those communities. Throughout the research process, I read academic literature dealing with the concepts of my thesis and the ways I researched it, which guided my understanding of my research topic and how I produced knowledge about it. This became apparent, for instance, in my transcription and analysis, as I elaborate in this chapter's *Transcription* and *Analysis* sections. Furthermore, this knowledge was produced in collaboration and interaction with my participants in our rational, dialogical and emotional relationships (Burkitt, 2012). In the *Emerging Practices* and *Co-Constructed Knowledge* sections, I give more concrete examples. Finally, while writing this methods section, my supervisors gave me advice about my work. They liked and disliked certain aspects of it, told me to be clearer about a number of concepts and to pursue particular avenues over others. They did the same thing for all of my other chapters. They were not the only ones; my upgrade panel, friends and fellow PhD students gave feedback on several chapters. And even they were not the last to advise. The examiners for my viva had me do corrections in order to pass. This process was clearly rational, relational and dialogical (Burkitt, 2012), and also includes the subjectivity of the reader, who follows up on some things and ignores others, depending on personal interests and background (Gilbert, 1976, pp. 295-296).

More recently, O'Reilly (2012) suggested revisiting the field or doing longitudinal research as a way to enable reflexivity. This is obviously not always possible, but if reflexivity is indeed relational and dialogical (Burkitt, 2012), the processual aspect of research can only be addressed in the way O'Reilly (2012) suggests. In my research, the on-going interviews with the postgraduate research students, about which I say more in the *Interviews and Participant Observation* section, enabled me to 'revisit' certain ideas with my participants and develop my own ideas in dialogue with them.

Reflexivity, I conclude, is as much a part of academic practice as having a critical attitude. It is like a thick description of the research process, which shows the ways in which the researcher engaged with theory, methods, participants and the wider disciplinary and sociocultural circumstances under which she works (Davies, 2008, p. 5). One should not make the mistake that reflexivity (and the admission of making mistakes while conducting the research) makes the research itself better. Rather, it provides the researcher's view on the production of knowledge and demonstrates how her understanding has developed. It is a way to acknowledge the partiality of the research and its limits, even if the researcher cannot comprehend those limits completely herself or show how it has affected her work exactly (Rose, 1997; see also Pels, 2000, pp. 15, 17). It addresses the subjectivity of the work, without

dismissing it as non-scientific. Subjectivity is an inevitable part “of ethnographic knowledge, interpretation and representation” (Pink, 2007, p. 23). In presenting a work in progress and the progress of the work rather than a finished product, I hope to show how my interpretation was socially situated (Spencer, 2007, pp. 445-446). This means that within the rest of the thesis I address how the research and the following conclusions were produced. I do this by focusing on my own background and how it influenced my choice of methods and their application. This is what Hammersley (2011, pp. 25-30) calls the methodology-as-autobiography approach in which *how* the research methods have been applied is as important as *what* methods have been applied.

Personal Academic Background and Improvisational Ethnography

In Chapter Two’s *Communities of Practice* section, I explained how it is within communities of practice that students, researchers and other academics learn, discuss, negotiate and change methods and thereby how to do research (Lave & Wenger, 1991; Wenger, 1998). And it is through researchers’ *practices* that methods emerge. Creative methods are then learnt, discussed, negotiated and changed in those same communities of practice. As such, they emerge into social scientific research practices and become embedded within them (Lave & Wenger, 1991; Wenger, 1998). Creative methods are not isolated from more traditional methods. As I show in the rest of the thesis, they are connected in many different ways, both as methods and methodologically. Just like more traditional methods, these practices in which creative methods are used and emerge are not fixed, but rather are constantly emerging in the doing of the research. Researchers continuously adjust them to the realities of the research in negotiation with other researchers, and so the methods develop over time (Ingold & Hallam, 2007).

This process not only applies to the methods my participants used, but also to my *own* research. That, too, was an emerging practice whereby I negotiated and improvised along the way, as I illustrate in this chapter. Using the methodology-as-autobiography approach, I take into account how I improvised in my own research and methods (Ingold & Hallam, 2007) when starting the research. I based my methods on methods textbooks, readings of other research and my own experience, but it was never possible to repeat exactly what I, or others, had done before, even if I wanted to (Wenger, 1998; Ingold & Hallam, 2007, p. 2; de Kloet, 2014). To give the reader a sense of how this practice emerged, the next section describes my academic background and understanding of research methods in the social sciences. In

addition, this works to clarify how I understood the practices of my participants (Fabian, 2008, p. 8) as well as how knowledge was produced in this study (Pink, 2007, p. 24).

Personal Academic Background

I was first introduced to social scientific methods when I studied communication science at the University of Amsterdam. This was a more mainstream post-positivist communication science department in which quantitative research was the standard. We were mostly taught statistical methods. The qualitative methods were more descriptive and exploratory, often applied as a first stage towards a quantitative approach using surveys and experiments. I was never very enamoured of this approach, and so chose modules which focused on qualitative and cultural studies approaches. Later on, in my research Master's programme, I had more extensive practical training in conducting interviews and focus groups as well as the analysis of qualitative textual material, mostly from a grounded theory perspective. There was less emphasis on participant observation and visual analysis, which was more theoretical than practical. In addition, the feedback I got on assignments using the latter approaches was less positive than on my interview and grounded theory analysis assignments. I consequently felt more comfortable applying those approaches in research. Knowing better what I was doing, I felt they provided me a more systematic way of conducting research.

During the research Master's, I first encountered creative methods through a mailing list on qualitative research methods. These methods were quite different to those from my own academic education. They intrigued me because although I could see how creative methods could provide or convey interesting insights into certain subjects, I could not align them with my own research practices – that which I had been taught was 'proper' research. This raised a whole set of questions for me such as: What kind of knowledge do these methods produce? How do they shape our understanding of the research subject? How is this different from the methods I already know? How could 'new' methods be developed in an academic context when doing research was prescribed by certain methods? Is this still academic research – and what was academic research anyway? Writing my Master's dissertation on these methods was a first step in answering some of those questions.

In my direct surroundings, I was the only one working on these methods; in my community of practice, I was considered an expert on them. Qualitative methods were already seen as 'leftfield' by most of my fellow academics in the communication science department; creative methods were even further afield. This became evident in conversations with my

colleagues. They did not always understand what I was doing or why. In one particular conversation, the head of the school in which I did my research Master's told me that I did not have a subject for my dissertation, and then added: "Well, you do. But, you know what I mean." His research used surveys and experiments to see whether people's perceptions about Europe changed according to different frames used by news media. By comparison, my research was seen as vague and abstract. I understood why he thought methods were not a proper subject. To me, however, it showed a lack of understanding that methods are not a given, and that they are more than just means to conduct research. I realised more and more how they shaped the research and how they needed to be adjusted for each project. Methods were thus neither objective nor systematic means to do research. At the same time, I found it hard to break away from the practices I had learnt. There was still a voice in my head, telling me what was and was not proper academic research. This personal tension inspired me to try to understand how methods were used in practice because this was something that changed research practices as well as what was considered academic knowledge. That curiosity led me to pursue a PhD on creative methods at Loughborough University.

In England, reactions to my PhD research were quite different. Being part of a new community of practice now, I no longer had to defend why I was using qualitative methods or researching creative methods. Working with professors Karen O'Reilly and Sarah Pink during my PhD impacted my understanding of methods within the social sciences. Both scholars used a reflexive approach; my approach was almost post-positivist. Additionally, they worked with ethnographic methods, and Pink was especially inclined to use creative and sensory methods. Their work encouraged me to be more reflexive about my own research, considering my role in shaping it. It also prompted me to do participant observation and film my participants' presentations, neither of which I had ever done before, thus leading me to negotiations in my research practice. Moreover, I contemplated using creative methods myself. However, because the PhD research was such an enormous undertaking in which many things would be new to me, I felt more comfortable using at least one method I was familiar with: interviewing. Yet, as explained in the *Interviews, Filming and Participant Observation* and *Analysis* sections, the interviews provided different data than I was used to, thereby requiring me to improvise from my practices as well. In hindsight, this may have been more perplexing because I did not expect it.

To sum up, my approach to researching creative methods was first informed by the research practices I had learnt in a quantitative, post-positivist-orientated department. Influenced by my supervisors and a new community of practice, I slowly adopted a more

reflexive approach in my practice, which I am developing to this day. This informed how I understood my participants' creative research practices. At the same time, my understanding was influenced by how my participants understood and practised creative methods. Such views were exchanged in the interviews with my participants, eventually amounting to a multi-layered understanding of what creative methods entailed. I discuss this process more extensively in the *Co-constructed Knowledge* section.

Improvisational Ethnography

So far, I have provided the context for the start of my PhD and my own process of improvisation in conducting the research. This began with the research topic. In the previous section, I noted my interest in creative methods and how they fit in with and changed the social sciences. This was informed by both the apprehension and the enthusiasm I felt for these methods and how they differed from the social scientific practices to which I was accustomed. In my first year report, I had formulated the following research question: How do creative qualitative social scientific research methods develop, and how do they change the field of social scientific research? This was a broad question, but from it, I started to develop my particular approach.

The question demanded I take a close, prolonged look at practices and experiences of people who used creative methods – an ethnographic approach. I use the term 'ethnographic approach' rather than 'ethnography' because the latter's meaning has become quite varied. Ethnography has never been a homogeneous set of methods or methodological approaches, but its heterogeneity has become more obvious over the last decades, for instance, through scrutinising the concept of 'the field'. I choose to acknowledge ethnography differently by showing how my research was inspired in its approach and methods by ethnography. As such, I lean towards Pink's (2002, p. 22) understanding of ethnography as a methodology, described as:

an approach to experiencing, interpreting and representing culture and society that informs and is informed by sets of different disciplinary agendas and theoretical principles. Rather than a method for the collection of data, ethnography is a process of creating and representing knowledge (about society, culture and individuals) that is based on ethnographers' own experiences. It does not claim to produce an objective or truthful account of reality, but should aim to offer versions of ethnographers'

experiences of reality that are as loyal as possible to the context, negotiations and intersubjectivities through which the knowledge was produced.

In this approach of ethnography as methodology, I do also look at how ethnography has generally been understood. Ethnography means ‘writing people’ (O’Reilly, 2009, p. 227; Mitchell, 2010, p. 2), which refers to the dissemination aspect of research, usually the monograph. However, it is often interchangeably used as a term for the methods used in this type of research. Pope (2013) says: “[w]e use the term ethnography as a short hand to denote a collection of methods – observation, participation, discussion, analysis of documents and self-reflection (and sometimes quantitative methods)”. O’Reilly (2005) and Mitchell (2010) have similar descriptions in which time, immersion and different methods play an important role. For O’Reilly (2005, p. 3), ethnography has a minimal critical definition in which ethnography is at least considered an

iterative-inductive research (that evolves in design through the study), drawing on a family of methods, involving direct and sustained contact with human agents within the contexts of their daily lives (and cultures); watching what happens, listening to what is said, asking questions, and producing a richly written account that respects the irreducibility of human experience, that acknowledges the role of theory as well as the researcher’s own role, and that views humans as part object/part subject.

Ethnography is thus an approach in which a variety of mostly qualitative methods is used over a prolonged period of time to understand the daily lives of a community. The researcher has a pivotal role in the methods and the how the daily life of the community is understood. She produces a detailed and rich interpretation of the daily lives of participants in the community. However, as already mentioned, the way ethnography has been applied in practice has changed because the field is no longer understood as a confined space or place. Also, in my research there was no field as such because applying creative methods was not attached to specific places and times. I therefore need to address this issue further.

Despite the variety of methods encompassed within an ethnography, participant observation is *traditionally* seen as the most important one in doing fieldwork (Mitchell, 2010, p. 2; Walsh, 2000, p. 217). However, in recent decades, the concept of the field has been heavily scrutinised and new ways of conceptualising the concept have emerged (Amit, 2000; Gupta & Ferguson, 1997; Marcus, 1995; Melhuus, Mitchell & Wulff, 2010). This has

implications for the way ethnographic fieldwork can be done. Subsequently, these new conceptualisations of the field seriously challenge the assumption of participant observation as the main method of ethnography.

What constitutes ‘the’ field has become less evident. Although the field was never a confined space, developments such as globalisation and migration demonstrated that communities of people are not bound to specific locales. Moreover, these communities, and the individuals therein, are connected in various ways to others all over the world. This has led to a deconstruction of the theoretical and empirical notions of what a field is (Amit, 2000; Gupta & Ferguson, 1997; Marcus, 1995). The field in which communities live their lives is almost always multi-sited (Marcus, 1995), and in recent decades some communities have become virtual (Hine, 2000; Kozinets, 2010). As a result, doing participant observation has evolved. It is certainly still commendable to do long-term participant observation, but it is not always feasible. More importantly, it is not always helpful in providing knowledge about participants, topics and ‘fields’ that are not all located in one place (e.g. Hannerz, 2003; Knowles, 2011). This means the work of the researcher “involves following processes in motion, rather than units *in situ*” (Mitchell, 2010, p. 7, italics in the original; see also Marcus, 1995, pp. 106-110). Researchers must make use of other methods or apply familiar ones differently.¹⁸ Short repeated trips to the field and use of shorter-term research methods such as interviews and life histories became more prevalent than doing long-term participant observation (Mitchell, 2010, pp. 7-8).

My research, too, lacked a clearly bounded space, as I discuss in more detail below. Moreover, the activities relating to creative methods did not always happen at set times in specific places. This meant I could rely less on participant observation to conduct my research, and had to use other methods more. Doing fieldwork, then, became a flexible approach in which “a willingness to respond to the constraints and possibilities of the field” (Mitchell, 2010, p. 5) was much more important; there was no longer a clearly defined field (if there ever was), where it was obvious which activities the researcher needed to do and people she needed to interview. This showed the inevitable improvisational aspects that ethnography and qualitative research more generally possess (see also O’Reilly, 2009, pp. 14-16).

Continuing my own journey in the PhD meant I had to think about where I could find how creative methods evolved and were used. When my supervisors asked me where I could

18 The latter is an example of improvisation following the reconceptualisation of the field.

find 'innovation' (at that time my supervisors and I still used this term) I realised there would be many different locations, non-virtual and virtual, to which I could go. The question thus became: Where I should go to, when and why? In other words, where and what was the field? This also required me to define more explicitly what creative methods were. Based on an earlier literature study for my Master's dissertation (van Romondt Vis, 2007), I narrowed it down to methods that used visual lens-based arts, poetry, performance and narrative. There were other creative methods such as quilting (Ball, 2008; Rippin, 2003), dancing (Blumenfeld-Jones, 1995; 2008) and working in Lego (Gauntlett, 2007), but in my literature study (van Romondt Vis, 2007) I found the lens-based arts, poetry, performance and narrative ones were more widely used.

Here, the construction of the field was an interaction between me, the literature, my supervisors and the absence of any one specific place I could go to. This meant my field was constructed in collaboration with the literature, my own academic background, my supervisors and my potential participants. The challenge of constructing the field is not uncommon. In research on Ireland's emergent biotechnology industry, Desmond (2004), for instance, describes the difficulties of getting to grips with a field where the participants were all over the country rather than in a specific location. Desmond (p. 263) also points out that it is about *when* the field is there and that these moments are related to earlier moments: "the space of action was relational in that it only existed in the context of and in association with other spaces and times". In the following excerpt she explains how it was not an existing field with clearly demarcated sectors, but one she herself constructed – the field always being in relation to herself.

Here again the construction of boundaries between the economic, scientific or state sectors seemed entirely artificial as they merged in and out of each other in confusing messiness, leading me at times to wonder where exactly was the field, only to find myself smack in the middle of it. Thus it became apparent that the research itself had also positioned me within the process, dismantling any preconceived notions of insider/outsider as 'I am always, everywhere in the field' (Katz 1994, 72). (...) The field was not some distinct, objectified space, but was rather where I was situated. (Desmond, 2004, p. 263)

For me, similar issues arose, where the main research object was the negotiation of creative methods. That negotiation was happening where researchers were employing these methods.

It thus became clear that I was going to look at the practices of people using creative methods, though these practices were employed by many different people in different places, and they were not necessarily members of the same research group. So where would I find these people? Another related issue Beaulieu (2010) points out is that in the social sciences as well as the humanities, researchers often work in more solitary circumstances than those in the natural sciences. Their work is not necessarily located in a specific location as it is for natural scientists in a lab. Beaulieu (2010, p. 456) calls this “intimate science”. She does not mean to say there are no particular places for humanities or social science scholars or that social interaction does not exist between them, but she quite rightly asks: “[W]here should one look for the *field*?” (ibid. italics added). To that I would add, more importantly, *when* is the field there? In my research, participants would read and think about their methods and subsequently write about them, but it was not known beforehand where or when my participants would engage in these particular activities. They could do this at the office or in a coffeeshop, during the day or late at night.

Beaulieu (2010) proposes the notion of *co-presence*, which considers ways of establishing a research relationship other than through being in the same location. To establish co-presence, one builds relationships with participants, which is possible in any kind of setting. Co-presence can be achieved through multiple modes. One such way is by being in the same physical location while still having social interaction through mediation (such as e-mail) and written texts (pp. 454, 458). This is inevitably a multi-sited approach (Marcus, 1995, p. 96), in which the multi-sited ethnography

moves out from the single sites and local situations of conventional ethnographic research designs to examine the circulation of cultural meanings, objects, and identities in diffuse time-space. This mode defines for itself an object of study that cannot be accounted for ethnographically by remaining focused on a single site of intensive investigation.

So there is necessarily a selection of sites and situations to which the researcher attends. In a way, this is no different from ‘traditional’ ethnographic research, because there, too, situations are included and excluded from the research (Hannerz, 2003, p. 207), though here the selection has become more visible.

Because I wanted to know how creative methods were taken up in the social sciences, the first criterion for inclusion was that not only participants, but also seminars, conferences,

websites and mailing lists, were in the realm of academia. Quite simply this meant that participants and their forums were in some way connected to a university or another academic body, such as the Economic and Social Research Council (ESRC) or the National Centre for Research Methods (NCRM). When it came to websites or mailing lists, the bulk of the contributors had to be academics. In addition, the creative methods involved had to be “a mode and form of qualitative research in the social sciences that is influenced by, but not based in, the arts broadly conceived” (Cole & Knowles, 2008, p. 59). More specifically, my participants had to work with visual lens-based arts, poetry, performance or creative writing. The main focus was on the researchers using creative methods, about whom I say more in the following section *Participants*, but the events, websites and mailing lists provided a wider background in which to situate the practices of my participants.

In conclusion, this research project required me to take a close, prolonged look at the practices and experiences of researchers using creative methods. Doing this invariably demanded an ethnographic approach (Pink, 2007, p. 22). The beginning of my research consisted of finding a field, which depended on defining more specifically what I meant by creative methods in this particular research project. While my focus became sharper, I also started realising that a site where I could find creative methods did not exist in the traditional sense (e.g. Amit, 2000; Gupta & Ferguson, 1997; Marcus, 1995). So rather than doing participant observation at one specific site, I had to establish co-presence (Beaulieu, 2010) with my participants, which could entail participant observation as well as social interaction through mediation and texts. This resulted in the production of various data through a combination of interviews, participant observation, filming, informal talks and gathering of documents such as websites and mailing list messages. In the next two sections I introduce the participants with whom I produced the data and say more about the process of data production.

Participants

To look at the development of creative methods, I worked with two groups of participants: postgraduate research students and experienced researchers. In this section, I first briefly describe the two groups and explain my reasons for working with them. Then, in the final part, I introduce all individual participants, their specific backgrounds and institutional context.

Being interested in the process of how researchers negotiated creative methods and

how creative methods thereby emerged, I had to work with people who were or had been in that very process. The group of five postgraduate research students I worked with were in the middle of the process. I met three at events relating to creative methods; two others replied to an email I sent out.¹⁹ They had been working with these methods for a prolonged period and had to make decisions concerning the methodological process of their research. This provided a wealth of information. I saw the process in action, rather than in retrospect. Some of the postgraduate research students were new to creative methods; others already had experience with them. This provided different perspectives on how they implemented the methods in their practice. In addition, there was a more practical reason to focus most of my attention on the postgraduate research students: they simply had more time to talk to me on numerous occasions. For them, it was an opportunity to discuss ideas with someone other than their supervisors, about which I say more in the *Interviews and Participant Observation* section. From June 2011 until July 2012, I had on-going interviews with this group, mostly lasting for an hour, but sometimes up to two.²⁰ In these interviews, I asked about academic background and how the participant had come to use creative methods – I call this ‘historical background’. In addition, I asked about how they were currently using the methods, the issues they were having and their plans for the next steps in the research process – I call this ‘current affairs’. As time went on, the interviews became more and more focused on current affairs and our exchange of views than on historical background.²¹ Besides interviews, I filmed one presentation by each participant in which he or she used or reported on creative methods. I also read some, if not all, of their work and working material. In the *Interviews and Filming and Participant Observation* sections, I say more about the materials that the participants and I produced.

The other group consisted of eight experienced researchers. By that I mean social scientists who had completed their PhDs and worked with and written extensively about one or more creative methods. They had diverse positions within their universities, spanning from professors to an academic-related post in which the main remit was researchers’ professional development. This group of experienced researchers gave me another perspective, having worked with creative methods for a longer time, sometimes in different departments or universities. They therefore knew more about the methods in relation to departmental policies and academic trends, which were the reifications affecting how practices took shape. They

¹⁹ For the email, see appendix I.

²⁰ For a full overview of the interviews, see appendix II.

²¹ For an example of a topic guide, see appendix III.

were all happy to talk to me, but their schedules were generally busier, which meant it was harder to meet with them. As such, I had only one conversation with each of them and read their publications on creative methods. During the interview, I asked them about their academic background; how they started using these methods; how their practices had changed; how their work had been and was being perceived; their position within the university; and what they saw as the opportunities and challenges for their way of working. The conversations lasted between a half-hour and two-and-a-half hours and I recorded them.²²

Although the conversations with both groups informed my analysis tremendously, the majority of my material consisted of the conversations and participant observation I did with the postgraduate research participants. As a consequence, most of my analysis is based on material from them. I added material from the interviews and analysis from the experienced researchers' group when it was suitable, and otherwise drew on observations and casual conversations.

Having explained how and why I worked with these two groups and the material that was produced, I now introduce the participants. The postgraduate research students group comprised Louise, Richard, Liza, Hilde and Paul. I introduce them first and more elaborately than the experienced researchers because my collaboration with them was more intense and prolonged; plus, their material provided the main part of my analysis. The experienced researchers were Sophie, Matthew, Mary, Adrienne, William, Bart, Ally and Marjory. All names are pseudonyms and in the *On-going Ethical Considerations* section I explain why I chose not to use their real names.

The PhD students

Louise

Louise's PhD project was on gender and embodiment of young adolescents, and she used photography in her project.²³ She had a background in gender studies and did her PhD in the sociology of sports research group within a larger sports science department. This research group mainly used qualitative methods, though the department was quantitatively oriented. When Louise started the project, she had no experience with visual methods, but at the beginning of her PhD she was a research assistant to one of her supervisors on a project using

²² For a full overview of the interviews, see appendix II.

²³ These introductions only mention the creative methods the participants used. I omit the other methods because they were not the focus of my research. If relevant, these are mentioned in the result chapters.

photo-elicitation. Her supervisor had used photo-elicitation quite extensively. Louise wanted to take a more ethnographic approach in using visual methods and was negotiating her own ideas with those of her supervisor and the department. When I spoke to Louise for my research, she was in the analysis and writing up phase.²⁴ By the time our conversations ended, she had moved into her first job at university.

Richard

Richard's academic positioning was similarly located to Louise's. He did his research within a smaller qualitative research group, situated within a more quantitative sports science department. For his PhD, he researched young adolescents' thoughts on and experiences with physical activity and health. He used photography and film in his project and had hoped to use his findings as the basis for other forms of dissemination. He wanted, for instance, to organise a physical activity and health awareness week with his participants. He also wanted to make posters based on his findings that resembled health campaigns. He took steps towards executing these ideas, but time constraints and the PhD's workload deterred him from finishing them. He had some experience with writing creative non-fiction in collaboration with a fellow student under the supervision of two professors during his Master's. At first, he was not particularly enamoured of them, but during the class and the subsequent process of writing an article about the research done in that class, he could relate more to them. Richard was keen on epistemology and methodology, and during his PhD was part of several discussion and research interest groups related to methods and methodology. One of those was focused on visual, narrative and digital methods, in particular. When I started talking to him, Richard was just about to begin his data collection. When our conversations ended, he was finishing that phase.

Liza

Liza was doing her PhD in a human geography department. It was very much a mixed department, where people were working in qualitative and quantitative approaches. She was working on young people and disability in the global South. She used photography, film and written diaries. She had also wanted to produce a performance with her participants based on her findings, but lack of time and the large number of methods she used made it too

²⁴ Seeing as I had many more conversations with all of the participants after I stopped gathering materials with them, I only refer here to the conversations we had specifically for the purpose of my research.

complicated. Liza had done interviews before, but had no prior experience with creative methods. While she and her supervisor discussed possible methods, her supervisor mentioned the research of a former PhD student who had worked on a similar topic in the same country as Liza and who had used creative methods, amongst others. Liza looked to her work for inspiration. Liza was a few weeks into her data collection and had started her analysis and writing up during our conversations.

Hilde

Hilde was also based in a geography department, but this one was at the faculty of science. Her research concerned geopolitical issues and the experiences of local people with these issues. She made participatory documentaries with the local people for her PhD project and worked collaboratively with a research team consisting of another researcher, a cinematographer and a plastic artist. Hilde was from Latin America, where she found that the use of participatory action research was much more part of academic practice. In her Master's, she had used photography, and after getting that degree, she worked for governmental organisations on environmental issues, where she used the participatory action approach for research purposes. During that time she also became part of a social and political collective which worked on socio-environmental issues. Each member used his or her expertise for approaching the issues. It was partly from this collaboration that her decision to pursue a PhD stemmed. She said having a PhD would give her a more powerful, authoritative voice. She was passionate about the collective as well as the participatory action research approach. Hilde was in between two of the participatory projects when we first spoke. She was at the end of writing up when we had our last conversation.

Paul

Paul's PhD thesis was an ethnography about the dramatic narratives of athletes. He did participant observation using film, performance and narrative in his research. He was based in a drama department, but had done his undergraduate and postgraduate degrees in both performance studies and sports sociology. For his postgraduate degree, the final project had to be a multimedia portfolio consisting of text, video and web-based elements, which had to be made entirely by the student. As a result, Paul clearly had some experience with creative methods and was situated in a department where such an approach was considered normal. He also set up a creative methods network for peer learning. Our conversations started when Paul

was in the middle of his data collection. They ended a couple of months before the submission of his PhD thesis.

The Experienced Researchers

Sophie

Sophie was the first experienced researcher I interviewed. She was a professor in sociology and did not have a background in visual methods, though started working with photography when a particular project demanded it of her. Because she did not have a background in photography, she often worked with others who did. She taught visual methods and worked in a department that was supportive of this approach and enthusiastic about creative methods overall.

Matthew

The second experienced researcher I spoke to was Matthew, who had a background in both visual anthropology and visual arts. He was a lecturer in visual anthropology at a university, but often worked for art galleries and local communities as well. In his research and teaching, he was interested in the connections between the academic and the art worlds. Like Sophie, Matthew worked in a department supportive of his interests.

Mary

Mary worked as an associate professor in a performance and education department. Her own education had also been in these two fields, and she had been an English teacher for a few years. She had first encountered poetry and performance as ways to convey research during her graduate degree, and in her ensuing career had often worked with well-known academics in this field. Afterwards, she continued to work in departments with a focus on performance and education, where she used performance and poetry as a way to educate.

Adrienne

Adrienne used narrative in her work on patient experiences with healthcare. Before she started her PhD, she had worked as a counsellor and had written about her own experiences with illness. In her research for the PhD, she was drawn to a narrative approach because she could

show her participants as humans, not just as patients. At the time of our interview, she was a research fellow at a school of education and continued to work with narrative approaches in qualitative health research. This was the school where she had done her PhD.

William and Bart

William, at the time of our interview, worked in a researchers' professional development department of a university. He had written a collaborative PhD, together with Bart, in which they explored writing as a research method. Both in the PhD and afterwards, they used different writing styles such as poetry and fiction, and often performed their work at conferences. Because William was not in an academic post, he was not expected to do academic work. This gave him freedom to write and present in this way. Bart, on the other hand, worked as a lecturer in an education department, where his approach was quite different from his colleagues. As he said in the interview: "I tend to be viewed much more askance." The fact that he got published gave him the freedom to continue this kind of work.

Ally

Ally was a research fellow at the time of our interview, working as an anthropologist in an arts and design department of a university. She was both an artist and anthropologist by training, but felt more like an anthropologist. Still, she would often collaborate with artists in her research work. Like Matthew, she was interested in the connections between anthropology and art. In the department where she was based, she taught the basics of qualitative research.

Marjory

Marjory was a professor in a school of social sciences. She worked with different kinds of creative methods, including photography, film, and narrative. Although she had a background in both arts and sociology, Marjory would often collaborate with artists as well as other researchers in participatory research projects whereby the participants played a very important role in developing the research. These projects had been quite successful in academic terms as well as for the communities Marjory worked with, but it was only then, at the time of our interview, that she felt supported by the school she was in.

These two groups of participants came from a wide variety of disciplines within the social

sciences: sociology, anthropology, education, sports sociology, health and human geography. Some had a background in the arts and moved into the social sciences, whereas others were initially trained as social scientists and took up creative methods later. They used different creative methods. They were in different stages of their careers and based in different kinds of departments, some more supportive than others. As methods are never uniformly taken up, this variety of participants helped me get a sense of the different ways in which the creative methods I looked at – visual lens-based arts, poetry, performance and narrative – were negotiated in relation to the social sciences. How I looked at these negotiations is the subject of the next section.

Interviews

As stated, the main part of my material consisted of interviews, both one-off and on-going. There are many types of interviews, according to Wolcott (2008, p. 47), and they can encompass anything “from casual conversation to the formal structured interview”. One is not necessarily better than the other. The choice for a specific type of interview depends on the setting in which the research takes place and the kind of knowledge the researcher seeks. Wolcott therefore promotes distinguishing between the different types of interviews to get a better sense of how and when certain ways of interviewing can take place (pp. 54-55). Heyl (2007, p. 369) distinguishes ethnographic interviews from other types as being when the

researchers have established respectful, on-going relationships with their interviewees, including rapport for there to be a genuine exchange of views and enough time and openness in the interviews for the interviewees to explore purposefully with the researcher the meanings they place on events in their worlds.

Time and quality are thus important. My research involved one-off in-depth interviews with the experienced participants and on-going in-depth conversations with the postgraduate research participants, but there were also casual conversations with people at conferences and seminars. Clearly, those conversations were not ethnographic interviews, but they did give valuable context information.

The one-off interviews with the experienced researchers were in-depth but not ethnographic. Lasting between a half-hour and two-and-a-half-hours,²⁵ they provided enough

²⁵ For a full overview of the interviews, see appendix II.

time to gain insight into my participants' academic biographies, experiences with working on particular creative methods in an academic environment and expert views on how the methods were developing. In those interviews, the experienced researchers would often ask about me, my PhD research, my background in and thoughts on creative methods. In that sense, the interviews were like conversations, as Mary one of my participants, noted. Before and after the interviews, I had brief exchanges with the experienced researchers, mostly over email, about the interviews, the questions I was going to ask and ethics. Later on, I also ran into some of my participants at conferences, where we continued discussing their approaches. However, these interactions were not of a sufficiently structural or on-going nature to count as true ethnographic interviews.

By contrast, the on-going interviews with my postgraduate research participants could be labelled as ethnographic interviews. My questions prodded them into certain areas and brought to light the different avenues they could take. Often, though, my participants would ask for my opinion on a certain methodological issue, with the interviews providing an opportunity to explore their thoughts on a particular issue. This steered the conversation in a way that benefited them but disrupted the course I had in mind. This was not always a clear-cut process, as I further explain in the *On-going Ethical Considerations* and *Emerging Practices Negotiating Knowledge, Ethics and Power* sections, giving examples of how the research was co-constructed. Nonetheless, it is exemplary of the interactions between my participants and me, whereby we exchanged views and thoughts and they, along the way, supported each others' research.

The postgraduate research participants all being at differing stages in their projects allowed me to see different parts of the research process. As mentioned, during our conversations we talked about what was currently going on in their research – the so-called current affairs – how their research had developed since we had last spoken, what issues they were dealing with at that time, what choices they had to make or difficulties they encountered and how they dealt with them. In the earlier conversations, especially, I would inquire more into historical background knowledge. This entailed questions concerning how they came to be doing this kind of research and their ideas about epistemology, research and the social sciences. For both types of questions, the wider context – involving their relationship with their supervisor and the institution where they were doing their research – was of importance as well. Although the interviews were unstructured in the sense that we addressed anything interesting that came up, I did make a new topic list each time on the basis of my on-going literature review and my previous interview(s) with the

specific participant; this was a way to follow up on prior issues or events that would have since taken place, such as any interviews or presentations they did.²⁶

Besides holding conversations with both groups, I read work of theirs that dealt with or used creative methods. For the five post-graduate participants, in particular, reading parts of theses and other works proved integral to our exchange of views and the co-construction of knowledge. However, it also provided insight into how their work was developing in the period that our interviews took place (June 2011 to July 2012). For instance, I read and gave feedback on Louise's methods chapter for her thesis. After I went through her presentation, Hilde changed some of the slides' order and information according to observations I shared.

What I had not anticipated in the on-going interviews with the postgraduate research participants was the difficulty I had in discussing certain aspects of their methods. They considered some things so normal that they would not mention them or, if they did, it was done so very matter-of-factly. Liza, for example, would say: "I made a topic list," or "I did three interviews – they went well," and then move on to the next topic. I tried to encourage her to elaborate, but that often fell flat. Over time, interviews became monotonous (Atkinson, 2013), something I dreaded very much. It seemed there was nothing to talk about and I felt I was gathering no new data. My participants asked me whether the information they were imparting was useful. I said it was though was not really sure. I was also afraid this meant they sometimes found the interviews boring and wanted to get them over with. It was only during analysis that I realised the lack of elaboration on these aspects of their methods was actually interesting, as it gave me insight into which practices they had already internalised and those they had not. Again, this is an example of how the research was co-constructed. The participants disrupted the way I had imagined the conversation would go.

Filming and Participant Observation

I also filmed one of each postgraduate research participant's presentations and tried to engage with the practices through participant observation. First, I chose which presentation to film, based on the topic. If the participant indicated that the presentation would not entail any creative methods, I would not attend. Second, my choice rested on logistics, asking myself: Could I afford to attend the presentation in terms of money, time and travel distance? Some conferences took place in Australia and Singapore, where it would not have been feasible for me to go. I filmed the presentations with a Handycam, which is a small, simple handheld

26 For an example of a topic guide, see appendix III.

video recorder with only a few functions: record, stop, play, zoom in and out. It also has a small screen on which the moving image can be seen during filming and played back afterwards. The video recorder was easy to use and carry around. Moreover, it was not intrusive. In filming, I alternated between focusing on my participant and the wider setting including the screen with the presentation. Paul's presentation was an exception, where I solely focused on him. He had asked me not to film the images he showed because his participants were visible in them and they had not given permission for a third party to use the material. I used the camera as I used a voice recorder for the interviews, as a way to document what was said and done by my participants during their presentations and the subsequent questions and answers. The recordings served as field notes, not documents to be disseminated. Despite their similarity to field notes and the recordings of a voice recorder, they did raise a whole new set of questions. As Grimshaw (2001, p. 3) explains:

Images by their very nature establish a different relationship between the ethnographer and the world she or he explores. Moreover, image-based technologies mediate different kinds of relationships between ethnographers, subjects and audiences than those associated with the production of literary texts. For instance, students quickly discover that working with a video camera makes them visible, publicly accountable and dependent upon forging new kinds of ethnographic collaborations.

The last point was exactly how I felt when I was filming my participants. Whereas before I was just a member of the audience, now I was in a different position. Filming explicitly engaged the other people who were present. They might have wondered what was being filmed and why. More importantly, they might have wondered whether they themselves were being filmed. It is good practice to ask an event's organisers for permission to film, and I did so even though I was afraid it would not be granted to me. There are lively discussions about the ethics of the visual in the social sciences (Beddall-Hill, 2011; Boxall & Ralph, 2009; Prosser, Clark & Wiles, 2008; Wiles, Coffey, Robison & Heath, 2010; Wiles, Coffey, Robison & Prosser, 2012). I was therefore slightly surprised when it was not at all difficult to get permission from the organisers and session conveners or when others attending the sessions did not raise questions. It must be said, though, that the organisers did not always announce to the attendees that I was going to film. Yet, these were all sessions with quite small audiences, where I believe I was easily accessible to inquiring minds, although no one

did approach me.

Engaging with the PhD students' practices through participant observation was harder. Although I strived to be present at their practices, I was only marginally successful. It was not always possible to do participant observation when the PhD students gathered and analysed their data; some had already done the gathering or were abroad, amongst other reasons. There were ethical issues to take into consideration, too, such as the anonymity of my participants' participants. All of them were working with their own participants. Some of my participants had worked hard at building rapport, which potentially could have been harmed by my presence. Another issue was their knowledge production. My presence could change that quite significantly. Then there were the issues pointed out by Desmond (2004) and Beaulieu (2010), asking: Where is the field? And when is it there? Thinking through the research and analysing it was something that could be done in conversations my participants had with fellow researchers, such as supervisors and peers (me, for example). Such conversations are invaluable to furthering thinking, but analysis also happens in solitary moments, for example, while sitting at a desk in front of a computer, or randomly, while in bed or on the bus. Often, ideas enter our heads when we are not consciously inviting them in.²⁷ There is no fixed time or space where this happens. It depends entirely on the researcher. This reality also made it hard for me to be present for my participants' analyses.

In instances when I was present, however, there was another challenge to observing my participants' practices. These practices were not always visible in the sense that the way the methods were applied looked no different from other methods. Let me elaborate on this more by recounting the experience I had with Richard while he was making posters based on his data. These posters were fashioned after health campaign posters, inducing the onlooker to think about and possibly change their views on and behaviour towards certain health issues. Richard sat in front of a computer with a notebook. He changed a word here and there in the text on the screen. He looked at it and then at his notebook. His actions resembled the writing of a paper as I had seen my fellow students do in the computer lab or in the office. There were times he was just sitting with his notebook on his lap, thinking. He would open documents on the computer but not do anything with them. He would go back and forth between different windows. I was unsure of what to film. Should I have filmed him and what he did? Or, should I have filmed what was happening on the screen because his actions had an effect on what was happening there. I asked him what he was doing, but it felt as though I was disturbing his

²⁷ As a personal example, I have kept my phone next to me in bed so I can record thoughts whenever they come up.

thinking process. I was filming him, but there was nothing much to see. Of course there was a lot to observe – Richard, the computer, his notebook, the room we were in and much more – but not a lot was happening. I was not capturing creative methods in the way I thought I would. I had expected to see something exciting. My first thought was thus similar to what Hannerz (2003, p. 211) has expressed:

There are surely a great many activities where it is worthwhile to be immediately present, even actively engaged, but also others which may be monotonous, isolated, and difficult to access. What do you do when ‘your people’ spend hours alone at a desk, perhaps concentrating on a computer screen?

As in the interviews with Liza, I did not feel I was gathering any data. I was gathering something, but not the data I had expected. I thought Richard would have been able to say more about what he was doing. And perhaps naively, I thought that watching his movements at the computer and their resulting actions on the screen would simply show me how he ‘translated’ the findings of his research into the messages on these posters. Later in my actual analysis and writing up phase, I read Löfgren’s (2014, p. 73) article on mundane research routines and started realising how this seemingly uneventful moment was actually really interesting – it showed how these creative methods are embedded in more traditional and familiar academic practices, such as sitting at a computer and starting to write. These taken-for-granted moments actually show the skills academics have of ‘knowing how’ to write a paper or how to analyse data. The initial invisibility of the skills Richard was using to create the posters showed me how they were part of the academic’s toolbox. Unlike the skills of researchers in the lab or craftsmen in the workshop, these skills are less tactile and thus less visible. According to Löfgren, this is a reason academic skills have mostly been researched in the context of the lab, where visible activities take place (p. 74). In addition, the invisibility of the skills showed my own and Richard’s familiarity with these skills. In using a method, such as participant observation or filming – where seeing things happen is vital – it became very clear that my own familiarity with it had made the skill invisible.

This realisation, then, became part of my overall analysis. In Chapter Five and even more so in Chapter Six, I show how these creative methods were embedded within the mundane routines and everyday skills within academia (p. 73), and that in some ways creative methods were no different from more mainstream qualitative methods. That was, however, one reason I felt I was not gathering data. As stated earlier, creative methods are sometimes

referred to as ‘innovative’ (Pain, 2009; Taylor & Coffey, 2008; Wiles, Pain & Crow, 2010; Xenitidou & Gilbert, 2009, pp. 60-65), which indicates a break with the past. Even though I had already dismissed thinking of their methods as innovative, I still expected them to be different. Yet, the practices I was seeing all seemed so familiar – not least because I was a qualitative social scientist like those I was observing. I was a ‘native’. This meant that most practices I was witnessing were no different from how I had done my research many times before. I was lacking ‘strangeness’ (Van Maanen, 1995, p. 20). However, while I was not gathering the data I had expected and in fact did not at first see the data, I was still definitely addressing my research questions. It actually showed me the opposite of what I had expected: these so-called new practices were in fact quite similar to the ‘old’ ones and the way in which Richard was making his posters was a small improvisation from the ‘old’ practices.

Later on, I observed Richard working on a photo. He was toying with an idea, but could not yet visualise it. In this moment, Richard also engaged with me and the camera I was holding. He explained what he was doing. He explained his thought process. Then he became quiet and it became a bit awkward because I felt like I was intruding. I felt I was forcing Richard to engage with the camera, rather than just do what he wanted to do. Moreover, he told me I was making him feel uncomfortable just by being there, looking over his shoulder, watching him. This was obviously not enhancing our rapport or the knowledge production of my research. I therefore decided I would not pursue this way of observing my participants and instead focus on the other ways I was engaging with them.

Nonetheless, this experience proved invaluable for my research, so how would it have been possible to do more observing with less discomfort? What did work was asking Richard to tell me about the process. Hindmarsh and Tutt (2012, pp. 57-73) and Baptista and Ferreira da Luz (2011) have done this kind of work, where they ask participants to tell them what they were doing. A crucial difference, though, is that the researchers whom these scholars filmed were working in pairs and already discussing their work between one another. The dialogue was thus a natural part of the process they were filming.

Concluding: Interviews, Filming and Participant Observation

This approach in which I used various methods enabled me to engage with my participants’ practices, thoughts and ideas about creative methods and, in the case of the postgraduate research students, observe how they developed over time. In these past sections, I discussed how I applied my main research methods of data-gathering: interviews, filming and

participant observation and the issues I ran into. Reflecting on those issues, I realised how my relationships with my participants and the position I took up was influencing my data-gathering. In discussing the issues I had with filming Richard's poster-making, I showed how my own academic background and the practices I had developed played a role in determining which data I could see. It also showed how through reflection, interaction with the literature, data and of course Richard himself, I improvised, straying from my own reifications and developing another understanding of the situation. Finally, my own emotions concerning the situation (feeling awkward towards Richard and feeling worried about not gathering any data) also impacted how I handled the situation. This example of the methodology-as-autobiography approach (Hammersley, 2011, pp. 25-30) showed how my research developed dialogically, relationally, emotionally and rationally (Burkitt, 2012, p. 471), and how the materials I gathered were co-constructed by my participants and me.

Transcription of the Interviews

The materials I gathered comprised field notes, emails, interviews and video recordings. For my analysis, I listened to the interviews and watched the videos repeatedly, but to keep them manageable also transcribed them. For the field notes and emails, already written, no transcription was needed.

Transcription of interviews can seem self-evident. To some, it is just a matter of typing what was said, where more or less details of the spoken words may be included. Saying the transcription was done 'ad verbatim' suffices to explain the transcription and the choices made in the process. However, as several authors (Brooks, 2010; Lapadat, 2000; Lapadat & Lindsay, 1999) assert, transcription is not transparent, but rather laden with theory as to what is considered data. Does it involve just content or also context? A good example of transcription based on theory is Jefferson transcription, which is connected to conversation analysis. Jefferson transcription gives clear rules for the transcription of conversations including silences, interaction and pitch. These details are given in order to understand how *what* is said and *the way* it is said influence structure and outcome of the conversation. Apart from conversation analysis, the discussion of transcription is often about the best way to 'translate' the sounds onto the paper. What the best way is depends on the researcher's interests, but this is not just about words or sounds. There are many other ways to think about transcription. O'Dell and Willim (2013, pp. 317-318) emphasise, for instance, the inclusion of elements such as the five senses or the rhythm and emotions of words and utterances, thus

making the transcription more performative. Arguably, poetic transcription conveys more of the non-discursive elements of what was said. However, one needs to remember that it is still a *trans*-scription, where the interview is changed from one mode of representation to another – a translation. In such a translation, meanings are inevitably lost, but also added (Benjamin, 1996). Moreover, one also needs to think about the audience who will read the transcription. Scholars unused to reading poetic transcription might not be helped by such a form of representation (see also O’Dell & Willim, 2013, p. 324). Awareness of these different ways of transcription is an important step in acknowledging transcription as part of the construction of the research.

I therefore prefer to describe how I did my transcription, rather than assume a reference to specific rules or the term ‘ad verbatim’ explains all. I transcribed all the interviews myself and used Express Scribe, a free downloadable programme that enables a user to play and pause, speed up and slow down in the same window where the text can be typed. In the beginning, I tried to transcribe all the words plus all the pauses, *erm*’s, *ah*’s and repetitions. I soon realised, though, that such a transcription would not befit the questions I had asked; I was not interested in the flow of the conversations, but more in *what* my participants told me. Furthermore, on a practical level, it would take far too much time. Accordingly, I decided to just transcribe the content, the words with some indications of emphasis or notation of tone of voice. I then sent the finished transcripts to my participants for them to check for any errors and, more importantly, to keep the conversation about ethics going, which I say more about in the *On-going Ethical Considerations* section. Reading their transcriptions, participants commented on their talking styles; one participant told me to feel free to clean up her transcripts. Clearly, encountering their spoken words in a different mode was confrontational, where the way these were uttered became devoid of the characteristics of the spoken word. However, for me this proved different. The process of transcription actually connected the words on the screen or paper to the words spoken in the interview. It helped me hold my participants’ voices and intonations in my head. When I read the transcripts, I heard the words being said as well as the words that were not said. I heard the meanings that were formed in dialogue without being verbally uttered. It was another layer of meaning conveyed in the conversations and helping me in analysing them. Because the reader was not present and does not hear the sounds, intonation or pitch, the simple use of quotes in the results chapters lost some of this richness. For me, as a researcher, though, all three ways of handling my materials – listening, transcribing and reading – helped me get to know and understand my material. In providing a context for the quotes, I hope to do the same for the reader.

Transcription of the Presentations: A Multimodal Approach

Similarly, transcription of the video recordings worked to take my analysis another step forward. In some ways it was even more helpful than when transcribing interviews. As this was my first time transcribing video, it was a lot less obvious what I should transcribe,²⁸ which made me think about what I needed from the transcription. I started by simply asking myself what I could see, what was happening. I noted down what was said and what was on the PowerPoint slides. Through this process I started to see more, such as the sequence of events, the people and the objects present. Things that at first did not catch my attention became visible through the process of transcription. This worked in tandem with reading literature about multimodality (Carter-Thomas & Rowley-Jolivet, 2003; Jewitt, 2009; Jewitt, Kress, Ogborn & Tsatsarelis, 2000; 2001; Kress, 2010).

In a multimodal approach, the researcher pays attention to a variety of modes in looking at meaning-making. A mode, according to Kress (2010, p. 79, italics in the original) “is a socially shaped and culturally given semiotic resource for making meaning. *Image, writing, layout, music, gesture, speech, moving image, soundtrack and 3D objects* are examples of modes used in representation and communication”. Language, then, is just one mode and others, such as colour, image, sound, gestures and gaze, are just as important in meaning-making (Jewitt, 2007, p. 304; 2013, p. 251). What’s more, the interaction between these modes becomes the focus in understanding how meaning is made, how the interaction between the different modes worked together to convey a certain message (Jewitt, 2013, p. 250). In the quote from Kress above, this becomes immediately evident through the use of italics. The font tells the reader that these words need extra attention. These words are emphasised, whereas the other words in roman are not. The combination of these different modes conveyed a certain meaning. There are other ways to do this, such as using bold font or a different colour. The choices between the different resources are dependent on the resources available to the participants (in terms of skills and material availability), but also on how these “modes have been shaped by their social, cultural and historical use” (Jewitt, 2013, p. 251). In academic writing, italics are generally used for emphasis.

Using a multimodal approach thus meant that my transcription needed to include description of settings and contexts in addition to that of participants’ movements and

²⁸ This marked the development of a new practice for me. It was unlike the familiar and therefore ‘invisible’ practice of interview transcription, which I had done many times before.

gestures, the visual materials – with their colours, shapes, sounds and sequences – as well as language both in spoken and written forms. Furthermore, the interaction between these modes needed attention.

Reading this literature while I was transcribing made me realise I was interested in how these presentations were embedded in the context of the conference or seminar, the presenter's educational background and disciplinary practice. I was also interested in the ways these presentations differed and resembled more traditional presentations. As in the above example with Richard, what I noticed and did not notice at first about the presentations was related to how creative methods were presented and how this was both similar and different to the presentations I had seen and taught throughout the years. The elements I noticed were those *not* fitting in with those presentations I personally was familiar with. In contrast, I was 'blind' to the ways creative methods were presented when they were similar to what I had seen before. Reading new literature expanded my horizon, encouraging me to look for other elements and giving me the tool to do so. This process then showed how I, as the researcher, impacted the analysis through my own reifications (Wenger, 1998, 58-62).

Concluding: Transcription

Transcription was a first step in my analysis because it made me think about *how* and *what* I wanted to analyse and thus how I needed to transcribe. The interaction between the literature, my own thinking about the analysis, starting the transcription and the sensory experience of hearing the interview and seeing the presentation whilst experiencing all the emotions I was feeling at the time resulted in a transcription specific to the needs of my research. It accommodated those things I needed for my analysis: the different modes of spoken and written languages, the content of what was said, laughter, gestures, intonation, the design and layout of the Power Point presentations, the use of colour and images (still or moving).

Analysis

As with my transcription, I have not adhered to a specific type of analysis (O'Reilly, 2005, p. 178). As Leder Mackley and Pink (2013, pp. 336-337) say, the analysis within ethnographic work is often difficult to document (see also O'Reilly, 2005, pp. 212 and beyond). This is especially the case because an ethnography does not focus on a single form of data

production²⁹ or separate the stages of collection from analysis (e.g. Davies, 2008, p. 231; Leder Mackley & Pink, 2013, p. 336-337; O'Reilly, 2005, pp. 175-177; 2009, pp. 14-16; Ten Have, 1977, p. 75). The same holds true for my thesis, where some of this chapter's previous sections have already shown my analysis to creep through. Here, as with my transcription, I recount what I did for my analysis and reflect on that in the applied context of my specific research, rather than simply naming a type of analysis (Duits & van Romondt Vis, 2009, p. 46; Leder Mackley & Pink, 2013). The next section discusses the analysis of the interviews, followed by the analysis of the video material.

Analysing the Interviews

For my analysis, I started with a close reading of all the interviews to get a general feel for what my participants discussed, which could possibly be turned into themes. At the same time, my analysis was guided by particular sensitising concepts (Blumer, 1954) drawn from my theoretical framework and literature review. Sensitising concepts serve as indicators for possible directions in the analysis, but they do not dictate it. Other themes can emerge from the analysis as well. My sensitising concepts (ibid.) were communities of practices (which included negotiation of meaning, reification and participation), boundary-work (which included the reward system or assessment of research), epistemology, improvisation, innovation,³⁰ power relations, ethics and the research reifications. I also paid attention to the participants' academic background and institutional affiliations and the development of their use of creative methods.

When analysing, I did not just focus on what had been said or done, but also on how, why and where it was said. For instance, I would ask my postgraduate research participants about their preparation for fieldwork. Interestingly, certain topics would be talked extensively about while others were deemed self-explanatory and barely discussed. Take the example of Liza from the *Interviews* section. The things she spoke about were so normal to a researcher, that they did not need any explanation, especially to a fellow researcher like me. Other times, the conversation with the postgraduate research participants took the form of a brainstorm or an analysis in which they constantly reformulated their thoughts on how their research was going. Of course, what they said in these conversations was important, but it was also about

²⁹ I think it is questionable whether other methods do so. But at least within an ethnography, it is very clear that a mix of different methods and materials is used.

³⁰ Even though I had already dismissed this term on the basis of my literature study, it remained part of the discourse surrounding these methods and my participants used it.

how and *why* they said these things. What was crucial here was that my participants were researchers talking to another researcher. Both of us had ideas about how research should be conducted and knew the other had those ideas as well, which made the interview more like conversations in which ideas were exchanged, rather than information given one way. Ideas were uttered, but these were not necessarily their viewpoints as much as possible ways of doing their research. In this way, the conversations were also examples of the negotiations and improvisations my participants were doing in regards to their own research.

The themes coming out of this first reading and the ways of talking, then, became a focus for a second reading, where I was constantly comparing between participants and between interviews (Boeije, 2002). What became clear, though, was that the themes remained very broad and abstract. This puzzled me. There were overall similarities, but they differed too much at the specific instances to be grouped under the definitive heading of a theme. This was different from my earlier research, where coding interviews led to the ‘emergence’ of themes (Boeije, 2002; Charmaz, 2008). This was not the case now, and at first I did not know what to do. My own methodological practices and background in qualitative research methods, as discussed in the *Personal Academic Background* section, affected the analysis. Although I had read quite widely on different methods and disciplinary approaches to qualitative research, my own practical experience with doing research was based on doing one-off in-depth interviews and subsequently doing a grounded theory analysis. It took me some time to realise that the themes revolved more around processes and the on-going activities outlined in the three sub-questions (negotiation with others, negotiation with personal practices and emergence of the creative methods). These processes involved a whole range of different activities in which time was an important element, such as the brainstorm mentioned above. This meant I had to improvise from my earlier understanding of themes to a new one, which encompassed the ways in which my participants spoke about and were practising creative methods over time rather than a moment frozen in time.

Analysing the Presentations

The videos of the presentations were similarly analysed. I started by watching and listening to them multiple times. I transcribed the presentation itself using a multimodal approach (Jewitt, 2013; Kress, 2010), the setting where the presentation took place, both in physical terms – as in the location where the presentation took place – as well as the organisational context. I had planned to use the same sensitising concepts (Blumer, 1954) of the theoretical framework and

the constant comparative method (Boeije, 2002) to interpret how my participants made their presentations, how they presented their work and how their presentations differed from and resembled other presentations both within and outside of the group of participants. However, as described above, during the transcription of the presentations, I realised I was interested in how the presentation of my participants resembled traditional ones. I therefore abandoned my initial sensitising concepts and started reading about conference presentations (e.g. Carter-Thomas & Rowley-Jolivet, 2003; Dubois, 1980; Rowley-Jolivet 1999; 2002; 2004; Rowley-Jolivet & Carter-Thomas, 2005; Shalom, 1993; Swales, 2004, Ventola, Shalom & Thompson, 2002). I then used the general structure of presentations as described in this literature to compare my participants' presentations to. I paid attention to the multimodality of the presentation in which spoken and written language, still and moving images, colour, font, gestures and body language worked together to create meaning (Carter-Thomas & Rowley-Jolivet, 2003; Jewitt, 2013; Jewitt, Kress, Ogborn & Tsatsarelis, 2000; 2001; Kress, 2010; Rowley-Jolivet, 2004, p. 146). This meaning was created in the choices that my participants consciously or unconsciously made: their choice to use certain modes and not others; their choice for a particular option within a particular mode; how my participants combined these modes; and finally, the context within which these choices took place. This context shaped the presentation on multiple levels. It had consequences for how my participants prepared themselves for the presentation, the way the presentation itself was constructed and how the presentation might be conceived. The context manifested itself in different ways. The concrete context of where the presentation took place – including the room and the equipment available in it – enabled the participants to hold their presentations in a certain way. The more general context of the institute or association organising the conference shaped the presentation as well. Finally, the academic context – and in particular, the context of certain disciplines or research areas – asked for specific ways of presenting. This meant that certain practices predominated in presentations (cf. Jewitt, Moss & Cardini, 2007, p. 306), which subsequently shaped them. The multimodal approach helped me understand how the modes worked together in a specific context and how they were part of the negotiation of the creative methods.

Concluding the Analysis

For my analysis of the interviews and the presentations, I combined several approaches to accommodate the insights I gained during the process. I had planned to use the sensitising

concepts (Blumer, 1954) from my theoretical framework, but in both cases I had to adopt other ways of doing the analysis. For the interviews, the themes turned out not to meet my initial expectations. It was only when I realised that they represented on-going activities in which change and stability were important aspects that my analysis started to make sense to me. I was then able to understand the different ways my participants spoke about their research. As mentioned, they would discuss their approach with me: brainstorming possible ways of conducting the research, asking me questions and talking about aspects they were interested in rather than answering my questions. These ‘disruptions’ of the interviews and my subsequent improvisation of how to analyse them are distinct examples of co-creating knowledge in which my participants and I were both trying to figure out how creative methods worked and were negotiated. Their negotiations shaped my understanding of how creative methods worked. My questions about their practices as well as some of the answers to their questions, in turn, shaped their understandings.

As for analysis of the presentations, I abandoned my initial plan of using sensitising concepts (ibid.) of the theoretical framework when I read literature on multimodality (Jewitt, 2013, Kress, 2010) and presentations (e.g. Carter-Thomas & Rowley-Jolivet, 2003). This literature made me realise that I wanted to know about how my participants’ presentations created meaning through the use of different modes and how they resembled and differed from ‘traditional’ presentations.

I thus moved from an approach in which I looked for fairly stable themes to one where I focused on development and tried to unpack how this took shape in different situations. As such, the process of my analysis was an example of my own negotiations and improvisations in the research process. Like the process of my participants, about which I elaborate in chapters Four, Five and Six, the improvisations I made in my practices were not radical changes, but rather incremental. This showed how they were still connected to and embedded in the practices I learnt at university.

On-going Ethical Considerations

This section discusses the ethics of my own research, such as the procedures I used, but also the on-going conversations with my participants about ethics (such as e-mail exchanges following the transcribed interviews) and the way I conducted my interviews. Finally, I elaborate on one specific interaction with one participant, Richard, in which I had to negotiate the ethics and methodology of my own research practice as part of that on-going practice.

Because my research involved working with human participants, the university required me to complete an ethical clearance checklist. The list's questions inquired about, for instance, the vulnerability of the participants, the way data was gathered, how this data was stored and whether or not participants were able to withdraw at any point. This was to ensure the research did not in any way harm the participants. In addition, I created participant information sheets to seek informed consent of both groups of participants.³¹ However, I consider these as starting points towards an ethical conduct of research, rather than an endpoint. In my view, these documents embodied only a narrow take on what ethical research is, often based on the logic of quantitative research design. Measurements, such as the checklist, the participant information sheet and the informed consent form, are often aimed at managing institutional risk (Macfarlane, 2010, p. 20) and rarely mesh well with qualitative research. This is because they are based on assumptions about the predictability of research and, subsequently, the occurrence of ethical issues. Qualitative research often has an iterative design (O'Reilly, 2005, p. 3), which makes it difficult to foretell whether ethical issues will occur and, if so, what they might be (Macfarlane, 2010, p. 20). As discussed in the previous sections, there were points during this project where things did not go according to plan. And I did not always have a full outline or a detailed plan.

There are other complicating factors when the research is disseminated. How the audience receives the research and what they subsequently do with it are impossible to predict. As Rose (1997, p. 317) states, "the researcher is not the only authority on academic knowledge and its effects". This does not mean researchers should ignore ethical guidelines (ibid.), but rather that they should take a different approach in which ethical considerations take place as an on-going process. A broader conception of ethical conduct of research involves not just ticking off boxes on a form, but constant negotiations with one's participants. This is encouraged not only because the research project might change during its course (Heath, Charles, Crow & Wiles, 2007, p. 404), but also because ethics is not something which is settled once and for all. Participants might change their minds about their consent. Moreover, some of the ethical ideals are not as black and white in practice as in guidelines, and thus require a constant conversation (O'Reilly, 2005, pp. 59-63). The power relation between researcher and participants is not fixed, but rather a dynamic one; the researcher often has an advantage over the participant, but I do not think researchers always have a more powerful position. They can just as well be vulnerable at times and it is also important to

³¹ See appendices IV, V and VI.

consider harmful effects for researchers as well as participants (see, for instance, Ballamingie & Johnson, 2011; Smith, 2006, p. 651). As my participants were fellow researchers, I had to carefully think through how I could approach them (Wiles, Charles, Crow & Heath, 2006). How could I look at their work and practices without revealing their identities (p. 284), or making them feel I was judging them? On the other hand, how could I maintain a critical distance and allow myself to disagree with the way they conducted research? I did not necessarily want to become an advocate for the advancement or use of these particular methods (Lumsden, 2013).

The participant information sheet and the informed consent form helped me address these subjects. One of the first things we spoke about in the interview was participant anonymity. I had expected this to be a tricky subject. On the one hand, my participants might be easily recognised by those who read my research. Their colleagues and peers could come across my work and potentially recognise my participants. On the other hand, my participants might be quite happy to be recognised and get acknowledged for their thoughts, ideas and arguments (Grinyer, 2002; Macfarlane, 2010, p. 21; Tilley & Woodthorpe, 2011). In the contemporary climate of authorship, as Ally, one of my participants, said, you might want people to know it was your research. I come back to this later in the *Co-constructed Knowledge* section. Thus, anonymity as a way to protect one's participants can be a good idea, but it can take their own voices away, which is not necessarily what you or, more importantly, they want (Gordiano, O'Reilly, Taylor & Dogra, 2007; Grinyer, 2002; O'Reilly, 2009, p. 62). To work with anonymity on principle does not take into account the unexpected nature of the qualitative research process. Macfarlane (2010, pp. 23-26) therefore proposes to work on the basis of virtues rather than principles. Virtues do not prescribe a course of action in the same way a principle does. An example is respectfulness (p. 24-25). In some cases, it is respectful to maintain your participants' anonymity; in others, it might actually be more respectful to tell the readers who they are. Of course this is easier said than done for researchers working in the institutional context of a university with ethics review boards, who require researchers to conduct research according to specific ethical guidelines. As said, my university required me to give anonymity, but in subsequent publications this could be renegotiated into a co-authorship.

In the end, it was precisely because my participants were researchers themselves that the conversation about anonymity was easy. They were familiar with having these conversations themselves and the precariousness it entailed. Some had asked me about my ethical approach beforehand over email, but mostly we discussed it at the beginning of the

interview when I showed them the participant information sheet and asked them to sign the informed consent form. I left it for my participants to decide whether or not they wanted to be anonymised, but in general they did not pay much attention to the forms. In subsequent conversations over email, for instance, when I sent them the interview transcriptions, I would again address the issue of anonymity. Most of my participants replied to correct some of the names they cited and to say they were fine with the interviews. Two of them explicitly said I could use their real names. One expressed wanting to see how the interview was going to be woven into possible articles. Some did not reply; I could only assume they were okay with the transcripts. One person, however, specifically asked to maintain anonymity and to leave certain interview elements out. This then begged the question of whether I should I anonymise the others as well. O'Reilly (2009, p. 62) says she decided to anonymise everyone in her research because it became confusing to use both real names and pseudonyms in the same study. Grinyer (2004), on the other hand, used both real names and pseudonyms in her research with parents and their children with cancer. I chose to anonymise everyone for two reasons. The first was that not all participants responded to my email containing the transcribed interview, so I did not have explicit consent from them to use their real names. Second, like O'Reilly (2009, p. 62), I felt it was less confusing for me and clearer for my audience if all participants were anonymised. However, a pseudonym was not necessarily enough. To convey my argument, I needed to describe details of my participants and the contexts in which their research took place (Tilley & Woodthorpe, 2011, pp. 198-201). Even in early versions of my results chapters, where I disclosed almost nothing about my participants, one of my supervisors was able to identify participants merely from interview quotes. This made the balance between anonymity and the disclosure of enough contextual information a difficult one (Tilley & Woodthorpe, 2011), and careful consideration of the participants' stances on this matter was crucial. In some cases it might not have mattered greatly whether the participant was identified, but in other cases, especially relating to institutional boundary work, this was problematic.

Another way I addressed the ethical aspects of my research was through trying to maintain an open research relationship. I did this by opening up space for my participants to raise questions. This process of involving them and their thoughts on the research started at the beginning of the interviews, or, as they would say, conversations. I would start the interview asking if they had any questions for me. Often this would not be the case, except for some clarification on the research. However, questions would come up later on during the interview. With the PhD students, especially, this would happen more as time went on.

Sometimes they were apprehensive about it. Liza for instance, would ask if she was allowed to ask me a question, showing she was aware of research conventions. Others, like Richard, were less prudent, as I show in the next section.

Emerging Practices: Negotiating Knowledge, Ethics and Power

Here I describe a conversation during which Richard asked for my advice on an issue he was having. The conversation illustrates some of the ways in which ethics and power were negotiated in the research process. Additionally, it shows how creative methods are negotiated as well as how the knowledge in this research was co-constructed. It does not offer a transparent reflexive account of the research process, as small details of our interactions went unnoticed (Smith, 2006, p. 647), but rather the messiness it inevitably was (Rose, 1997, p. 314). Using the methodology-as-autobiography approach provides a more honest account of how our positions within the conversation constantly shifted and how power emerged in this process. My own personal background and practices guided the research, though Richard's defiance of them resulted in a situation in which both Richard and I contributed to the knowledge produced.

Richard was seated in the chair opposite me. It was the 2nd of December 2011. It was our first proper interview, but I had known him for a while now. We had seen each other at events on methods such as reading groups and seminars on ethnography and creative methods, where we spoke about methods and often also got into more epistemological discussions. So Richard knew I was interested in methods. Now, he had become my participant and I asked him how his research was going. He was telling me about how he intended to recruit his participants. He wanted to hand out surveys and, on the basis of those results, choose specific individuals to participate in his larger qualitative study. He was aware of the possible problems of using both quantitative and qualitative methods, but explained why he felt in this case it was appropriate. Some of his peers, however, were sceptical or even dismissive; they said it was not proper qualitative research. Then, Richard looked at me, and asked: "What do you think?" I hesitated. Should I answer his question?

I did not hesitate because I did not know how to answer his question – I could answer it. I hesitated because I was afraid it was the wrong thing to do in terms of my research, my methodology. Was I ruining the rest of this interview and possibly our research relationship? I had thought about my participants asking such a question, but when it happened it was still a bit of a shock and rather than a well-thought through rationale, it was a gut feeling that made

me answer his question. I do not elaborate here on how I answered it. Instead, I elaborate on my reflections on this specific situation that touch upon power, ethics, what I think knowledge is, how it is produced and how it played out in this specific situation.

From a post-positivistic perspective, it was easy: what I had just done was a complete mistake. The interviewer is not supposed to utter much more than an open question. This, however, assumes that there is something objective out there: that there is such a thing as *the* experience, *the* thought, *the* idea of the participant. It is there for the researcher to find as long as she makes sure she is as invisible as possible. She reflects on her role in the conversation – what she says and does, who she is or could be in the eyes of her participant – only to try to erase those effects as much as possible. The aim is to find out what is *actually* happening (Davies, 2008, pp. 4, 11; Hekman, 1986; Pink, 2007, p. 23).

As described in the *Reflexivity* section, I do not really agree with that line of reasoning. I do not think there is an essential thought, experience or idea. They are always in dialogue with one another, be they with actual people, the voices of other academics in your head or the article you are writing. However, I had been trained to listen to the participant's voice; I was not to ask any leading questions, let alone tell him what I think. This meant I should not dominate the conversation. In the modules that taught me how to interview, discussed in the *Personal Academic Background* section, it was said that in a good interview the participant spoke the majority of the time, only interspersed with single sentence questions by the researcher. Even in more post-structural, postmodern constructivist epistemologies, the researcher wants to know about *the participants'* thoughts, ideas and experiences. It follows that the researcher does not share hers. What was more, Richard was asking for my 'professional' opinion. If I told him my theoretical arguments, would I not literally produce my own data? But what if I did? Was this not exactly what my research was about: how methods were being developed by discussing ideas with others such as supervisors and peers? This was an instance of that process in action. Besides, was it not a bit presumptuous of me to think Richard would instantly change his mind just because of what I had said? Moreover, it was unlikely he would not come across these ideas in conversations with others, in articles or at conferences. This was just one moment in an on-going negotiation of meaning (Wenger, 1998, pp. 58-62). I could not see myself as external to this process.

Another crucial aspect was his position as my participant. When my officemates or my academic friends asked me for advice, I told them what I thought. However, now that Richard was a participant instead of a friend, would I remain silent? Saying nothing would have made for an uncomfortable situation, but it also felt unfair to do so. Richard was sharing his

thoughts, ideas and experiences with me. It felt uncomfortable to only be on the receiving end. Answering his question could go a little way in evening out this imbalance. The imbalance was not just there in the situation of the interview, but even more so in the recontextualisation and representation of the research, according to Briggs (2001, pp. 914, 916, 919).

I want to be careful about the way I employ power in these situations. As I explained in Chapter Two, power is not something you have, but rather is part of a complex interaction between traditions, established practices, expectations, symbolic, economic and social capital and how people appropriate them. It is too simple to juxtapose the researcher and the researched as powerful versus powerless. Nor is it helpful to characterise Richard and the rest of my participants as 'elites' in the sense that they possessed power through their expert knowledge (Littig, 2008). What is important is people's assumptions about the power the other possesses (Desmond, 2004, p. 265-266). Not only were my participants too diverse for a category like elite to hold any meaning, but also, 'the elites' do not possess unconditional power (Smith, 2006, p. 645). To define power as a quality possessed by elites is unhelpful, as it does not account for how power is manifested in different research situations such as the interview or dissemination. Smith (2006) uses another characterisation of power, which she calls post-structural, where power is fluid, diffuse and mobile. In the conversation with Richard, it is evident that the power was with neither of us, but rather shifting between us.

When I started this research I thought it was going to be difficult to gain access. I thought of my participants as more powerful. Although they were able to negotiate situations to their favour, the situation overall was not characterised by a simple dichotomy between experts or elites and laypeople. The reason was that I was also an expert of sorts: a social scientific researcher who knew about qualitative research methods. Moreover, despite power not being something I or someone else had, in the setting of an interview there were certain expectations of what would happen. As a researcher, I had a responsibility to be conscious of the power dynamics in research situations. I was the instigator of the interview, and, in general, I tended to ask the questions. I gained from talking to my participants – Richard, in this case. I also had power afterwards, when I recontextualised what he had been telling me, through my interpretation and representation of that conversation and the others I had had with him. On the other hand, I was not simply an almighty researcher and Richard, a helpless participant. It was not just I who was gaining from the situation. Richard was taking the opportunity to direct the conversation towards his needs. He, like other participants, had said that talking to me had been helpful to get his head round things, to make things clearer. Both

groups of my participants were researchers, all having done interviews for their own research. That hardly made them inexperienced, 'vulnerable' subjects who did not know how an interview worked.³² Lumsden (2009, p. 498) points out how "the researcher's social position shapes their experiences in the field". In her research on boy racer culture, Lumsden's gender and sexuality proved important in her relationships with the predominantly male participants. In my case, my knowledge about methods, in general, and qualitative ones, in particular, was important. Richard approached me as an expert who could help him with his research. This shows how power was negotiated and became visible in how our aims and goals drove our interactions.

Allen (2003, p. 8) understands power in this way to be "mediated as a relational effect of social interaction". He goes on to say how "power is not a uniform or continuous substance transmitted across tracts of space and time; it is always constituted in space and time" (ibid.). Allen criticises that power has been conceptualised as being everywhere, thus keeping us from seeing the particularities of power and the various modalities in which power is experienced. Power is never general, but always of a particular kind (ibid.). Examples of these particular kinds are domination, authority, seduction, manipulation and coercion. Allen does not aim for an exhaustive and mutually exclusive typification of power, but he does show that power not only works in different ways, but also has different effects (p. 6). In the example with Richard, there were different kinds of power that could be experienced. There was the power I experienced as a researcher in the research relationship and as an expert in qualitative methods in the eyes of Richard. At the same time, I experienced Richard's power exactly as a participant. I wanted our research relationship to be equal as much as possible, which, in my view, meant to give something back. Both of us 'possessed' particular resources the other was interested in with which we could exercise power: knowledge about and experience with qualitative and creative methods. However, as Allen (2003, p. 5) says, resources are helpful to exercise power, they themselves are not power, but can be depleted or used incompetently. "[P]ower as an outcome cannot and should not be 'read off' from a resource base, regardless of its size and scope" (ibid.). Both Richard and I had other options to access those resources. Richard could ask other researchers for advice. I had other participants. In each relationship, albeit differently, power is constituted in the social interaction. This particular encounter between Richard and me was one of the instances in which power was negotiated and experienced. This was on-going for the duration of the research process, as well as after, not

³² One can wonder whether such inexperienced, 'vulnerable' participants even exist.

just between Richard and me, but concerning all my participants.

In conclusion, this conversation was an example of my own practices being questioned. Even though my theoretical affinities lay with constructivist reflexive theories on the production of knowledge, my practice had been informed by my own academic background. Even though the modules I had taken were based on the same constructivist reflexive theories, interviews were still seen as successful when participants would open up, when they would speak freely, for a long time, uninterrupted by the interviewer. Interestingly, my own reifications in regards to doing interviews were shaped more by my practices than the constructivist reflexive reifications I had read about. Clearly, though, those reifications did not work in this situation and I had to improvise instantly. This made the encounter with Richard a good example of the methodology-as-autobiography approach. It was also a good opportunity to reflect on how my participants co-constructed knowledge with me by disrupting the course of the conversation; how these interactions are fraught with contradicting and divergent views on ethics; and how it is more helpful to work with virtues than principles in such situations because they allow for improvisations – that is more productive than stubbornly sticking to principles or reifications that do not work (Macfarlane, 2010). In the following section, I focus specifically on the ethics of one aspect this conversation showed: co-constructed knowledge.

Co-constructed Knowledge

The relationship between my participants and me calls for reflection on the intellectual property of this research. As discussed in this chapter, I worked closely with the postgraduate research students, especially, and for a while we formed our own communities of practice in which knowledge was shared and negotiated (Wenger, 1998). Therefore, the knowledge conveyed in this thesis is a co-construction. Here, I address this issue by first discussing other examples of co-creation in this research project, then looking at ways the participants' contributions can be acknowledged and finally how this relates to reifications concerning authorship.

The previous section gave some examples showing the co-construction of this research: Richard who steered the conversation towards his needs, Liza who asked me questions. There were other examples, such as when Louise asked for feedback on her methods chapter and Hilde, on her presentation. Inspired by my comments or not, the changes my participants subsequently made found their way back into my research. That, in turn,

enriched my understandings of how the other participants did things. Mary told me her work on a bibliography of poetic inquiry and a book chapter on reviewing this method were ways of creating academic legitimation for it.³³ This showed me how my participants were trying to break down boundaries, which was how I subsequently understood Bart and William's and Hilde's research; having been awarded with PhDs, their research created precedents with which boundaries could be broken down.

In this sense, my participants were interviewees – ‘observees’ – as well as teachers. They showed how they did their research and thought about it, thereby teaching me about creative methods and the different ways research could be executed. I would ask them how they did it, but they asked me as well. I then again was a researcher encompassing the roles of interviewer and interviewee: learner, observer and documenter. I organised the research, decided what would be researched and took responsibility for the outcomes. I looked at my participants' work from a different perspective than they did. Having said that, as the examples in this chapter illustrated, my interpretation and analysis were based on how they did their work, on what they said and thought, and their taking control of the conversations, as Richard did. This made their work an inevitable and inextricable part of the work I created here. Both the participants and I contributed to the knowledge produced in this thesis. Both their and my understanding formed a layering of perspectives, which were negotiated in the process of the research. In this thesis, my perspective was put forward, although it became difficult to call it *my* perspective anymore.

An important question thus arose: How do I formally recognise the work my participants did apart from a thank you in the acknowledgements? There are different ways to acknowledge such work. For instance, in other forms of research dissemination, such as film or an exhibition, participants can be named as authors (Cahill & Torre in Lake & Zitcer, 2012, p. 391). However, acknowledging the work in the writing up of the research has been given less attention, according to Lake and Zitcer. They acknowledge this comes partially as a consequence of how research is traditionally done (pp. 389-390), but this deserves specific attention, especially when research is done with other academics. In the next section I discuss three ways to acknowledge the work of participants in academic output: different ways of writing, citing participants and co-authorship. I also note which options I could use in my work. In discussing citations and co-authorship, it also becomes apparent that certain reifications in producing academic texts hinder putting them into practice (Sinha & Back,

33 The examples I use here of Mary, Bart and William, and Hilde are discussed further in Chapter Four.

2014, p. 483).

Ways of Writing

Lake and Zitcer (2012, pp. 396-398) suggest four different ways of writing up the research for collaborative authorship. The first three reflect the collaborative nature in manner of writing: by “avoiding abstraction and interpretation”, which they find to be a form of dominating the participant (p. 396); by “contextualization”, in which

the subject speaks for himself or herself and the researcher’s responsibility is to situate or contextualise the subject’s narrative in a broader descriptive framework. By contextualising the subject’s practical knowledge, the researcher’s external perspective and the subject’s experiential vantage point can be complementary and collaborative rather than contradictory and exclusive (p. 397);

and by “transcending distanced objectivity”, in which the different perspectives of both the participants and the researcher are represented in dialogue (p. 397). The fourth is a more radical strategy, in which the logic of research needs to be rethought. This is closely related to discussions in participatory action research: about who plays what role in the research; who is the researcher; who is the author; and who is given the authority. Here the self-evident position of the researcher as author and authority needs to be rethought, according to Lake and Zitcer (2012). They argue that because “the collaborative ideal requires collaboration all the way through” (p. 398), it requires a different approach from the start. This comes in contrast to the first three strategies, which can be done even after the research is over and in non-collaborative research.

Citing Participants

Another way of acknowledging the participants’ works is found in Reyes Cruz’ (2008) suggestion to cite her participants in the theoretical framework. She struggled with giving her participants a voice in her thesis and subsequent writings. She felt her participants gave her insights that were more than ‘data’ and deserved to be discussed in the theoretical framework. Trying to decolonise the way knowledge was produced, she suggested citing Graciela, her participant, but as she pointed out: “The act of citing textual traditions is intricately linked to the (im)possibility of foregrounding nonacademic’s reflections about social reality in

academic discourse” (p. 657). She went on to explain that citing is a way to place the researcher in a discipline and to create academic legitimacy for the researcher’s argument (p. 657; see also the discussion of Gilbert, 1976; 1977 in the theoretical framework). So, citing a non-academic for academic articles, books, funding applications and reports was a challenge for her because it did not place the work within a discipline, and it did not create academic legitimacy for the argument. The only way she could cite her participants was anonymously and only for non-academics: namely, school administrators and other local professionals (p. 657).

I could cite my participants’ work in this thesis because they are what Reyes Cruz (2008, p. 651) calls “legitimate player[s]”, and their work is judged accordingly. However, I have not done so in an obvious manner; I did not flag citations from my participants in the theoretical framework because I decided to keep my participants anonymous for the thesis. Nonetheless, I acknowledged their work through the citations. In addition, I note throughout the thesis when knowledge was shaped in collaboration with my participants. However, in the rest of the thesis, like Reyes Cruz, I cite my participants anonymously. In subsequent publications, it is possible to focus on one participant and co-authorship could be an option.

Co-authorship

In co-authorship, the participant is credited for work by adding his or her name to the publication Lykes, for example, worked with a group of Mayan Ixil Women in Guatemala. They had invited her to work with them on a research project “to respond to some of the psychological, economic and educational consequences of the [civil] war” (Lykes et al., 1999, p. 207). In that project, the women, in collaboration with Lykes, produced an exhibition, photo book and a website (Lykes, 2006, p. 271), as well as a co-written academic article (Lykes et al., 1999). For the exhibition, photo book and website, co-authorship does not present a problem, but for the academic article it can, as Sinha and Back (2014) show. They did something similar in a project working with young migrants and “offered [them] the opportunity to become observers of their own lives through writing, taking photographs and keeping journals and scrapbooks” (p. 474). They felt very strongly that their research collaboration with one specific participant, Charlynn Bryan, and the article that emerged from it had been co-constructed.

Charlynn helped us notice things we would otherwise not have seen without her insight and her analytical choices concerning both what to observe and the means by

which she was going to conduct her observations. Equally, assembling her perspectives with theoretical ideas that the researchers helped to raise led her to think again about her relationship to the past, present and future. Consequentially, she re-evaluated those relationships, and her past, from the vantage point of the here and now. In these dialogues, we reimagined empirical enquiry in a way that blurred the relationship between observer and observed, data and analysis and participants and authors. (p. 475)

Sinha and Back decided to add her name to the byline, noting that “the author’s attribution should read ‘Les Back and Shamsir Sinha with Charlyne Bryan’” (p. 483). Although the article was published, Bryan’s authorship was debated. The publisher said offering a pseudonym was standard practice (p. 483) and “queried the ‘ethical and permissions issues’ involved in revealing a participant’s identity” (pp. 483-484). In addition, the publisher wanted an institutional affiliation for Bryan. “In order to be credited, it is presumed you have to have a university association” (p. 484). Such institutional limitations can make it harder to acknowledge the work of research participants. Still, I have often seen articles written by independent researchers. In Lykes’ articles, this did not seem to be a problem, but there are invariably differences across journals. Nonetheless, the issues both Reyes Cruz (2008) and Sinha and Back (2014) experienced indicate that giving authorship to participants is not unproblematic. For my research, there is slightly different issue. Co-authorship with my participants in subsequent disseminations would not be the problem it was with Reys Cruz (2008) and Sinha and Back (2014) because my participants all had an academic affiliation. The thesis, however, is generally written by a single author (see also Chapter Four’s *Bart and William: Negotiating the Regulations for Research Degree Programmes* section) and the work of others can only be “specified in acknowledgments or in footnotes”, according to the Loughborough Certificate of Originality accompanying this thesis. Additionally, the university guidelines required me to anonymise my participants, as discussed earlier in this chapter. I therefore have not even considered co-authorship yet.

It is not possible to know beforehand how a research relationship will develop, how the knowledge will get constructed and whether participants will expect or want to become a co-author, but keeping co-authorship in mind from the beginning helps make it achievable. A way to do that is the article idea chart developed by Forchuk and Meier (2014). Ultimately it is not much more than a chart that is accessible to research participants, allowing them to propose topics for articles and allowing other participants to join in on those topics.

Nonetheless, it is a way of enabling co-authorship from the beginning by making the dissemination component of the research transparent to all involved in the research (p. 162). This is one way in which different “forms of authorship–contribution [can] evolve” (Sinha & Back, 2014, p. 485). As such, it is crucial to have an on-going open relationship with the participants in which they can be invited or asked to be part of the dissemination. In deciding upon future publications from this thesis, my participants will be involved in deciding if and how a co-authorship should take shape.

The knowledge produced from this research has been in collaboration with my participants, notably the postgraduate research students. In this section, I gave examples of specific points in which the collaboration was obvious and how it informed my analysis. Such collaboration demanded my reflecting on how their work could be acknowledged. I then discussed three possible ways of doing so – ways of writing, citing participants and co-authorship – and addressed the reifications potentially disabling the acknowledgment. While these reifications can make it hard to acknowledge the work done by participants, there are ways to overcome them. Most importantly, the researcher needs to take such possibilities into account from the beginning of her work.

Conclusion

In this chapter, I showed how my methodology took shape through adopting the methodology-as-autobiography approach (Hammersley, 2011, pp. 25-30). This approach entailed my describing *which* methods I used to answer my research questions as well as *how* I applied those methods. Because reflexivity played an important part in how I applied my methods, I started this chapter with my thoughts on reflexivity, which is not an easy or straightforward component of doing research. After discussing its development and some of its pitfalls, I argued that reflexivity is a rational, relational, dialogical and emotional process (Burkitt, 2012). It is not about making the research more objective or better, but about explaining the process of my research production. For the reader to have a clearer idea about how I started this process, I outlined my academic background and education, showing how my view on methods and methodology affected the research production.

To answer my research questions, I used an ethnographic approach (Pink, 2007, p. 22). In that approach, one challenge was how to construct the field (Beaulieu, 2010; Desmond, 2004) because there was no specific place I could go to. My field turned out to be constructed in collaboration with the literature, my participants and my supervisors. The participants I

engaged with determined where my field was, but more importantly *when* it was there. This was not necessarily always a physical space, but could be virtual. I therefore established what Beaulieu (2010) calls co-presence with my participants.

Afterwards, I introduced my research participants and discussed why I worked with them. Their diversity, both in terms of the creative methods they used and the stages of their careers, enabled me to see creative methods in development from various perspectives; as communities of practice, each participant brought his or her own background, opinions and affiliations to other communities of practice into the negotiations with these methods and others within the community. In this way, the development of creative methods is reflected in how my participants used and talked about them.

My next step was to elaborate on the methods I used with each group. This was a variety of methods providing me with an in-depth understanding of my participants' engagement with creative methods. Having in-depth interviews helped me gain understanding of how my participants learnt about creative methods, the way they applied them and whether applying creative methods had entailed any struggles. In the on-going interviews with the postgraduate research participants, this process of learning and negotiating the methods became even clearer because of the longitudinal nature of the conversations. Second, I tried to do participant observation, but I was not always successful. My main problem was the 'invisibility' of the practices for me. This invisibility or self-evidence of the methods was also a problem during the interviews. In both cases I felt I was not gathering any data, which was a consequence of my own reifications of what proper research and data were. Third, I filmed the presentations of the postgraduate research participants, which gave me detailed information on their presentations' content, design, sequence, words (spoken and written) and setting. I believe filming helped me see more details. Specific structures became clearer here than when working with field notes and the digital documents of the presentations simply because of the speed and wealth of information conveyed during the course of the presentations. That said, it is very well possible that I missed seeing other aspects of the presentation because I was focused on filming. Finally, I did participant observation at conferences and seminars, wrote field notes and collected newsletters, call for papers and websites to contextualise the material of the interviews and presentations.

The next section concerned my transcription and analysis. I first discussed the lack of theory on transcription. I then discussed ways of thinking about transcription in conjunction with my own process of doing and thinking about transcription. In response to the literature I read, especially on multimodality (Jewitt, 2013; Kress, 2010), my transcription style

developed from a very detailed one to one suited for the analysis I envisioned. Similarly, in my analysis I had to negotiate and improvise from my initial design. Again, my own reifications played a part in how I approached the material and how I myself had to negotiate and improvise from those reifications in order to understand the various materials I had co-constructed with my participants. My initial reading and looking for themes according to my sensitising concepts (Blumer, 1954) was disrupted by the aspect of ongoingness, which did not fit into themes as I knew and understood them. Likewise, I improvised from my plan to analyse the presentations according to the sensitising concepts (ibid.) when I realised I was interested in how these presentations resembled and differed from ‘traditional’ presentations. This also came in conjunction with reading about a multimodal approach (Jewitt, 2013; Kress, 2010).

Finally, I discussed the ethics of my study. I listed the formal requirements of the university, but explained how these are only one particular and minimal take on research ethics. A more appropriate approach, in my view, is Macfarlane’s (2010) research ethics based on virtues rather than principles. This ethical approach does not prescribe exactly how the research has to be done, but rather takes into account the context in which the research takes place and the on-going and changing needs of the participants. I continued this section with a discussion of power and the way it was present in my research by recounting in detail an interaction I had with Richard, one of the postgraduate research participants. This encounter made evident that power, too, is relational, and does not reside necessarily with the researcher or the participant. Finally, I talked about issues surrounding co-authorship and ways in which I could acknowledge the contribution of my participants in the co-constructed nature of the thesis.

Chapter Four: Making, Breaking and Negotiating Boundaries

Introduction

This chapter shows how my participants negotiated creative methods with their supervisors, peers, administrative personnel, colleagues and others they encountered during the research process and how this shapes the development of creative research methods. These negotiations took place within various communities of practice and on the basis of how the participants and their peers thought research should be done (reifications). This chapter then addresses the first sub-question of this thesis: How do social scientists negotiate the use of creative methods with other members of their research community? Besides the first sub-question, it partially answers the third sub-question: How do creative methods emerge in the process? This chapter also enables me to argue that change is incremental and happens through little shifts in the negotiations over improvisations. This then also leads me to argue that innovation understood as a break with the past is neither possible nor desirable.

In this chapter, I focus on specific situations in which boundary-work (Gieryn, 1983; 1995; 1999) is performed in relation to the research methods of my participants. These situations serve as examples to look at *how* boundary-work is done and by whom, how power plays a role in these situations and how they are situated in and shaped by communities of practice. These examples show that boundary-work is never a simple opposition between adversaries, but a complex practice with many stakeholders whose negotiations of the inclusion or exclusion of research methods are based on different interests. It shows how boundary-work as a concept goes both ways: boundaries can be put up, but can be broken down as well. Nielsen (2008) calls the latter anti-boundary-work. As said in my discussion of boundary-work in Chapter Two's *Negotiations and Power* section, this specification of the concept is not really necessary because Gieryn's conceptualisation does not distinguish between breaking down, blurring or putting up boundaries. However, indicating something as anti-boundary-work does instantly show the intent of the person performing boundary-work, which is the case in one of the examples discussed in this chapter. The examples also show that boundary-work can be performed by both adversaries of creative methods and the users themselves. Boundary-work is done in the service of maintaining good research standards, of how research is supposed to be done. However, as I discussed in the first two chapters of this thesis and elaborate more on in the next section, those criteria are subject to interpretation and negotiation. This is what makes boundary-work possible.

I first discuss the concept of boundary-work and instances in which my participants

talked about a more general sense of not fitting into mainstream social scientific research methods. Then, I delve into the main part of this chapter, description and analysis of four different situations in which boundary-work was done. The first is an example of the negotiation between a postgraduate student and her supervisors in which her methodological approach was contested by the supervisors. The second is an example in which the methodological approach of the researchers was contested by the research office. The third exemplifies two performances of anti-boundary-work by one of the experienced researchers: firstly, she embedded the creative methods more firmly within the academic practice of peer-review and secondly, she put together a bibliography of poetic inquiry, thereby establishing a body of work of peer-reviewed literature. The final is an example of boundary-work done between members of the same community of practice, whereby criteria of assessment were negotiated and established.

These examples then lead me to argue that boundary-work can be done to exclude, change as well as advance the use of creative methods in social scientific research practices. Boundary-work is a continuous and simultaneous practice which impels researchers to make smaller or larger shifts in their use of creative methods. It also urges other participants within these communities of practice to adapt their ideas about what good academic research and whether or not they accept those shifts.

Boundary-work and Power

Engaging in academic research always implies engaging with others in various ways such as: locating your work in the existing literature, collaborations, citing someone's research, having one's own research peer-reviewed, but also peer-reviewing others' research in funding proposals, abstracts for conferences and supervision. In these interactions, researchers continuously apply their judgement of what good research is, and this is communicated to one another. Through these processes, researchers learn what good research is and how to judge it. This is a continuous process, whereby what is considered proper academic research evolves all the time through the negotiations that take place in these previously mentioned collaborations, citations, peer reviews and supervision. From these interactions, new reifications emerge that become the object of new negotiations. These processes are not only continuous, but also simultaneous: negotiations are taking place at the same time, between different people negotiating different – sometimes only slightly so – boundaries. Moreover, academics are often part of multiple communities of practice where different reifications are

in place. In all these encounters, boundary-work can be done (Gieryn 1983).

As I explained in Chapter Two's *Negotiations and Power* section, boundary-work is a concept that builds on the constructivist idea that any attribution of meaning is based on social conventions. The interpretation of those social conventions forms the basis for the inclusion or exclusion of research in an academic community of practice. It is a form of negotiation based on certain criteria, about whether or not research is academic, or whether or not it belongs to a certain discipline. However, as Gieryn (1983, p. 792) points out, there are no essential criteria that define academia or a discipline in particular. They have shifted over time as a result of the on-going negotiations in communities of practice, resulting in new reifications (see for instance De Wilde, 1992). Nonetheless, it is on the basis of those shifting, non-essential criteria that research is included or excluded.

As Gieryn (1995, p. 405) explains, science does not have any essential characteristics that determine whether or not something is deemed as scientific or that even give science the authority it has. ““Unique” features of science, qualities that distinguish it from other knowledge-producing activities, are to be found not *in* scientific practices and texts, but in their representations” (p. 406). In those representations scientists and others, who wish to draw or blur the boundaries of science, make use of previous arguments, attributions, and descriptions of science to (re)construct the boundaries of science. These boundaries are constantly redrawn and as such do not determine what is academic or not. However, “[s]cientific practices and antecedent representations of it [do] form a *repertoire* of characteristics available for selective attribution on later occasions” (Gieryn, 1995, p. 406, italics in the original). That repertoire changes over time and certain characteristics or practices once part of the repertoire may have been replaced by others, which means it would be pretty hard to describe the boundaries of science with those characteristics.

[S]ome maps[, however], achieve a provisional and contingent obduracy that may pre-empt boundary-work. Borders and territories of cultural spaces sometimes remain implicit, matters of personal belief or of such apparent tacit intersubjective agreement that people working together need not explicate “what everybody knows” about the meaning of science”. (p. 407)

Still, many academics perform boundary-work despite the apparent stability of the map, in order “to seize another’s cognitive authority, restrict it, protect it, expand it, or enforce it” (ibid.). The practice of peer review is one way of doing that.

I want to emphasise that peer review is an inherent and necessary part of academia as a community of practice. It is part of the shared enterprise of producing new knowledge. There are academic standards (as discussed in Chapter Two) that come into play at different levels (e.g. research councils, the university, the department or disciplinary-based associations) to uphold what is perceived as the quality of research and they rely on peer review. For instance, research councils provide funding on the basis of peer review. Another example is the Research Excellence Framework (REF). This system to assess the quality of the research in higher education institutions works with expert review. This system, amongst others, provides funding bodies with the information on which to base the allocation of their funding. Other examples of how the standard of quality is upheld are supervision, the year-end progress report of the PhD, the examination of the PhD, peer review of journal articles and debates in articles and at conferences.

At the same time, the definition of good-quality research can be debated as can who defines and judges this definition. Bourdieu (1975, p. 25) points out the circularity of this practice. There is no external measure of quality. The criteria for quality are defined by the same people who are engaged in the practice, in order to fit the work they do; as such, the criteria have a self-referential and self-generating character (Greenwood & Levin, 2003, p. 163).

Furthermore, the criteria for peer review are necessarily broadly defined in order to encompass a sufficient range of topics and ways of approaching them. However, this makes them variably interpretable, opening up the possibility for boundary-work. The following example of peer review criteria,³⁴ I was given for the review of an article, illustrates how different interpretations of the criteria are part of the negotiations on what *is* or is *not* academic. The four criteria were as follows:

1. Does the paper make a significant and original contribution to its subject area?
2. Is the paper well-structured, and clearly argued – stating the issue or issues to be addressed at the outset, presenting and developing a consistent argument, and concluding with a summary statement?
3. Do the arguments presented show an awareness of, and do they engage with, previous writing in the area?
4. And finally 4. Does the paper meet the standards of written expression and scholarly conventions expected in an international scholarly journal?

³⁴ I used these criteria in Chapter Two to illustrate academia's pursuit of new knowledge.

The first, third and fourth criteria can be interpreted quite subjectively, depending on the peer reviewer's background and experience. Yet, it is assumed that it is clear what the subject area is and the reviewer knows this. The discussion about communities of practice in Chapter Two raised the question of whether reviewers share the same interpretation of the subject area; yet, it is on the basis of this interpretation that the reviewer decides whether the article makes an original contribution and whether it engages well enough with the body of literature in the area. Furthermore, the community of practice of the reviewer also informs his or her ideas concerning the standards of written expressions and scholarly conventions. At the same time, because they are open to interpretation, these criteria allow for new lines of thinking and new ways of doing research, thereby pushing the boundaries of the subject area.

Doing boundary-work implies exercising power, possibly contesting and maybe even subverting it. Although it is often easier for those with scientific authority to draw the boundaries (Bourdieu, 1975), other sources of power such as economic and social capital play a role here too. These often come from stakeholders such as the government and commercial companies and therefore play an important role in the production of knowledge and the context in which it is embedded (Etzkowitz & Leijdesdorff, 2000; Hessels & van Lente, 2008; Leydesdorff & Meyer, 2006; Pestre, 2003). It has been argued that the relationship between university, government and business has become more entangled (Gibbons, Limoges, Nowotny, Schwartzman, Scott & Trow, 1994), though Pestre (2000; 2003, p. 245) stresses that there has always been an interaction between modern science, governmental institutions, courts, military institutions, businesses and society. In one of the examples he provides, he shows how in the eighteenth century natural history and agronomy were

organically linked with major social projects, as the acclimatization of exotic living organisms, which was seen as having great financial potential for states, entrepreneurs, and peasants; and with the moral reform of society as a whole. Natural history and agriculture were two aspects of the same enterprise aimed at the development of natural resources, the advancement of commerce, and the improvement of the nation's wealth. They were advocated and practised by the same people, both in the Jardin du Roi and on private estates (Buffon's property in Montbard, for example); and the same people served as experts to both landowners and the Crown. Natural history and agriculture also provided 'solutions to the problem of the moral and physical degeneration of the nation', a major concern of the

time, ‘and these sciences came to embody the concerns for social reform of many individuals who were later to be involved in the French Revolution’. (p. 248)

The production of knowledge was clearly intertwined with the goals of social, economic and political projects, and research was not just done on the premise of knowledge for knowledge sake. As discussed in Chapter Two’s *Negotiations and Power* section, Latour (1999) also argues that the scientific cannot be viewed separately from the political or economical; they are always in interaction. This means research is never only shaped by those with scientific authority, but by those with scientific, social, political *and* economic authority. In recent studies, globalisation and cuts to funding have been emphasised as pivotal in the interaction between the universities and other stakeholders (Slaughter & Leslie, 1997; Ziman, 1994; 2000; 2003). The ‘impact’ of research is a current example of how this interaction between academia and businesses, social and governmental institutions is affirmed. Impact refers to the relevance and usefulness of the research outside of academia, and has become a criterion to judge and fund research on. From the examples of Pestre and Latour, however, it is clear that the interaction between academia and ‘the outside world’ is not a new development. The impact agenda seems rather a continuation of what has been happening for a long time. It has been heavily contested, though for the way in which impact (and thus the relationship between academia and the different stakeholders) is quite narrowly defined in economic terms (see for instance Colley, 2014; Francis, 2011). From this, it becomes clear that there are many people involved in the process of research who all have their own reifications in regards to research from which they (re)draw boundaries. I now turn to these situations in which boundary-work is performed.

Hints of Boundary-Work

While the use of creative methods in research is often seen as acceptable by reviewers or others judging the quality of work, there are also situations where the methods or the methodological approaches my participants use are questioned. Participants would talk about boundaries that were hard to substantiate but felt nonetheless. As Matthew, a visual anthropologist and one of the experienced researchers said: “There’s a sort of institutional resistance to [a more art-based approach to visual anthropology] that operates at sometimes quite an explicit level, sometimes a much more – well, it’s just not what anthropologists do, is it?” (Matthew, interview, 7 December 2011). The implicit level Matthew refers to works on

the assumption that it is clear what anthropologists do and do not do. It also denies anthropologists from changing their practice.

Sometimes the perceived antagonism can be enough to push my participants' work in certain directions. While power is thus not always actively exercised, it is always present and plays a role in how each situation plays out. Bart, also one of the experienced researchers who used a collaborative narrative and performative approach, for instance, said that he is "still considered to be quite a wacky person, here – in, even within, my own school of education" (interview, 29 March 2012). He also said it was relatively easier to get his work published in methodological journals, but his goal was to publish in other more mainstream journals. William, another experienced researcher who collaborated with Bart in the narrative and performative approach, talked about colleagues who were going for tenure and were advised not to write in more creative ways because it was not seen as valid research. "Whatever that may be," he added. Finally, Adrienne, an experienced researcher who used a narrative approach, responded to a question concerning the acceptance of her work at conferences by saying that none of her abstracts had been rejected. However, she instantly nuanced this by emphasising that the conferences she submitted to were those that supported narrative approaches to research the lives of chronically ill. She thus put her work out in contexts where it was likely to be accepted. Although there was no actual boundary-work performed in the sense that her work was rejected because it lacked, according to academic standards, Adrienne was well aware of the possible antagonisms towards her approach.

It was hard to substantiate such claims with 'evidence', but they were nonetheless experienced as such and thus had their influence on the work of my participants. This influence becomes clearer in the following four sections where I discuss explicit examples of boundary-work.

Hilde: Negotiating Methodological Approaches

In this section I discuss Hilde, one of the postgraduate research participants, and her use of participatory visual methods and the negotiations she had with her supervisors. This is a clear example of boundary-work on the part of her supervisors. This example also shows how researchers' involvement in different communities of practice can cause tensions when their practices do not completely align. Those practices concerned the application of the method (i.e. the number of participants, the kind of data Hilde gathered and most importantly *how* she gathered that data). Finally, I show how Hilde negotiated the differences between the different

communities of practice.

Hilde used a participatory action research approach to look at a geopolitical issue and the tensions this caused in a specific area in Latin America. As said in the introduction of Hilde in Chapter Three, such an approach in research is fairly common in Latin America. She collaborated with local people – the participants in her research – and a team comprising a cinematographer, a plastic artist and a researcher. Hilde was the main coordinator of the project as she had written the research proposal, but her team and the local people *together* made the documentary concerning the geopolitical issue and the local people's thoughts, feelings and experiences related to this issue. In addition to the team and the local people, Hilde was also part of a political and social research collective whose members are all involved in working and campaigning on the geopolitical conflict from their own expertise. Hilde told me many times about the collective and it was clear she was passionate about their cause. It was one of the reasons why she was doing the PhD, as it would provide authority for her to speak out about this issue. In one of our conversations she called academia a legitimizing discourse:

And I thought that someone should say it [the reasons behind the geopolitical issue] and say it louder and say it in a way that it could convince people – not only like a cafeteria talk, but just giving like good information, quality information for supporting that. So I decided that I had to do it [research the geopolitical conflict]. It was my... I cannot live if someone doesn't do it and yeah, so I will do it. (...) And then, so I thought okay I have to do that from, in academia, because academia is legitimizing discourse for... especially in [Latin American country]. We don't have a lot of academics having a PhD. (Hilde, 20 January 2012)

Besides Hilde's three communities of practice – the Latin American academic community, her research team and her participants and the political and social research collective – there was at least a fourth one, even if only for the duration of the PhD: the European academic community of practice of her discipline. It is this one that I focus on here, but her participation in the other communities of practice is important to understand how she negotiated her methods with her supervisors.

The PhD trajectory involves an on-going process of negotiation between the postgraduate researcher and the supervisor. Regardless of quality or approach, the work of the postgraduate research student is constantly being monitored by the supervisor. The research

proposal gets accepted, the next steps in the research discussed and draft chapters are read and corrected. These moments of negotiation can be struggles between the postgraduate research student and the supervisor, but they can also be positive and in agreement. In Hilde's case, her methods evoked both encouragement and scepticism from her supervisors. As said in Chapter Three's introduction of the PhD students, she used a participatory action research approach. Her supervisors were sceptical about this approach because, as discussed in Chapter Two's *Research Reifications* section, it was not how research was supposed to be done. Because the research was done in collaboration with the participants who were not academically trained, her supervisors were worried that the research would lack academic rigour. Such an approach put its credibility on the line (Nielsen, 2008, p. 173). While preparing for her fieldwork and in the dissemination phase of her research, Hilde had to convince her own supervisors more than once of her methodological approach.

They have very much focused on my methodology. I don't know if they always do that. It is my first time [that] I have two supervisors and so I don't know how it works and how it worked for other people. But my feeling is that they – since the beginning, they knew that I was going to have a participatory action approach as an umbrella for all the different methods I'm going to use. They have never used that method or that approach in their research. So I think they were very, not distrustful... but unsure of what I was doing. So the first time they... I had to design a very detailed sheet of what I was going to ask, who I was going to work with, why. Very, very... and I think it was a bit too much and it was because they... they didn't... I don't know... They weren't confident enough with what I was doing. Because it's not what they have been doing and what they have done themselves and what [they] support other[s] to do. (Hilde, 20 January 2012)

The methodological approach Hilde wanted to use in her research not only differed from the research practice of her supervisors, but also raised questions from them. To make sure Hilde's work would be good enough, her supervisors asked her to write a detailed proposal. Although Hilde was unsure if this was normal practice, she felt this was a way to monitor her and that it was unnecessary. Still, she complied with their request. In this way, her supervisors exerted power over the way she conducted her research. Even though they did not prohibit Hilde from using the participatory action research approach, they did make it harder for her to continue her research in the way she wanted to.

Asking Hilde to write such a detailed outline of her methods and methodological approach signals her supervisors' lack of familiarity with and confidence in the participatory action approach Hilde used. The extra work she had to do as well as the lack of her supervisors' full support made her visual methods approach a more challenging endeavour. However, Hilde was adamant about using this approach. As I mentioned in the introduction of this section, she was involved in other communities of practice. For the social and political research collective she was a part of, doing participatory and emancipatory research was normal. She told me about the time she first encountered participatory action research and how it had inspired her to do research again. In addition, she was already in contact with the local municipality, organisations and community she was going to do the research with and certain agreements were already made. So for Hilde, there was no question whether or not she would use this approach.

Another way in which Hilde's supervisors took control of the research process was by closely monitoring the steps she was going to take. The detailed description of methods they requested made the research process transparent and verifiable (Bryman, 2008, pp. 66-76, 664; Seale, 1999, p. 189), which made it easier for her supervisors to peer-review and guard the boundary between what is deemed academic and what is not. They could also check the amount of work the production of the documentary entailed and what kind of data it would provide. Hilde had prepared eight workshops to train her participants in the production of the documentary. Because her supervisors were unfamiliar with the approach, they first thought eight workshops would not provide enough data for a PhD, which had to entail a substantial amount of work. Hilde had to make clear that the amount of preparation for one workshop was much more work than preparing for and conducting one interview. It also provided much more material than an interview, she argued.

Another, more important, aspect of the detailed description was that her supervisors made her think through the process of the research beforehand, which is considered an important part of academic practice (Bryman, 2008, pp. 66-76). Generally, research should be well thought-through and different possibilities should be examined beforehand. Choices should then be made on the basis of these careful deliberations. Asking Hilde to write the description, her supervisors tried to manoeuvre her into a more transparent and verifiable (i.e. academic) approach. Particularly in relation to the participatory approach of the documentaries, her supervisors were worried about the academic level of the work. They were happy for her to use visual methods, but they were wary of her participatory approach. Hilde told me that as part of her approach she remained very open to changes and flexible in the

research process. One supervisor especially urged Hilde to guide the research process more to ensure she not only got social outcomes, but also research outcomes. Hilde, by contrast, did not make this distinction. For her, these were one and the same, and the social outcomes were important to her. Therefore, she chose to continue her work along this line of thought, rather than conform to her supervisors' wishes. She said it amounted to tension between them, but that it was a political decision she had taken. It is clear Hilde could make that decision because of her participation in the other communities of practice (i.e. the social and political collective and the people with whom she was conducting the research) and her stronger personal alignment with them. In the following exchange, she discussed it some more.³⁵

Pauline: And you were talking about your supervisors knowing that visual methods...

Hilde: Ah, yeah, yeah. So, [they said about the visual methods] "Do it, do it!" And yes, well, I design [the research plan] and because it was very, very specified (...) [everything] I was going to do. They say: "Okay, yes do it." First I said "[photo] albums", and they say "good." Then I say "documentary", and they say "good." So [they] didn't really care much about... They were more worried about [it] being participatory. They've always been more worried about that. Okay that was fine, but then now... They have been quite critical about how academic the product is. And... they have a different idea of what academia should be in society. And I think it's a more puristic conception of academia, more orthodox conception. I think it is because of the Northern context in which they have lived academia. Because, for me, one challenge in academia is to contribute to social change. And that is not different from academia, it's part of academia. (Hilde, 20 January 2012)

Hilde refers here to the difference in conceptualisations of research (whether or not social change is part of what research should accomplish) in the two academic communities of practice of which she is a member. As I explained above, boundaries are arbitrary (Gieryn, 1983; 1995) and the line between critical qualitative research and the participatory action research approach is indeed sometimes very thin. Hilde explicitly said in the previous quote that the creative *method* itself, film or photography, was not rejected by her supervisors as a valid research method for her project. Earlier on in this interview, she said her supervisors

35 I address the underlined phrases in the next section.

were keen for her to use visual methods; they had heard it could be interesting. What they were worried about was her participatory *methodology*, which has implications for the way she uses the visual methods. As she said: “*They’ve always been more worried about that.*” This thus created tension throughout the process and continued into the dissemination of the research. Hilde said: “*Okay, that was fine, but then now...*,” turning to the situation as it was at that time and indicating the struggle over the methods was not over yet. The debate had now turned to the documentary itself and whether it was academic. This point was repeated to me by one of her supervisors at the social gathering after the screening of the documentary (for a more elaborate description of the presentation, see Chapter Six’s *Creative Methods and Presentations* section). Her supervisor and I were talking about the documentary and she said to me she had often questioned Hilde about the documentary and its relevance as an academic piece of work. In this process, Hilde was thus continuously challenged.

Clearly, Hilde was constantly walking a thin line, pursuing her own academic goals in keeping with her social and political principles while trying to keep her supervisors happy with the work she was doing. Ultimately she managed it, as she was awarded a PhD. However, along the way, there were tensions, as she told me in later conversations and also referred to in the preamble to her thesis: the European academic community of practice was cautious towards her Latin-American activist approach. There, Hilde wrote: “I had to develop a strong methodology and epistemological framework that was sufficiently academic – and not just activist – in a university that was not used to the participatory and action oriented approach I was proposing with which to address the local conflicts creating displacement in [Latin American country]” (preamble to Hilde’s thesis). Together with the detailed description of her methodological approach, this shows where Hilde had to do her own improvisations and the ways in which little shifts were made in the participatory action approach she used.

Her thesis consisted of four published or ‘in press’ articles accompanied by theoretical and methodological frameworks and a conclusion. In that written dissemination of her work, the creative methods and the participatory action approach took up an important part: they formed the bulk of her methods, and in one of the articles she reflected on her use of them. The creative dissemination, however, was not part of the thesis. For her PhD, it did not matter that her documentaries had been shown at various film festivals or had empowered her participants. That was not part of obtaining the PhD; only the written thesis was. On the other hand, her thesis was based on her participatory action research and has been accepted as such. The articles on which the thesis was based were published in peer-reviewed journals with

impact factors of 1.5 or higher. In addition, the supervisor who was most sceptical of her approach co-authored one of the articles. These are all signs of redrawn boundaries and the little shifts both Hilde and her supervisors made.

Of course, the obtaining of the PhD and the article publications were only specific moments in the on-going process of negotiation and the power relations developing in those. Nonetheless, they were an outcome of a much longer process in which the research had been developed, conducted and disseminated, all the while under the scrutiny of her supervisors, peer reviewers of journals, the participants in the research and the political and social research collective Hilde was a part of. What's more, they were met with (sometimes reluctant) approval. As such, they are new entry points in the negotiation of what research is and how it can be done; they offer avenues to new reifications, but also to new boundary-work.

In this example, Hilde's engagement in different communities of practice brought different reifications of research together. This resulted in boundary-work performed by her supervisors because they initially did not deem the participatory approach academic enough. Still, through extensive dialogue (through her detailed methods chapter and research design) and Hilde's persistence to use this approach, little shifts were made in both parties' reifications of what research was. Hilde did this in developing a "strong methodology and epistemological framework that was sufficiently academic", as she wrote in the preamble to her thesis. Her supervisors did this through their acceptance of her participatory approach to research.

Bart and William: Negotiating the Regulations for Research Degree Programmes

In the following example of boundary-work, I discuss the submission of Bart and William's thesis, which was encumbered by the examinations office. In this case, the community of practice involved in the process of writing and, notably, in submitting the thesis, consisted of the PhD candidates, their shared supervisor and the examinations office. The participation of the examinations office was only minimal in the sense of actual involvement in the process of writing, but all the more crucial when it came to the submission of the thesis. The regulatory framework of the university submission guidelines (indeed they are guidelines, not rules), which is in a recursive relationship with the work of the PhD students, did not overtly provide for this kind of work, even though the research did not lack in academic quality. The examinations office's performance of boundary-work did not relate to the academic content of

the thesis, but to the formal guidelines (reifications) concerning the submission. Thus, the negotiations of the three parties concerned the way these guidelines were differently interpreted by the participants. Even though the formal rules were not changed, their possible interpretation was opened up and their PhD submission set a precedent.

Bart and William were both participants of the experienced researchers group. I interviewed them separately, but because they often worked collaboratively, they would frequently refer to each other in their interviews. Bart and William had started working together when they wrote their PhD thesis. It was a collaborative endeavour in which they explored writing as a research method and they together wrote one thesis. Their work can be labelled as performative social science, auto-ethnographic and collaborative, and they used different styles of writing: academic, poetry and fiction. As they explained to me, a collaborative PhD was quite unusual, but it had been done before. Nonetheless, before Bart and William started their PhD, Hanna, their supervisor checked with the director of programmes, the graduate dean and finally the examinations office to see if and how such a joint project would be possible. The director of programmes was supportive and the graduate dean agreed there were no formal impediments for a joint project, though the submission guidelines were designed with single-authored theses in mind.

Additionally, Bart and William had to meet certain extra requirements. For instance, the length of the thesis had to be twice as long, and it had to be clear who had written what, so the doctorate was clearly earned by both. Finally, they needed to methodologically justify the joint submission. Still, their supervisor, Hanna, had been warned to expect rigorous scrutiny, but it was only when Bart and William both handed in their theses at the examinations office that questions were raised about their work. The way in which they had written it was heavily contested. During the process of writing, the division of who had written what had become blurry. Informed by a narrative approach, their work had become so intertwined that even though it was clear who wrote what (as William explained to me: “That’s how we wrote: [it] was very clearly me writing, and then Bart writing.”), the individual texts did not make sense on their own anymore. Bart and William remained unaware of the problems their collaborative work would raise, because Hanna did not inform them of the negotiations she had to do until just before they had their defence and the negotiations between her and the examinations office had been settled.

The thing is that Bart and I... thought... that really... we were, we lived and wrote in the illusion that we could just do what we wanted, right. Meanwhile... Hanna was

doing all this kind of... these political negotiations and creating the space for it to happen. I mean and, until, till really, very close to the end, we were not aware of any of that. She was just coming back to us saying: "Yeah, that's fine, you go ahead." And we just thought... that didn't take very much.

[Both laugh.]

William: But, so Bart and I were just doing our thing. But... but... you're right. I mean, I, and that piece does show that [referring to an article that he wrote together with Bart and Hanna on this process]... that, um... it was very political. What we were doing had... er, it took a lot of work to get through and, and wasn't, wasn't straightforward and, and um, was contentious... But, but on a day-to-day basis, up, right up and till the, the month before our viva... we had... no idea. We, we were just merrily writing away.

[Both laugh] (William, 1 March 2012)

In the weeks prior to the viva, the examinations office did not immediately accept the submission of the thesis or send it on to the examiners. The examinations office was unsure whether the thesis violated university guidelines, and what followed was a negotiation between Hanna and the examinations office over its legitimacy. Questions about whether the thesis had gone through the university's proper approval processes, what kind of authorisation the project had gained beforehand and formal definitions of what a thesis should look like all became the objects of dispute. Although improvisations of the university guidelines for thesis writing and submission are happening continuously, in this case the interpretation of the guidelines become a case of boundary-work (Gieryn, 1983). Hanna and the examinations office each had different ways of exerting power to enforce their interpretation of the guidelines. Hanna, as a professor, had symbolic power in the form of what Bourdieu (1975) refers to as scientific authority. The examinations office had its own authority to exert power, being the arbiter of the university guidelines regarding the submission of PhD theses. The boundary-work here was done on the basis of administrative rules and the party challenging the submission of the thesis was managerial. The guidelines in the code of practice were not explicitly against a joint submission – there were no formal reasons to reject the theses – but they had been based on the way theses were usually written, which was single-authored. It

said nothing about the quality of the academic content of the thesis³⁶ and this was actually not an issue; during the viva, their work was instantly accepted as a strong piece of academic writing. Clearly, Hanna managed to persuade the examinations office, and Bart and William passed their vivas, creating a precedence for this kind of approach.

This example shows boundary-work can also be done on the basis of more formal and representational qualities of academic work. The example also shows that those who interpret the formal and representational qualities and have the power to push through those interpretations can be non-academics. Furthermore, the university guidelines concerning the writing and submitting of the thesis did not categorically accommodate collaborative writing. In this case, the improvisation was one-sided. Bart and William did not adjust their thesis, which meant the examinations office had to do an improvisation and made a shift in their interpretation when they accepted the thesis. This might stay a one-off case, because the guidelines in the code of practice of Bart and William's university have not been adjusted. Nonetheless, the range of interpretations has slightly broadened and opens up possibilities for other improvisations and negotiations of these reifications.

The two cases of Hilde and Bart and William are examples of research that were judged on the way they were done, but they differ in terms of how boundaries were put up and by whom. At the same time, they both concerned the way academic work is 'supposed to' be done. As I discussed in Chapter Two's *Research Reifications* section, the way academic work is supposed to be done does not refer to a fixed set of rules. What's more, those rules and guidelines that do exist are subject to multiple interpretations as these two examples have shown. In the process of these two examples, then, the boundaries of what is considered academic work and how academic work can be done are slightly redrawn again and again. In these two cases, it is the research itself and the use of creative methods that were accepted within existing practices. Yet, there are other aspects of the process of doing research where boundary-work can be performed enabling creative methods to develop, which is discussed in the following two sections.

Mary: Performing Anti-boundary-work

In this section, I discuss two examples of anti-boundary-work (Nielsen, 2008), both done by Mary, an experienced researcher who used poetic inquiry to convey her research.³⁷ Anti-

³⁶ Interestingly, collaboration within the academic world is quite normal. For example, no one asks the authors of a journal article to make clear what each has written.

³⁷ There are many other names used for this kind of creative method. Examples are poetic transcription, poetic

boundary-work is a specific form of boundary-work, where the goal is explicitly to break down the boundaries. In these two examples, Mary breaks down boundaries that hinder poetic inquiry in its acceptance as an academic method. First I present why Mary started using poetic inquiry and then discuss and analyse her performances of anti-boundary-work. In discussing the way Mary does anti-boundary-work, these examples firstly add to the kind of boundaries that can be drawn and broken down, and they show the means with which those boundaries can be negotiated. This links directly to the second point of the examples: they show how methods are negotiated. Thirdly, they show how creative methods become embedded within academic practices.

Mary had a background in drama, teaching English poetry and had done her Master's in education. She told me she started using poetic inquiry as a direct result of a class she took during that Master's called "Writing as research". It was there that she read Richardson's *Fields of Play* (1997) for the first time. The class, as well as the book, became inspirations for her Master's thesis. Mary had been struggling to find an appropriate way to represent her participants' voices. She said:

There's been an extended dialogue and then what you see in the final, um, thesis or case study or art-research article is a snippet, right? And, and, generally speaking, a snippet that proves the point the researcher wants to make.

Pauline: Yes... yes.

Mary: I was deeply concerned around this notion of voice, because [what] I [find] – and that I can track directly back to my theatre background – is that voice is one of the most, you know. In, in theatre you have two main tools. You have your voice and your body. (Mary, 29 February 2012)

In this 'snippet' and throughout the conversation we had, Mary referred many times to the power dynamics involved in research and the 'crisis of representation' (Schwandt, 1997), which have been an important influence on the emergence of creative methods, as I explained in the introduction of this thesis. She felt academic language was often reductive in the way it conveyed participants' experiences and theories (a snippet of an interview, a block quotation

representation, poetic narrative and research as poetry (Prendergast, 2009, pp. xx-xxi; van Romondt Vis, 2007, p. 71).

of a theorist, a reference). To do more justice to the experiences or theories was an important impetus for her to use poetic inquiry. In addition, her background in drama and teaching English poetry made her inclined towards such a poetic inquiry approach.

One way in which Mary performed anti-boundary-work was in showing there was indeed a field of poetic inquiry. Richardson (1992; 1997) was a huge influence on the development of poetic inquiry; as such, she is cited a lot by researchers who use this method. However, as Mary told me, a senior academic had told her she should cite not only Richardson: “[S]urely there are other people than Laurel Richardson doing this work.’ And I said: ‘Oh right, I think there are.’ And he said: ‘Well don’t just think, why don’t you find out?’” (Mary, interview, 29 February 2012). This led Mary to compile an annotated bibliography with peer-reviewed articles using or writing about poetic inquiry (or any of the equivalent terms named in footnote 37). Mary insisted the articles were peer-reviewed, so their quality could not be disputed. Note how she sees the peer review as a stamp of quality, a point I return to later on in this section. The extensiveness of the bibliography was another way to convince. At the time of the interview, the bibliography contained over 200 references, indicating an emerging field.

The bibliography became a resource in two ways. It provided those who were new to poetic inquiry with a body of work to start exploring this approach. More importantly, it was a body of work that could not be denied and, as such, became a resource to negotiate boundaries with.

It, it becomes very easy to quell any concerns of a committee member who says: “Well, I don’t know what this poetic inquiry stuff is and why you would want to do it.” To say: “Well, actually, there’s an annotated bibliography out there that has over 200 peer-reviewed citations of the, that use poetry in qualitative research in the social sciences. And just the fact that that bibliography exists is often enough. This is not just me saying this. This is grad students who reported this to me. [It] is enough for supervisors and committee members to go: “Oh! Okay then, that’s fine. (...) Because, because this is how things get legitimated in academia. If enough people are doing it and doing it successfully, that creates a sense of legitimacy. (Mary, 29 February 2012)

Mary argued that considering its size, the annotated bibliography could be used to inform academics about poetic inquiry and legitimate it. She concretely pointed out the social,

constructed and negotiated nature of research and used this to her own advantage. Mary's bibliography thus becomes a 'tool of persuasion' (Gilbert, 1977, p. 115). Gilbert argues that having references to earlier work – or in this case, a whole bibliography – support the research because it shows how the research is related to other work in the field. In addition, it shows how the research advances earlier work (p. 116). It also “establishes the authority on which the author's argument is founded” (Gilbert, 1976, p. 287). The bibliography thus offers a plethora of 'tools of persuasion'. However, as Gilbert indicates quite rightly, if the readers know the work or author cited, they do have to be convinced by the individual work or the author in general (ibid.). Becher and Trowler (2001, p. 114) also say that referencing other researchers' work helps “to reinforce the statements you put forward” (p. 114), but even more, “it suggests that you are keeping good company” (ibid.). Moreover, the bibliography indicates that the field of poetic inquiry exists and, in putting together the literature, the method and its community of practice that employs it becomes more visible and established. “[T]o bring texts together presenting them as a school of thought [offers] guidance to readers wading their way through (unfamiliar) territory” (Bruffee, 1986, p. 773). Creating the bibliography thus helped embed poetic inquiry more firmly within academic practices such as referencing (Gilbert, 1977), and the bibliography became a form of cultural capital which provided researchers the means to exert scientific authority. At the same time, the research in the bibliography was more easily cited, which enhanced its authority (Gilbert, 1976, p. 287). This made it harder for others to perform boundary-work and reject the method.

In the same vein, Mary wrote a book chapter on the review process for poetic inquiry. Again, this enabled the method to become more firmly embedded within academic practices. When Mary had sent out an article to a journal, one of the reviewers criticised it for her use of poetry. It was not the use of poetry, as such, because the journal welcomed it, but the reviewer rejected the way in which Mary had used it. She told me reviewers, in general, did not necessarily know how to review poetry in this context and it was difficult to find reviewers who did. This contradicts Mulkay's (1972) claim that research in new areas is more easily published as there are fewer criteria with which to judge the work. It rather seems that the lack of criteria (or the inappropriateness of existing criteria) prevented Mary's article from being published. When Mary was then asked to write a book chapter on how to peer-review articles using poetry, it provided her with a perfect opportunity to resolve this issue. Mary hoped the chapter would provide reviewers without a background in poetic inquiry, with the tools to say something meaningful about the research, rather than rejecting the work simply because they could not apply the criteria they knew. Obviously, this does not guarantee that

articles with poetic inquiry will be accepted, but it becomes easier to peer-review this kind of research, thereby embedding it more firmly within academic practices.

The chapter Mary wrote also serves another purpose. While it provides reviewers with guidelines on how to judge the quality of research using poetic enquiry, it also sets a standard for researchers themselves concerning what that quality should be. Earlier, Mary had made a point of peer review as a stamp of quality for the articles in the bibliography; with this chapter she made it possible for poetic inquiry to be awarded the stamp. In this sense, it is a nice example of the way communities of practice work. The chapter Mary wrote is both a product of her participation within the community as well as a reification of new practices. It is a negotiation of earlier criteria for peer review (reifications) that did not provide for these ways of doing research anymore. The improvisations and little shifts had turned into new (tentative) reifications. The acceptance of these reifications in the field is yet to be seen, but the invitation to write the chapter is a sign of the need for change. As I explained in Chapter Two, this is part of an on-going process in which these reifications become part of new negotiations. The book chapter will be reviewed and peers will critique it, add new ideas and use it in their own work. While this helps establish the practice, it also offers new ways to put up boundaries.

In both of these examples, there are no clear antagonists who perform boundary-work, except for the one peer reviewer, but there is a clear protagonist. In both cases, the possibility of boundary-work is part of why Mary does these things. Power thus is not exercised directly by anyone, but emerges in this situation through her and others' awareness of academic practices and how to use those practices to the advantage of poetic inquiry. It is thus a nice illustration of the way power is "a relational effect of social interaction" (Allen, 2003, p. 2). The bibliography and the book chapter Mary produced are resources. Still, as Allen indicates, these can help exercise power, though the mere possession of resources does not equal power. They have to be applied correctly, and they can also be used against you. It can be a way to enable the review process, but does not hold the ultimate power over it (p. 5).

In this section, I discussed two examples of boundary-work. These examples show how boundary-work pertains to academic practices other than the conduct of research and the use of the methods themselves. It also concerns enabling the practices surrounding the use of those methods. Mary performs anti-boundary-work because the bibliography and the chapter on peer review both enable researchers to use poetic inquiry within academic research. These examples thus show how other practices surrounding the conduct of research also need to be adjusted. Another example would be the academic dissemination of research, using visual

methods, that is conducive to the publication of quality photography. To publish poetic inquiry, it needs to be peer-reviewable. Furthermore, it is also clear that Mary is aware of academic practices such as boundary-work, and uses them to her own advantage. In these ways, she is able to potentially subvert certain power mechanisms by providing resources with which power can be exercised. These resources still have to be used competently, though, as Allen points out (2003, p. 5).

Sophie: Negotiating Standards of Good Research

Their openness to creative methods did not mean my participants were not critical of the way they can be applied. This example shows how researchers themselves do boundary-work on their 'own' creative methods, by being critical of the way in which creative methods are being taken up. In doing so, they show that such methods are already subject to certain criteria and researchers are actively developing the academic criteria for judging such methods.

This example involves Sophie, one of the experienced research participants who used visual methods. In our interview, she criticised other researchers' use of visual methods, but her own work had also been critiqued. Both of these facts point out again that there are criteria to judge these methods on, contrary to what Mulkay (1972) claims. These criteria involved academic as well as visually aesthetic criteria. The example additionally shows some of the issues that some social scientists have, such as a lack of training in the visual – both in terms of its aesthetic, but also the intellectual, academic ideas a photograph can convey. First, though, to show Sophie's attachments and how her criteria developed, I need to explain a bit about Sophie's history with visual methods and the communities of practice she was involved in.

Sophie was a professor in sociology at a British university. In her research, Sophie often collaborated with photographers. She first started using visual methods because she had been working with mentally-ill participants who were unable to articulate their lives verbally. When she saw an exhibition of photography made by mentally-ill people, and saw how full of information these were, she realised she needed to capture their lives with visual methods. She decided to work with a photography student. While doing the research, there was some dialogue going on between her and the photography student about how the photographs worked in relation to the written work she was doing. Eventually, the research they did together ended up in a book. However, as this was the first time she had worked with visual methods, there were things about the photographic production process that she did not know

yet, which she felt meant the photographs could not reach their full potential.

The production values weren't great. And I didn't... realise that you had to stay on those things. Subsequently, you stay on with, on it with publishers and their designers to make sure the photographs look good, um, but he [the photography student] wasn't fussy about that, and I didn't understand how you did that so the book looks as it does, you know. It's poorly designed, visually. (Sophie, 7 December 2011)

Here Sophie started to learn that using photography within social scientific research requires having more than just having good photographs. Just as in photography books, the photographs need proper visual design so the photographs can transcend being merely illustrative and also work on an analytic level.

In this process, Sophie started to get a sense of the criteria for visual methods. As she told me, these were made even clearer through the criticisms of her work by another photographer: “And he is quite critical of [the book] in terms of its look... [We both burst into laughter]. So I, okay then, you know how better to do this. Let's have a go. So I invited him into my [new] project” (interview, 7 December 2011). In this project, the dialogue between them was much more intense, partly because of his critical perspective on her earlier work, partly (and very importantly) because he was a professor in sociology too. In the following two quotes she talked about how the process between them worked.

Because he's a professor too, I suppose there was much more. And a sociologist, there's much more of a dialogue about what the photos are doing, what work they're doing, how we think about them, and... you know how, he kind of... You know, because we would hang out together in [place X] and how he would try and work visually with what I was working with, with a recorder and in terms of my notes and sketches and stuff like that. So dialogue, much more of a dialogue begins there. And it, it goes through the fieldwork. But especially, it reaches a crescendo in the production of the book, (...) and there's a big discussion, I, you know, I go to [where he lives], he comes to [where I live]. There's a knockdown fight about how the book should look...

But we argued a lot about whether the photo repeated the text. Because I think you, you write with images, you don't write about images. So you render the images in the

narrative. You don't need to repeat yourself. You wouldn't write it twice, why would you do it twice with images. So then that leads to discussions, like what work images do, and how [they do it], what works for the text or where they're placed in the book. (Sophie, 7 December 2011)

At the time I interviewed her, Sophie was working with another photographer on a new project and taught in a visual sociology programme where professors in photography and in sociology taught the curriculum. In these various communities of practice, collaborations between the different disciplines were encouraged. By being part of them, Sophie was able to learn through doing, as she said herself. In these collaborations, she said there was a continuous discussion on the interaction between photographs' visual aesthetic as well as sociological qualities. Through these negotiations, criteria are formed concerning what constitutes good visual research.

Indeed, the following is an example of how Sophie argued for discussions on the quality of work done with visual methods. She abhorred how some academics uncritically embrace visual methods in the race towards novelty in research and how that lack of a critical approach led to low quality research.

I think probably the visual sociology I'm most critical of is what seems to've come along, and I suppose it's part of a rush to use the visual in everyone's research. Everybody now has a PowerPoint. You know, it doesn't matter what they're doing and often they've just got words on, but there's a rush to the visual, and I think some of the worst kind of that research is "This is what we're doing. Here's a picture of it." And I find that really banal(...), unless it's a landscape we can't imagine. If it's somewhere on a garbage dump in Addis Ababa, thank you, I'd like a photograph of it, 'cause I can't imagine that. But if it's like, you know, here's number 4 bus: yeah, thanks [I-knew-that-already intonation][laughing] And?!? So, you know, I think that some of that, it's led to some really low-level analytical work and people don't think about how you use... the visual to... well, first of all, to do your research rather than to just illustrate it and how you use it to do the analysis you're gonna. How it works in the analytical register. And I think the visual can add... all of those things. I'm not saying only the visual can do it, but I think they do them in a particular way and they do them well. And so we need to think about those more penetrating questions rather than doing that very banal, you know, "Here is a picture, and this is what we're

doing.” (Sophie, 7 December 2011)

Participating in the different communities of practice – her collaborations with the different photographers and the course she has set up and is teaching – meant she had to negotiate numerous times the conduct, analysis and dissemination of visual methods from a social scientific perspective with an aesthetic one. In these moments, Sophie learnt and created an understanding of what the visual can and should do, thereby actively shaping the criteria for visual methods. It established a framework from which to do boundary-work by dismissing the research that only uses the visual as embellishment. Without an analytical part, these visuals are not part of a method as far as she was concerned.

Even though Sophie did not dismiss specific research (i.e. the study of Dr. X or Professor Y), she did perform boundary-work by outlining what she considered to be proper research. In doing so, she helped establish the criteria for these methods in her participation in various communities of practice.

Discussion: Boundary-work, Communities of Practice, Power and Innovation

Boundary-work never takes place between polar opposites. It rather occurs between those practices which only slightly differ, whose similarities then need to be de-emphasised and differences accentuated – or, depending on your standpoint, the other way around: similarities emphasised and differences minimised. It is then less of a surprise that my participants only made small shifts in reaction to the boundary-work performed towards their research. The improvisations on reifications of a community of practice are always negotiated with other members of this community. As such, power always plays a role in these negotiations. Boundary-work only highlights it. Nevertheless, these small shifts indicate a sophisticated dealing with power relations. In this section, I say a bit more about these power relations, boundary-work, innovation and change.

The first two examples show that both Hilde and Bart and William did not change the nature of their research, but still managed to get past critical questions. Hilde did this by embedding her work more firmly within an epistemological framework. Bart and William did this by pointing towards other interpretations of the university guidelines on thesis submissions. They also pointed out how their theses had gone through the appropriate channels of approval. The same resources that could be used against their work were used by them in their favour. Mary similarly subverted the power relations by providing poetic inquiry

with resources with which to do anti-boundary-work. Sophie's case was slightly different because she herself performed boundary-work, but here, too, an on-going negotiation about how visual methods is done well is clearly a relational process in which Sophie made little shifts on how to do it.

These examples then show that my participants 'have' the power to resist and defy the critique on their work, thereby enabling the use of these methods. Nonetheless, the changes are only small shifts. They are improvisations to the reifications used in their communities of practice. Thus, relatively new areas of research do not necessarily give a researcher the freedom to do whatever they want, as Mulkay claimed (1972).

This, then, raises another question about the notion of innovation in research methods. As I described in the introduction of this thesis, in the UK, alone, there have been numerous events, research projects and journals dedicated to innovation in methods and methodology; here innovation refers to developments in mixed method research and internet-based methods, but also creative methods. The common discourse suggests that knowledge produced with these innovative projects is better suited to understand today's complex world (Friman, 2008, p. 15; Taylor & Coffey, 2008, p. 7). The term 'innovation' implies a break with the past, as I discussed in Chapter Two's *Academic Practices and Innovation* section.

However, as the examples in this chapter have shown and the findings presented in the following continue to illustrate, it is not easy to be innovative. The examples show how the development of creative methods is better explained through Ingold and Hallam's (2007, pp. 2-3) notion of improvisation and the way they juxtapose it to innovation. Improvisation refers to the process, whereas innovation refers to the results. The latter can thus only be established afterwards and even then it is problematic because of the relational nature of the term. This means innovation in one situation is normal practice in another. This does not mean there is no change involved, but rather that it is only specific to the situation. Change is rather a gradual process, limited and enabled by our practices. Finally, the cases of boundary-work are good examples of the inherent tension in academic practice between connecting to earlier research and producing new knowledge (Bryman, 2008, p. 74; Burke, 2000, p. 33; de Wilde, 1992, p. 7).

This means the call for innovation in research is problematic because innovation is instantly limited by the practice of embedding the research in the work of others. This practice of embedding is not only found in the literature (Bryman, 2008, pp. 4-5; Gilbert, 1977; Seale, 1999, p. 189), but it is also supported by the practices of my participants in this chapter and subsequent ones. This means to continue the push for innovation, initially an understanding of

the research process as complex and difficult, is needed. Boundary-work (Gieryn, 1983) is an important part of this. Secondly, this process needs to be supported in ways such as opening up room for improvisation and play in the process of doing research.

Conclusion

I have described and analysed different forms of boundary-work, which have addressed the first sub-question: How do social scientists negotiate the use of creative methods with other members of their research community? Each example showed a different way in which boundary-work can be performed: the extra work Hilde had to do to convince her supervisors; the examinations office questioning Bart and William's thesis; Mary, who performed anti-boundary-work by compiling a bibliography and writing a chapter on peer review for poetic inquiry; and finally, Sophie whose own experiences were worked into the criteria for proper visual methods to do boundary-work from. These examples show there are multiple ways in which boundary-work can be done. Boundary-work does not only concern the research, but also other aspects of academic work. The examples also show it is done on many different fronts, within and between communities of practice, with supervisors, administrators and peers. This is not specific to creative methods, but their appropriation in the social sciences evoke clearer performances of boundary-work, because there are multiple communities of practice involved and thus multiple reifications which are then differently interpreted. The reifications of these different communities of practice involved play an important part in the cases of boundary-work, providing the participants the material to negotiate and improvise with, the basis with which boundary-work can be performed. They enable and limit change. They evoke a need for improvisations because the reifications within the communities of practice are often not suited to some of the ways in which creative methods are conducted, for example as in the case of Bart and William.

There are multiple ways in which the negotiation can occur. The participants negotiated creative methods through complying, standing their ground, using academic practices and in referring to other communities of practice. Power in these negotiations was relational and did not reside with specific people. Mary, for instance, used the resources that potentially could keep poetry out of academic practice to her own advantage to ensure its place in academic practice. Hilde, too, worked on connecting her research to the academic framework of her supervisors. This, then, starts to provide an answer to my third sub-question: How do creative methods emerge? They emerged through the connections they

made with the ‘established’ academic practices and withstanding performances of boundary-work. Simultaneously, my participants’ own efforts of doing boundary-work and anti-boundary-work gave these methods criteria which structured their practices and embedded them within academic practices. Furthermore, the boundary-work and anti-boundary-work slightly changed how these methods were applied in the way mentioned above, but also in concrete ways. For instance, Sophie’s critique of visual research, which worked merely in an exemplifying rather than an analytical way, changed the way in which photography is used in visual methods. Of course, there was more to it than just her critique, but it is one of many voices in the negotiations surrounding visual methods which pushed towards such an interpretive, reflexive ethnographic methodological understanding of photographs rather than an ‘observational’ realist approach one (Pink, 2007, pp. 31-32). As becomes clear, the emerging of creative methods was not a linear process of development – there were many lines of development, going in different, even opposite directions (Ingold, 2007).

Finally, the findings of this chapter enable me to argue that developments in creative methods are not innovations, but improvisations. My participants improvise from the reifications of their different communities of practice, but at the same time always connect to these reifications. If they fail to do so, the same reifications are the basis on which boundary-work will occur. This leads me to conclude that calling for innovative methods is problematic because researchers always have to take the reifications of their communities of practice into consideration. It also means that research claiming to be innovative needs to be regarded with caution. This does not mean change does not occur in research or in the methods used, but rather that they are small. Nonetheless, they are important because over time they amount to the bigger changes to the reifications of how research in the social sciences is ‘supposed to be done’. Then, similar to de Wilde (1992) – who argued that the nature of sociology was its ongoingness and not its objects of investigation or the approach to the research problems – creative methods find their way into the social sciences through the negotiations with and improvisations of the reifications of their communities of practice.

Chapter Five: Creative Methods and Negotiating Personal Practices and Reifications

Introduction

In this chapter, I look at how my participants negotiated creative methods into their own established practices and the potential struggles this entailed. In contrast to the previous chapter, in which I showed how my participants had to negotiate with others, here I provide examples of how they negotiated their own practices, their own reifications of how research was supposed to be done. These examples show how their negotiations were determined by the communities of practice in which they were engaged. For Liza, Richard and Louise, this meant negotiations were set within a disciplinary framework. Paul, on the other hand, had a background in two disciplines that he was bringing together. This meant his negotiations were set in an interdisciplinary framework. This chapter therefore shows once again that improvisations were made from those practices and reifications that the participants were familiar with – and that only incremental change is thus possible. It also shows how creative methods emerged in that process where improvisations were made to well-known practices and the application of the creative methods was dependent on the participant's negotiations.

For some participants, this meant looking at that first point at which they started using a specific creative method. For others, it meant looking at the smaller shifts they made in their (maybe already routine) practices in the process of on-going learning. Roe and Greenhough (2014, p. 51) state: "Research is often a practice strongly shaped by habitual, learned practices and techniques, but one equally perhaps shaped by those moments where the researcher is shaken out of their comfortable interpretive framework." Wenger (1998, p. 53) makes a similar point, writing: "Even routine activities (...) involve the negotiation of meaning, but it becomes much more apparent in situations that involve things we care about or that challenge us." Wenger's observation that these negotiations are more apparent in challenging situations applies to my work. The negotiations in challenging situations were indeed more prominent in the conversations I had with my participants and, as I elaborate below, were indicative of the ways their research practices were challenged by the use of creative methods. At the same time, an apparent absence of negotiations proved similarly interesting. Certain practices and the changes in them remained unmentioned by my participants, but again, as Wenger points out, even in routine activities negotiations were involved. These invisible negotiations index even more established practices – namely, what is considered proper research. Describing and analysing those challenging and everyday negotiations begins to address my second sub-

question: How do social scientists negotiate the use of creative methods into their own research practices? The description and analysis provide a partial answer to the third sub-question as well: How do creative methods emerge in the process?

The aims of this chapter are twofold. Firstly, I seek to show how my participants learnt these methods through appropriating rather than receiving factual knowledge (Lave & Wenger, 1991) through a book or a presentation. In these appropriations, negotiations took place between the participants' established practices, ideas about what knowledge and research is and their adoption of the creative methods. Power played an important part in these processes although it was present differently than described in the previous chapter. Power became visible in the practices the participants themselves had established over the years and in how these practices determined the way they integrated creative methods. As such, they are examples of the way learning in communities of practice works and how change happens within them. Change was, again, a matter of little shifts, as the creative methods were still connected and embedded in traditional practices in terms of methods as well as methodologically. Creative methods were never used in isolation, but always in conjunction with 'traditional' practices. Secondly, these examples of learning and using creative methods illustrate how the popular demand for 'innovative methods' in social science was problematic. Using innovative methods was presented as simple and straightforward. However, rather than simply learning facts about them and then being able to apply them (Lave & Wenger, 1991), their adoption and application take shape within a particular context.

As I elaborated in Chapter Two, learning takes place in communities of practice (Lave & Wenger, 1991; Wenger, 1998). It is a continuous process in which those who learn the practice constantly negotiate reified meanings while also applying those reifications in everyday life. In this chapter, I focus on this learning process for my postgraduate research participants, the reifications they used and how these need to be negotiated. As for the reifications, I make a distinction between smooth and rough negotiations. Smooth negotiations are those that are almost invisible, where improvisation and appropriation are necessary but seem to cause no problem. Rough negotiations, on the other hand, demanded more work on the part of the postgraduate research participant. Their academic background and affiliations prevented their simply integrating the creative methods into their own established research practices. Even though literature and some training were available, appropriation and improvisation of this knowledge into the practical context of their own research proved crucial, as each situation was new (Ingold & Hallam, 2007, p. 2; O'Reilly, 2005, p. 4; Pink, 2007, pp. 4-6).

My focus here is specifically on the early-career researchers because I followed their research over a longer period of time, enabling me to see first-hand how they engaged with the methods. I would ask them how their research was going, and often the structure of the conversations would resemble those I would have with my own supervisors. There would be a quick summary of what they had been doing; things that went flawlessly were mentioned without much attention, which was a reflection of their established practices and smooth negotiations. The bulk of the conversation, though, would be a discussion of the plans they had for the next steps in their research, often in connection to issues they were having at the time. These could be setbacks due to things not going as planned, or it could be that things were going extremely well despite earlier doubts. The conversations were therefore often about discrepancies (visible and rough negotiations). The postgraduate research participants and I often found ourselves thinking through those next steps together: how to do those things, which approach to take, the potential pitfalls and how to deal with them – all of which are part of the academic practice of thinking through an issue (Bryman, 2008, pp. 66-76). This focus was to be expected because a question such as ‘How are you doing?’ evokes stories of experiences that stood out rather than stories about mundane routines. It thus evokes stories of improvisations in unexpected situations. Moments that jumped out for my participants were shaped by their backgrounds, skills and different communities of practice (Wenger, 1998). At the same time, these contexts determined what was deemed normal by my participants: what they did not need to say or what could be said without explanation, especially in conversation with another researcher who knew about creative methods, methodology and epistemology – me. I now turn to discussion and analysis of the negotiations of four of my participants.

Liza: Negotiating Data-gathering in Participatory Visual Methods

Liza found herself negotiating between a participatory and a more guided approach towards her participants in their use of photo diaries. But her theoretical and epistemological standpoints and expectations had not offered an appropriate way of negotiation between the two. In Chapter Three’s *The PhD Students* section, I introduced Liza and her research in which she worked with vulnerable young people in the developing world and studied their everyday experiences. Liza had no theoretical or practical experience with creative methods and she first encountered them while looking into other research done on her topic.

Basically, with my methods I think that was completely up to me, what I’m going to

do... And [my supervisor] just said: “Well, you can just try anything, and you will see what works out,” because she didn’t have experience in [country where Liza did fieldwork]. And then my second supervisor, she worked a lot in [country where Liza did fieldwork], basically, and she told me, like, about methods a former PhD student of hers had been using in [country where Liza did fieldwork], and she was also working with youth, so that is how I got to know about all those, you know... photo diaries, and she also used written diaries and audio diaries and stuff like that. I just thought [to myself]: “Yeah, maybe you can use that as well.” (Liza, 27 November 2012)

This quote is compelling because it, like others I collected from her, shows the freedom Liza was granted by her supervisors, while, at the same time, there were subtle signs of direction, guiding her to the way things were done. Liza said it was up to her to decide which methods she was going to use, but her second supervisor subtly provided suggestions for possible suitable ways of doing her own research by pointing out the work of a former PhD student. It is important to note that her supervisor did not tell her to use these methods; Liza was free here to choose her own. Nonetheless, her supervisor’s input was not neutral. Liza could assume these methods proved fruitful for the other student, and that her supervisor approved of them. She could also assume they could be suitable for her. This provided Liza with a framework to embed her work within the literature (Bryman, 2008, pp. 4-5; Gilbert, 1977; Seale, 1999, p. 189).

There were other indications that creative methods were approved within Liza’s field of research. For instance, when she read about research done in the global South, she came across performance methods or, as she called it, drama.

So, the same (...) [held true for] drama. I have been reading about it: basically, what other researchers in the global South have been done , and I made my notes [on] how they did it, basically. And how they approached people, you know some Ubuntu theatre workshops and things like that, working with people together. I just read about it, to be honest. (Liza, 27 November 2011)

Again, Liza indicated that the literature on research methods used in the global South presented creative methods as a suitable way of doing research in this context. The methods might have been new to Liza, but in her field, performance had been used before. It was not a

random choice she made; it was in the literature and, as such, an acceptable way of doing research. It became clear that ‘completely up to her’ was actually already embedded in certain options. The literature guided her and, through the literature, she connected to earlier work. It had been done before. Nonetheless, it was a shift for Liza, personally. Her research experience involved more familiar methods, such as in-depth interviewing, but not creative methods. She learnt the creative methods by applying them in her PhD research. What is interesting is that, again and again, connections were made with existing frameworks. As became apparent, Liza embedded the methods in familiar frameworks using the literature and her own methodological practices as structures in which to embed them.

When I asked Liza how she prepared for the methods, she told me the following:

Okay, basically for the interviews, I was just looking at the concepts I'm using and was framing the interview guide, and discussed it with my supervisors until we were all more or less happy with it. Yeah, that is what I also did for the focus group discussions and the expert interviews. Then, with the photo diary, I also included my supervisors and asked them, you know, if there [is any good guidance to give the participants what kind of photographs they should take], (...) or if I should leave it to them [her participants], and they [her supervisors] basically advised me to see how it works out [saying:] “You know, you have to see, and we can't say if that's right or wrong. You have to find out yourself.” (Liza, 27 November 2011)

With the instruction [for the photo diaries], (...) I wanted to do it according to my research question, (...) I'm investigating, like, places of inclusion and exclusion, socially and economically. So I thought: “It's nice to have that in the pictures too, and have a story behind that person, basically.” And that is how I build up the instructions, saying basically: “What are the places you like or you feel comfortable in and what are the places you don't feel comfortable in?” Things like that. (Liza, 27 November 2011)

A number of things are particularly interesting in these quotes. Notice, for instance, how little Liza actually explained about preparation. Clearly, for her, preparation for the methods is a familiar practice that needed no explaining to a fellow researcher, and she assumed I was knowledgeable about them. At times, I tried to get her to elaborate on these practices, but to no avail. For Liza, it was enough to clarify what she was doing by telling me that her

theoretical concepts framed the interview guide. That also seemed sufficient to explain what she did for the focus groups, expert interviews *and* the photo diaries. Apparently, she felt this needed no explanation or negotiation. Moreover, for the photo diaries, she used her experience with the other methods in her application of the ‘new’ method.

This brings up a second point: namely, how Liza embedded her use of a ‘new’ method within practices familiar to her. The visual method was going to be used in the same way as the interviews. There is little difference between the way Liza engaged with the visual method and the interview-based methods. This shows that the negotiations she had to do with the reified practices of preparing an interview – so that it could work for the visual method – were smooth. However, and this is a third point, when she asked her supervisors for advice on how to engage the participants, the three of them discussed the interview guide at length, but not the instructions for the photo diaries. This meant her supervisors played a much more negotiating role in the design of Liza’s interview guide but not the photo diaries. For whatever reason, they did not discuss possible ways of doing the photo diaries with Liza. They told her she would need to “see how it works out”, which gave Liza the freedom to work from her own practice. It is all the more interesting, then, that she remained with tried and tested ways from the literature, as I will now elaborate on.

Up until a certain point, the negotiations had been smooth, but, as it turned out, asking the participants to produce photo diaries was not as simple. It proved to be quite difficult, and Liza discovered discrepancies between what other researchers had done (or at least had reported doing), what she prepared and how things played out in the field. Before Liza asked her participants to produce photographs, she spent a lot of time with them, gaining their trust. When she finally asked them, they knew her fairly well. Nonetheless, during the interaction with her participants, Liza was unsure how much to guide them, and she struggled with epistemological ideals and methods in practice. This turned out to be a rough negotiation. On the one hand, Liza wanted her participants to be free in the kind of photographs they produced, so the research would be participatory, and power relations would be less one-sided (Holm, 2008, pp. 329-330). She wanted her instructions to interfere as little as possible. Possibly she thought it would also be a way to get unbiased data, but Liza did not mention this as such. On the other hand, she noticed how her participants were unsure about what it was Liza asked them to do. Her participants were seeking her approval, and they wanted to make sure they were doing it right.

And I felt I was very, very important, to give them the instructions too. Like only to

tell them. Like they were really, some of them were insecure [about] what to take pictures of and they didn't want to do anything wrong. Even though I wanted them to do it very free, and they should take pictures of whatever they like or don't like, basically. That was too abstract for them. So I thought: "It's really good to give them the sheets [with instructions]." And I even had some people coming back to me, who ticked what they had done already, [saying:] "Look, [Liza], I've done this one and that one already." You know, then they enjoyed it, and they felt like they accomplished a task, basically. So that was quite good. (Liza, 27 November 2011)

Liza: I don't know, sometimes I still feel a little bit unsure about the instruction thing. Because if you talk about participatory methods and having... yeah, letting your participants having control over the research methods, is that the right [way of saying that]? Do you know what I mean?

Pauline: Yeah, yes.

Liza: Yeah, it's strange to give them a sheet of instructions, isn't it? So I thought: yeah... but yeah I still don't know how... to do it, basically. If I just say: "Here is the camera and take pictures of whatever you like..." you know... It's somehow strange. (Liza, 15 January 2012)

I could see the confusion in their faces, basically, when I was saying: "Well, here's a camera. [It was as though they were thinking:] "What does she want?" (Liza, 15 January 2012)

These quotes show Liza struggled with how to handle her participants' need for guidance on the one hand and her own desire to make the research participatory on the other. These are thus examples of rough negotiations. The things Liza had read about these methods and the way they turned out in practice did not align. She echoed the participatory action research ideal of letting her participants speak for themselves rather than having the researcher prescribe their responses. This is not only an ideal within qualitative research, but all the more so in creative methods. As I described in Chapter One, the literature often cites using photovoice or photo-elicitation as a method because it enables people who are less eloquent or unable to express their views in interviews to speak freely and openly (e.g. Darbyshire,

MacDougall & Schiller, 2005; Einarsdottir, 2005, p. 527; López, Eng, Randall-David & Robinson, 2005; McIntyre, 2003, p. 48; Riley & Manias, 2004, p. 401; for an overview, see van Romondt Vis, 2007). Such an approach towards participants is questionable, but nonetheless informed Liza's practice. It is also a way to give the participants more power in the research relationship (Holm, 2008, pp. 329-330; Wang & Burris, 1997; Williams & Lykes, 2003). However, that was not always realistic, as Liza indicated, and despite her efforts and hopes for collaboration, she gradually took on a more guiding role. This does not mean Liza necessarily had more power in the research relationship – quite the contrary; her participants in a way asked her to take on this role. Liza did not want to do this for the aforementioned reasons, but, at the same time, her participants were unable to produce photographs, as became apparent when she said: "I even had people coming back to me, who ticked what they had done already." Liza evidently needed to improvise here, but she felt at a loss for some time, as evidenced in the way the discussion continued across interviews in November 2011 and January 2012. She needed to make a bigger shift in her adaptation of the method in order to get the photographs she wanted, to make the method work. The improvisation here was harder because Liza was unfamiliar with the practices.

So they're wondering: "Why is she actually talking to us about that"? And I understand, I see that. Even though you, I really take time to explain it to them. Sometimes I'm sitting there having monologues. [I'm thinking:] "Why I'm doing this?" for half an hour or longer. And I... you know, sometimes you really have the feeling they still don't know why... and how... Because it's not done that much or I don't know what, yeah... You can name so many reasons why they can be confused. And it makes me feel bad, to be honest, because I don't want them to just something to do me a favour. Or... but I guess that is a struggle I have to... just acknowledge and think about and just try to take as much time to explain it to them. And [what] I see with those people I have been spending a lot of time with is that they get it. They say: "Well, that's for your research." But I can't do that with all the 40 participants. I don't have time for that. (Liza, 15 January 2012)

Again, Liza experienced the discrepancy between the ideal of participatory methods (to collaborate with participants, to give them control over the research process) and how the application in the field panned out, where her participants did not understand why she did the research. Here, she explained how she tried to resolve the issue by explaining the photo

diaries in more depth. This led to another problem, which was insufficient time. Although there was no definite resolution (she lacked the time to inform everyone properly), Liza negotiated her way through the issue, and eight participants still produced and discussed their photographs with her.

Erm... I feel... less confident, erm, with that particular method [visual methods] compared to interviewing, for example, because I'm not using... It's the first time I'm using it basically. For my Master's, I have not done it. The only thing I have done is reading before I was talking to another former PhD student who has been using it here in [country where Liza was doing fieldwork] too, and I was pretty much following her advice in a way. She also said she, though, [that] it's very good if you leave it open to them to take, erm, what to take pictures of, basically, and she said in her case [that] the most interesting stories were from the pictures and things like that... I mean, I can't say the most interesting things came up. It's difficult to judge that, I think. But very interesting things came up. (Liza, 15 January 2012)

Liza's first use of photo diaries entailed examples of both smooth and rough negotiations. The negotiations were smoother when guidelines from the literature could be followed, and it was thus easier to embed them into the familiar practices of other methods. Improvisations and negotiations in those cases were nearly invisible, for instance, in the preparation of the instructions. When the discrepancies between her own familiar practices, the literature and the actual experiences in the field were too great and the need for improvisation became evident, negotiations were rougher. The lack of familiarity with those practices made improvisation harder. This exemplifies quite nicely Ingold and Hallam's (2007, p. 2) conceptualisation of improvisation:

no system of codes, rules and norms can anticipate every possible circumstance. At best it can provide general guidelines or rules of thumb whose very power lies in their vagueness or non-specificity. The gap between these non-specific guidelines and the specific conditions of a world that is never the same from one moment to the next not only opens up a space for improvisation, but also demands it, if people are to respond to these conditions with judgement and precision.

Improvisation is inevitable because the codes, rules and norms are insufficient in dealing with

the specific conditions of everyday life. At the same time, improvisation needs those codes, rules and norms to improvise from. Thus, the connection with earlier practices is always made. Because both her supervisors and Liza herself were less familiar with those practices surrounding photo diaries, she drew on her knowledge of other methods and what she had read about photo diaries. Still, the discrepancy was great enough for the improvisations to become visible.

Finally, power manifested itself in various ways. On the one hand, Liza took more control of the research process by guiding her participants more through it. On the other hand, her participants implicitly asked her to do so. Power is neither present in nor absent from a method. Methods in themselves are not participatory, but their application is (van Romondt Vis, 2010).

Richard: Negotiating Reifications of Data from Participatory Visual Methods

Richard experienced a discrepancy between the data he had expected from using visual methods and the data he actually received. His expectations were based on the epistemology and methods literature of visual methods. Richard had high hopes for meaningful photographs, but was disappointed by those he received. In this section we see how he resolved this discrepancy.

Similar to Liza, Richard was uncertain about the instructions for his participants and likewise felt he did not have full control of the method. Unlike Liza, though, his uncertainty did not concern a discrepancy between ethical principles and the practical conduct of research. Rather, it was a discrepancy between, on one hand, his different epistemological ideas about knowledge and its production and, on the other, the ideas' practical application. Visual methods, like any other, can be applied in very different ways; their application and the knowledge produced depends on the epistemological standpoints of those who use them. Richard was finding his way through those different standpoints. His search was manifested in his application of other methods such as the group interviews and participant observation though was more prominent in the visual methods he was less familiar with. His struggle between an 'observational' realist approach and a more reflexive ethnographic one stands out here (Pink, 2007, pp. 31-32). In order to see Richard's rough negotiation between them unfold, I explain a bit more about these two approaches. As Pink explains "this 'observational' approach depends on the problematic assumption that reality is visible, observable and recordable in video or photography" (p. 31). Pink contends there are two

problems with this approach. The first is the assumption that it is possible to observe and record 'reality' (p. 31). The second is that "we can observe and extract objective information (data) about our informants" (p. 32). She goes on to explain that the problem is that the approach only takes into account the visible and the material, whereas visual culture goes beyond that to include human imagination and conversation. Furthermore "material objects are unavoidably visual, but visual images are not, by definition, material" (ibid.). The reflexive ethnographic approach takes an opposite view. In addition, it considers the context in which the photographs are produced and that the meanings the photographs carry depend on the interpretation of the individual. The analysis of the photographs is dependent on what the researcher wants to do with the photographs. What becomes clear is that Richard needed to negotiate at different levels: his standpoint towards these epistemological approaches and how it got translated to conducting his research.

As mentioned when introduced in Chapter Three, Richard researched how adolescents of different backgrounds engaged with health in their everyday lives. I also said Richard was very interested in methods and different epistemological approaches. He was a member of several reading groups at his university that focused on epistemology and methods. One of those was particularly concerned with creative methods. Through his participation in these groups and his experience with creative non-fiction in his Master's, he became interested in trying out new ways of doing research. In addition, he used those methods as a way to circumvent certain restrictions related to working with adolescents. Being an unfamiliar adult, he was not allowed to be alone with them, according to school regulations. Moreover, he did not feel he could do participant observation by just hanging around the public places they frequented. To get insight into those locations, he gave them video and photo cameras. Still, because Richard had never used visual methods, his expectations were based on the visual methods literature.

In the conversations I had with Richard, we would talk a lot about methodology and epistemology. He often told me about books and articles by philosophers he had read or the clips he had seen online. He enjoyed discussions about epistemology, and I got the impression he enjoyed discussing it with me. Our discussions would also be a way for him to think through (negotiate) the different epistemological standpoints and the way they related to research in practice (Bryman, 2008, pp. 66-76). He would sometimes ask for my opinion, which was a methodological issue I myself had to deal with, as I was part of the very negotiations I was researching (for an elaborate discussion on this point, see Chapter Three's *Emerging Practices Negotiating Knowledge, Ethics and Power* section). Richard would also

often talk about a methodological issue he was grappling with and halfway through the conversation would say something along the lines of “actually, thinking about it now...” and then resolve the issue on the spot. His interactions with me were ways for him to express his negotiations, a way to think through the methodological choices he had and their consequences (Bryman, 2008, pp. 66-76). The following quote provides an example, with Richard describing the different disciplines in which his PhD project is based.

So because of that [being in between two disciplines], I'm constantly a little bit in a tension between... 'cause a lot of the sociologists – if I can lump them into a field like that – would be very critical of the methods used by the hard scientists in physical activity and health. And, similarly, the physical activity and health guys would feel that the visual diaries and fictional, the narratives that I'm writing as being kind of pointless and not being real research. So I do wonder sometimes, and then I think: what can I do [inaudible]? 'Cause I'm trying to balance between those two things. I'm wondering whether my thinking is inconsistent. Or my thinking needs to be more open-minded to both ways, and they're both useful ways of trying to find something out. It just depends on what you're trying to find out. And one method might be more suitable than another to that particular question. Or they [are] just totally inconsistent and incompatible and incommensurable, I don't know. So I wonder whether in the middle of my research process I can chuck in a little hypothetical test, and I have some certain control factors and then one variable and then have a pre- and post- sort of situation going on, as a kind of trial, and have a control group. And see whether I can implement a significant change in my trial group that I don't see in my control group. Those sorts of things, which one discipline would baulk at and another discipline would see it as being pretty useful. So yeah, (...) would think of that as being a bit of a tension at the moment. And partly I'm trying to rationalise it and when I rationalise it, I think it's perfectly acceptable. It's just... you can't ignore something because lots of people have been using it for a long time and it's useful. I can rationally understand why that is a legitimate way of getting [inaudible] about something. And on the other hand, I've got the personalities. I picture my supervisor and I picture the well-established researchers that I read and that write books that I'm treating as bibles and they would hate that sort of research. So there's a rational side and there's a kind of... I don't know, it's just [a] personal side of it, which is far more socially constructed, kinda still shaping as much of why, why and how I should

be doing things. So yeah... I might chuck in a little test somewhere. (Richard, 9 December 2011)

This quote indicates the various epistemological strands Richard was negotiating. His participation in different communities of practice and his broad interest in different epistemological ideas required negotiations because their standpoints did not align. This invariably is a rough negotiation, which also becomes obvious in the way Richard struggled with the kind of data he and his participants were producing. The data he received did not meet his expectations, and he was trying to figure out how to deal with this. The following quote demonstrates how Richard would think his way through this issue.

Pauline: So you've got the pictures already?

Richard: Yeah, some of them are good. Some of them [his participants] didn't quite get it. I'm not really sure what to do. Some of them have really gotten into it and [inaudible] some useful stuff, but a lot of it was just playing about with [the cameras]. So I'm not sure to what extent they should be, how I want them to treat it. I don't want them to treat it as a serious project, 'cause I'm not sure that they're that into it, that they're gonna go for it. They're sort of playing about with it. It's what they're doing at break time, is they'll take a video, they'll talk to their friends. They're: "Hey, hey!" They all say "hello" to the camera, "This is what we do, we sit on this bench." They tell me a little bit about that and then they just take photos of fairly random things that they have yet to explain to me. So I have to talk about it next time. So I don't want it to be like serious homework for them to do. I want them to enjoy it. So I don't mind them sort of having a bit of fun with it and playing about with it. But I'm a little bit concerned that they just sort of... If you do that, you don't really engage with anything that's perhaps more serious behind it – which are the things that I'm probably more interested in. (Richard, 14 February 2012)

Here Richard plainly expressed disappointment with the data he got. His participants “didn’t quite get it” and they were “just playing about”. Richard was more interested in the “more serious” things “behind” what his participants could produce if only they would “really engage”. The photographs did not conform to the idea he already had (about which he elaborates later). He wanted the photographs to be representative of their experiences, their

rationales. He wanted to get at an unconscious level of why his participants did what they did. But he was having a hard time doing that, as it was difficult for his participants to say more than ‘because I like it’. Richard believed age prevented their giving well-thought responses. Rather inconsistently, he did not question his own presumptions about what photographs can portray and how they work in research. Instead, he doubted his instructions for the participants and questioned their engagement. At this point, Richard still disapproved of the photographs because they did not fit his assumptions of what proper data was. Pink (2007, pp. 42-43) recounts a similar disappointment Hastrup reported feeling with visual methods and how it resulted in her conclusion that the methods did not work to produce ethnographic evidence, rather than questioning her application of and expectations for the method. Beaulieu (2010), on the other hand, experienced something similar, though drew a different conclusion. In her study on the knowledge production of two research labs, she asked her gatekeeper to take her to where the participants did their research, only to be told to look at their website, which offended her.

Youthful arrogance probably had a part to play in this episode. I hadn’t come all the way to North America to look at websites – which I’d already done from home anyway. But arguably, what also underlies the humiliation and frustration I felt when being told to look at the website are ideas about the proper way to do fieldwork in STS that I had internalized. This story illustrates how assumptions about what counts as proper ethnographic observation shape one’s field. Because I had set out to do participant-observation in a particular place, I tended to reject this invitation as not being very relevant to what was worth observing, and therefore out of line with my goals to conduct an ethnographic lab study. (p. 454)

Likewise, Richard is dismissive of the photographs because they do not meet his expectation of providing him insight into his participants’ engagement with health. He hoped the photographs or video would actually show him this engagement. Seen from this observational approach, the photographs or videos could indeed contain little information if his participants did not produce visual materials with the information he sought. In addition, some of his expectations were based on literature that adheres more to the reflexive ethnographic approach, whereby meaning is not simply equated with the content of the photograph. In the following quote, which is from a conversation two months later, he addressed again the issue of his participants not “getting it”.

Pauline: I was just wondering how that [photo diaries project] went. Was it easy? Or did you find...

Richard: No, it wasn't that easy because they weren't sure what to take photos of, because sometimes... Basically, I tried to explain what I wanted them to take photos for. And they weren't sure about that sometimes. So they're like... I described it as "try and tell me something through a photo," which is not as easy to talk about in words. I used the phrase "a picture tells a thousand words", and said to try and get into that mind-set and tell me something that makes me understand the situation a bit better than talking would do. And in quite a few of the situations, they didn't need to take a photo. They'd taken a photo for the sake of it. You know, they could have told me: "I do homework [and it takes a lot up of my day] rather than taking a photo of their desk. It was like, it's a desk. Do you know what I mean? It doesn't, it doesn't bring out anything more to the conversation. It doesn't bring anything more to my understanding or their understanding. They're just taking a photo, because a researcher told them to take a photo of something..." (Richard, 4 April 2012)

As discussed in Chapter One, there is quite a bit of excitement surrounding creative methods and how they supposedly unlock new areas of knowledge, but they may not always be appropriate (Pink, 2007, p. 43) or researchers do not realise different epistemological and methodological approaches are needed. Buckingham (2009, p. 653) argues that "the use of such methods – as of any method – needs to display a degree of reflexivity: we need to understand how research itself establishes positions from which it become possible for participants to 'speak'". Richard was clearly still struggling to align different epistemological assumptions, thoughts and ideas about these methods with others in the literature and the way they panned out in his specific research. But his last sentence in the quote above indicates he also started to think along the lines of what Buckingham (ibid.) argued: how the research established these positions from which participants were able to speak.

Richard was trying to convey to his participants what kind of photographs he wanted them to produce. Ideally, they would go beyond words, as he said the visual methods literature claimed, and they would tell him something other than what the interviews revealed. In the literature, the method was promoted as especially useful with young people, providing a good way in, serving as a prompt and a fun way to engage youth (Buckingham, 2009; van

Romondt Vis, 2007). However, Richard did not feel it enabled his participants at all, but rather showed what kind of relationship and power relations he had with them. He felt there was a discrepancy between what was said in the literature and how he experienced the method in practice.

In the following quote Richard explicitly said he initially used the method to connect with his participants because they were unable to express themselves very eloquently. He went on to argue that the method might not necessarily be the solution to that particular problem. He realised the problem was not solely in the instructions he gave, but also the difficulty of visualising the research topic.

Pauline: And you said something about the visual, how you thought it had its limitations, but also...

Richard: Oh sorry, yeah... Yeah... I think it has its limitations as much as talking to someone, as there is only a certain amount that language can convey – particularly if you have a limited vocabulary, like 14-year-olds do, and even more limited vocabulary when they're talking with their mates in an interview setting. I can see that on a linguistic level it's difficult for me to properly understand their concepts through an interview. So what I've done is I've gone to like a visual method. I've gone: "Right, okay, I need to understand their concepts through the language of photos and the language of sight," that sense to try and understand their concepts a little bit better and they are equally flawed perhaps. Some of the reasons why they do things, you can't really represent them visually. Of course there's other reasons why I'm getting them to take photos, like it'll be a photo-elicitation kind of thing, where I'll get them to talk about it and it's a bit more of a prompt. But then also I want their photos to be representative of their concepts, like any kind of formal communication or any word it might be. But obviously, you get to the stage where it's like well, if you want to find out more about this phenomenon, then a photo isn't the best way to do it. What [do] you want to take a photo of? And they [photographs his participants took] were pretty good, because I left it [the instructions he had given his participants] open-ended. (Richard, 14 February 2012)

Here Richard started realising that the visual is not necessarily the solution to get better, more or, for that matter, any kind of data from his participants. Moreover, he started realising that it

might not be possible to visualise his research topic as he had hoped. In the next quote, he emphasised again that the photographs did not bring any new ways of looking at the research topic. The only thing they brought were more themes. In addition, Richard expected the photographs to convey more meaning by being good. As with the interviews in which he wanted his participants to say more than 'because I liked it', he wanted the photographs to show more than just an object.

Yeah... on some occasions [using the cameras] was good, because it did reveal... It revealed something that wasn't happening in the conversation. We talked about a subject on a couple of occasions, a couple of times when I'd go in to interview and it hadn't come out. Yet when they're taking... I give them a camera and they're in a home situation, something else comes to mind, like the stairs or the home gym. They hadn't talked about that before. But they had talked about it through a photo. So I think that might highlight simply [that] if I had done an interview with them at home that those things might have come out. So I think... Probably, just thinking about it now, it's probably more useful, simply because it took the data collection into a different social space. It took it into their home social space rather than a school social space. So it was the sort of vehicle to get some of my research into their homes. So I was the camera. There was a bit of me in that camera and they were going away and they were showing me because I was the little person in that camera. So they were taking, taking, taking, [thinking:] "Okay, I'll show this to Richard, I'll show this to Richard, I'll show this to Richard." And so I was sort of... [thinking that] I could get in on their home life that way but, you know, if... I don't. Sometimes it wasn't like the photographers or the visual ethnographers really describe it as... when they analyse the photo sometimes, as you know [they say:] "Look at the balance there, look at the focus of that photograph and..." None of that was there at all. It was just... fairly... like rudimentary photos of artefacts which remind them of [health]. I didn't get a whole bunch of meaning out of them, apart from that really. (Richard, 4 April 2012)

When I probed Richard about what would have made good photographs and thus good data, he said the following:

It's hard, and I can't just say that without explaining it, but I do believe some photos

were crude and not very meaningful and some photos were more sophisticated and more meaningful. I'm not really sure. I suppose I'm judging that based on... whether that photo told me something new... (Richard, 4 April 2012)

Even though Richard maintained an observational approach, the way he had conducted his research did allow more thought to be given to how he enabled his participants to speak (Buckingham, 2009, p. 635). Slowly, Richard adjusted his thoughts on and approach of the method to one integrating both what he had read about it and how he experienced it himself.

[Using a camera] just lends itself to some things and not others, I think. And it depends massively on your participants is what I've learned, and the instructions you give them and the social situation [you] allow them. (Richard, 4 April 2012)

At first, Richard was disappointed by the data he received and sought a solution through how he went about it. Later, though, he realised that his expectations of the kind of data in these particular situations were possibly problematic. At the same time, he realised that his expectations of what his participants could convey through the photographs were unrealistic. These expectations were fed by the descriptions of ethnographers and photographers and his own experience of photo journalism. He had hoped his participants' photographs would visually engage the viewer on an emotional level, but they did not. This expectation was reminiscent of one of our final conversations, in which Richard expressed how he had expected to discover something spectacular and to actually change the way his discipline thinks about his research topic.³⁸ He would find out something substantial and new and this method would help him do that, or so he hoped. He came to realise, however, that the method had limitations similar to those of others because it was addressing the participants about the same issue in the same way. Still, this section shows that Richard had come to realise why the method was useful for him, which did not necessarily have to do with visual knowledge, but rather with a wider approach to understanding the topic.

Thus, Richard gradually adjusted his expectations of the method. He started to understand that for his participants to produce photographs in the way he had first expected would be unrealistic. They would have to be trained photographers, which quite simply they were not. Nonetheless, using photography encouraged the participants to think beyond the

³⁸This was an expectation Louise also expressed.

setting of the school, even though the meanings of the photographs could have easily been conveyed to Richard in an interview. Moreover, the photographs made things more real for him. They brought out details that his participants had not discussed during the focus groups and let him better understand the surroundings in which his participants lived. Finally, the method enabled him to learn about situations he could not observe first-hand. Because he was working with adolescents, ethical guidelines prevented his being present at his participants' homes and after school hang-outs as well as being alone with them.

Louise: Negotiating Methods of Analysis

This section deals with the difficulty Louise had in finding an appropriate method of analysis for her research. Louise felt there was a discrepancy between the specific way she had applied visual methods and the methods of analysis available. In addition, she felt it was important to do her research properly, which meant, for one thing, that it had to be well connected to the literature. A problem presented itself, therefore, when she was unable to find such literature. She resolved this issue by acknowledging that this is part of the process of doing research and by using a mix of methods and improvising from them. As such, Louise exemplifies the tension between doing something new and connecting to prior research. This tension structures the development of creative methods in a particular way.

In her search for an appropriate method, Louise had to negotiate between her own academic standards, the specific way she had applied the methods and all the ways of analysis 'out there'. As I show through quotes from my conversations with her, she was keen on embedding these methods within an academic framework. She talked a lot about how research 'should be done', and it is evident that these norms framed her search for what she saw as a proper analysis. At first, she was having a hard time finding a fitting method of analysis for the specific data she had, a method that would also live up to her academic norms. Slowly but steadily, though, she came to terms with the fact that the perfect method was not 'out there' and she managed to deal with the improvisation.

As said in Chapter Three, Louise worked with adolescents in schools, researching gender and embodiment. She started out using participant observation to build some rapport with her participants and then added photography and focus groups as additional methods. Some of the photographs were produced by her participants, and Louise herself produced photographs as well. Her supervisor had been using photography as a method for a longer time and encouraged Louise's use of the method. Louise, however, was not always convinced

by the way her supervisor used the photographs, especially in regards to the use of the data and the analysis. Louise was therefore trying to find her own way into the method. The use of photography with her participants was a smooth negotiation for Louise, and she did not say much about the process. Partly this was because she had already used this method in an earlier project. In that project, she told me, she had noticed how building rapport with the adolescents worked well as a way to get the participants engaged. The gathering of the data was thus a smooth negotiation for Louise. Analysis of the photographs, by contrast, brought rough negotiations for her.

In many of our conversations, Louise would address academic norms and what she considered good and bad research, which resembled the norms I discussed in Chapter Two's *Research Reifications* section (Bryman, 2008; Guba & Lincoln, 1989, pp. 233-250; Seale, 1999, pp. 140-148). For instance, on numerous occasions she talked about how research and analyses had to be systematic (Louise, interview, 13 June 2011). Another example came when she talked about how the proper way of doing an ethnography was to spend a prolonged time with the participants and to go back to them numerous times. Or, she said that the right way to do participatory research, as she saw it, was to truly let the participants decide on the course of the research and let them conduct it (Louise, interview, 8 July 2011). Louise did not dismiss other ways of doing research, but doubted whether you could call them 'ethnographic' or 'participatory', and she felt others were less rigorous in using those labels. She also voiced concern (Louise, interview, 13 June 2011; 21 March 2012) that her work needed to be comparable enough to other work in her field so she would fit in but at the same time needed to say something new so she could add to that body of knowledge (which is a variation on the argument of this chapter and thesis!) (Bryman, 2008, p. 74; Burke, 2000, p. 33; de Wilde, 1992, p. 7; Loughborough University, 2014a). Relevantly, in the comments on an earlier version of this chapter, one of my supervisors asked the same question of me: "What are you bringing that is new?" As Louise put it:

But I don't just want to base it on what previous people have said because you're supposed to come up with something new. I want to say something new and different (...) And I'm reading people's work again and there is so... There's quite a lot of stuff that does similar things to what I'm doing. And I knew, I kind of knew that, but I want to say something new... Just this sort of... weight on my shoulders of trying to say something new while also making sure I'd deal with my data in a, in a good enough way, so that I'm similar enough to be... to these studies. (Louise, 13 June 2011)

...that being a good thing that I've worked within this, I think this, um... sort of existing work, but... Yeah, I just feel like you have to... Not, not something completely new, but... the point of getting a PhD is that you've added to knowledge. And so I was worried that I was like just regurgitating exactly the same things. (Louise, 13 June 2011)

It's what I expected, but... I don't like that. I don't like saying: "Oh this matches that expectation", 'cause, well, what good is doing research if you know what you're going to find? (Louise, 8 July 2011)

These views on how research should be done clearly influenced Louise's approach to the analysis of her material. During the time that I was talking to her, she frequently said she struggled to find appropriate literature to help her analysis. She got overwhelmed by the different forms of analysis, but no one way spoke to her. Because her approach was post-structuralist, she argued that discourse analysis was an appropriate way of analysing the data. She was therefore interested in finding out how to do this, especially in relation to images, but in one of our conversations she discussed at length why the literature she came across was not quite what she was looking for.

And I didn't find that [discourse analysis as Potter and Wetherell discussed it] particularly... useful to the way I wanted to deal with my data. So I'm kind of still a bit stuck and the way I've just, I mean, I've analysed all my data now. And it's been interview-based, it's like, the foundation of it has been the interviews and the coding I did for that. And the photos [have] sort of been to illustrate what's been in the interviews, or where something different has come up. It has not been systematic, really, and, I think, I am a bit worried about that.

Pauline: Hmm.

Louise: Um, but I don't really know how else to do it because I'm analysing the types of photos that I've got... When you look for analysis of photographs. And there's quite a bit of visual methods books, but they... they talk about using photographs for photo-elicitation. So therefore they're just an instrument for the interview. Or you've

got ethnographic photos that are collected by the researcher and... you're doing like a content analysis, and very systematic. For instance, someone will look at covering images on a... on a magazine for the last 30 years and look at the progression and changes... And they measure things like... the whiteness of the skin of the models... the colour of their hair, that sort of thing. I mean, I've seen examples of that sort of stuff, but I've got students' photographs, and if I want to use them ethnographically like as a photo diary... It's quite difficult to find a way of analysing them.

Pauline: Yeah.

Louise: 'Cause nothing's really... Like set out as to what you can do with it. And especially, because I also got researcher photographs taken around the school... That way I can analyse all of my photo data as a whole... Um... As well as my interview data plus my observations in a, in a way. So I guess that's a really long way of saying... I don't have a single analysis method for all the different types of data. (Louise, 13 June 2011)

This quote plainly illustrated the issue Louise had in finding a way to properly analyse her data. The main problem was that her photographs, in her opinion, fit none of the methods of analyses because of the way they were produced. This made it difficult for her to use a single framework with one method that would make the analysis consistent and systematic, which was very important to her. She was worried she had not been able to do that. Moreover, by using existing methods of analysis she could embed her analysis in a legitimate academic framework. The issue of analysis did not come with an easy solution. In the following quote she continued her search for a fit between her data and a way to analyse them.

Rose [in reference to the book Visual Methodologies] has got two different chapters on discourse analysis. Um, I've not read it for a while, so... So I think I didn't find it particularly. It was like something that I could potentially use, but didn't find, like, I was like: "YES, I can take this sort of method!" Or because [Rose] was talking about... collected images again. She wasn't talking about participant images. [silence] And she had, like, one chapter was on Foucauldian analysis. I can't think what the other one was. Um... And I not really sure... I can't, like... I couldn't really explain how you would do a discourse analysis on photos based on what Rose said.

(...) I didn't dismiss it, but I didn't find it immediately useful for me. Um... yeah. (...) Yeah, I was hoping for something that could help me... to actually do a systematic analysis and it wasn't that sort... well, it was a systematic analysis of different sort of types of photographs to what I've got. So I couldn't really use it. Um... (Louise, 13 June 2011)

Louise emphasised numerous times that her photographs were different to those in the literature and that this gap in the literature was a problem for her. For instance, in a presentation she gave, she said:

What I'm gonna do today is to present some of the data and themes that interested me and also that have raised a number of questions that related to how I can analyse and interpret their photos and how I can represent students' voices (...). As we know that visual methods is still an emerging field, the literature I've been able to draw on to analyse photos has meant me trying to fit existing ways of dealing with photos to my attempt to treat students' photos as both ethnographic artefacts and through photo-elicitation as interview aids. So the photos were used in the interviews to elicit responses. (Presentation, Louise, 14 September 2011)

In the presentation as well as at other times, she addressed how the existing methods required a lot of improvisation to adapt the method to her specific research. She felt, though, that these improvisations took her out of the academic framework, which is not surprising considering her emphasis of the academic values discussed earlier. At the same time, she was well aware that these improvisations were a careful balancing act all academics have to perform.

I don't want to use it as an excuse and say that there's not a lot of people having done visual data, but in some ways it is true. Like, you know, we talk about it being an innovative data, um, sort of data. And I'm like, well is it? Am I just using that as an excuse to say "Ooh, I don't know how to do [it] because no one has done it before."? When I'm like, "Well, they have done it before. They just don't explicitly say it how've they done it." (Louise, 13 June 2011)

Nonetheless, it was a relief when a professor acknowledged to Louise that there was no set way of doing a visual analysis. In the end, she used a variety of methods to do her analysis,

such as content analysis to get a feel for the photographs and discursive analysis for the interviews in which she discussed the photographs with her participants. In her thesis, she wrote that she managed to find her way through, but she voiced her doubts and confusion in describing the process as well. She said that the analysis of qualitative data was inherently messy and the analysis of visual material is difficult in general. She questioned how to do the analysis (similar to what I had heard her say in our conversations) and pointed out that there is still room in the literature to fill this gap. She also acknowledged that her own struggle in *doing* these methods is part of their development and how new knowledge is produced.

Louise struggled to find an appropriate method of analysis for her photographs. The various sources of her photographs and her insistence on a systematic and consistent approach – one in which she connected well enough to earlier work while also doing something new – made the improvisations she had to do difficult. It shows how her (partly self-imposed) reifications about how research should be done played an important role in how she adapted the method to her needs.

Louise's struggle to find an appropriate analysis is not peculiar to creative methods. Methods of data-gathering and, in this case, analysis are never an exact fit (O'Reilly, 2005; Pink, 2007), and improvisations are always part of that process (Ingold & Hallam, 2007). At the same time, the way in which Louise had produced her visual data was not covered by the literature. This gap provides both opportunities for new research and challenges concerning how to improvise from the earlier research.

Paul: Negotiating Different Disciplinary Reifications

This section focuses on how Paul tried to unite his dual disciplinary background in drama and sports sciences in his research and, specifically, in disseminating his work. Analysing his thinking about performative social science and disseminating the work, it became apparent how these processes were shaped by the interaction between the different communities of practice he was involved in. This shows, once again, that the participants' background and the practices they are familiar with play a determining role in the way they apply the creative methods. The methods are embedded in those practices. However, because Paul had a dual background, his approach to the negotiation was interdisciplinary rather than disciplinary.

Paul had employed participant observation for three years at two combat sport gyms, where he participated in the combat sport himself as well observing the people there. He interviewed the athletes and videotaped the interviews. He used this material in his

presentations (two of which I saw), as well as in his written dissemination. In the presentations, he performed re-enactments of specific practices from his field site, which would interact with a video showing recordings of the interviews he conducted with participants, the field site itself, text supporting theory or analysis and references appearing on the screen (for a detailed description of one of the presentations, see Chapter Six: *Creative Methods and Presentations*).

Paul was keen to do such presentations for various reasons. Most pertinently, he studied drama for his Bachelor's and Master's degrees, during which he took electives in sports studies and used his free time to play various sports. His interest alternated between the two subjects. As he put it: "And I kind of flipped back and forth to where I'm like, 'Drama is brilliant!', to where I get to the point where I'm like, 'Oh I hate drama!' You know, we need more sport in drama and stuff like that." (Paul, 10 November 2011). Secondly, in his Master's of Fine Art, Paul gained the technical skills for making multimedia performances. For the dissertation project, each student had to individually produce a multimedia portfolio, which would be a web-based document, incorporating written text and audio-visual material. The students had to produce all the materials themselves, which meant Paul did the filming, sound, editing and so on. There was no formal training; students were expected to educate themselves. There was a computer lab and a technical team to help them realise their creative ideas. In the process of trying out things, Paul took inspiration from television, commercials and videos, and tried to replicate the work of professionals by watching online video tutorials. Through the modules he did in sports studies he got inspired by the work of Brett Smith, Andrew Sparkes and David Brown, who have either advocated or used narrative and performative ways of disseminating their research within the field of sport sciences. Paul recounted a seminar where he met Sparkes, who encouraged him to pursue this "creative narrative stuff" that he did. Finally, Paul told me that he often got really bored and annoyed at conferences. Of course there were really engaging and compelling presentations, but many disappointed him because they lacked the ability to engage the audience and did not give good presentations.

At best, [presenters] seem like they're terrified and don't know what they're doing, and at worst, seem like they really don't want to be there and they can't care less whether you engage with them or their research. (Paul, 10 November 2011)

In reaction, Paul wanted to find a way to make his own presentations more lively and

engaging, giving his audience a sense of what was going on at the site. He used the “creative narrative stuff” to try to bridge the gap between the experiences of the researcher and the audience who had not been present at the research site. He felt that was where a presentation could really contribute.

Yeah, I think, I think for me a big part of it is that desire, and I think it stems from, like, the work of Smith and Sparkes. So a big part of it for me is, is that desire to want to allow the audience into the research or into my experiences or the experiences of my participants. 'Cause I think that more than the theory and more than the analysis, for me I think that's the main way that people can engage or that's a way that people, I can provide an experience or I can hopefully give a window to an experience that differs from what they could get if they were just looking at theory in a textbook or what they could get if they were to do some text analysis themselves. (Paul, 10 November 2011)

At first, his project looked at similarities in how actors and athletes experienced and narrated their practices. This gave Paul a chance to evaluate possibilities for knowledge transfer between the disciplines of sports science and dramatic arts (Paul's thesis, 2012). Although he later narrowed his focus to athletes, the starting point of his PhD was a sign of his dual interests, which continuously returns in the discussion of his work.

Paul told me about how he created his presentations. The development in the presentations was so self-evident to Paul that it was clear these were smooth negotiations for him. Because of his background in drama, he was able to easily translate his field notes into a presentation. He had enough ideas about how to do that, and the struggles he had pertained more to the execution of the performance than content or form. For instance, he worried the DVD player would not work or that he would forget his lines.

For Paul, the bringing together of drama with sports science was exciting, but there were also difficulties in doing so. Those presented him with rough negotiations. He talked about the search for a new common language that could speak to both disciplines, the different ways of dealing with data or anonymity and the lack of a real connection with the other discipline. He said that the two disciplines had a different kind of engagement with theories and empirical material (Schoenberger, 2001, pp. 366-367). The following quote shares his thoughts on the ‘Research and performance’ module that he taught to first-year drama students. One of the things he asked students to do was put theory into a dramatic

presentation. He explained how this process differs from social scientific practice, and at one point said the process was less academic. When I asked him about this, he replied as follows:

The level of analysis is different. It's a different quality of analysis. They're looking at the material to say "Well, how can we make this performative and how can we make sense and how can we understand that performance?" as opposed to saying, "Well, this individual performs this, and this reflects on society in this way," you know. It's kind of a different level of analysis, I suppose, and I suppose what I mean by the less academic is that it's... it's taking greater creative liberties. So, you know, we may be taking somebody's narrative and somebody's story, but we may be reconfiguring that considerably, making real change and real adjustments and presenting something that on face value... you wouldn't think it has any relevance or any relation to the original narrative. (...) I kind of say it's less academic in the sense that I couldn't then take (...) that performance to a sociology conference and say: "Okay, so this is our response to... the narrative, because it's a different... It seems to me it's a different level of engagement, a different level of analysis and so that the two, whilst they're complementary, the two don't necessarily... They're not interchangeable." (...) Well, I don't know, it just seems it'd be a very different process to go present that, you know. It requires so much more... I don't know a different engagement to make that work and to make sense of it, whereas in a drama room, it doesn't. You present that and if it works dramatically, it works dramatically. There's no issue as to [asking yourself]: "Well, was it true to the original text. Does it work dramatically? What's the performance? Is the performance good? Is it valuable?" It's those kind of questions. The questions differ, I suppose that's what I mean. The questions are different, that would need to be posed, need to be answered. (Paul, 30 January 2012)

One of the things happening here was that Paul was starting to understand the differences between the two disciplines' engagement with theory, empirical material, analysis and dissemination. He realised that a drama conference required something different of him than a sociology conference. This is more than an obvious observation. The negotiations are of a different order because the topic has to be placed within the tradition, practices, definitions, theories and reifications of both disciplines, rather than an improvisation from the reifications of one discipline.

Paul discussed the kinds of negotiations between social sciences and the performative

arts with me quite often and at length. He pointed out how the nature of those negotiations differed. Paul felt the way this engagement was currently done in the social sciences was cause for worry for the development of performative social science. There was a lack of recognition that in the engagement with drama and performative work, the social sciences would become something different. Paul had accepted that, but he felt the lack of acceptance by others posed a threat to his own work as well. One of the difficulties with interdisciplinary work is that common goals and concerns are approached from a disciplinary perspective with their own particular definitions, uses and practices (Pink, 2003, p. 191). In those cases, the negotiation could be understood as a colonisation, where there is no real openness to change and one discipline has to adhere to the disciplinary rules, concepts, theories, boundaries and criteria of the other discipline (Gieryn, 1983, pp. 791-792). In Paul's case, the performative arts had to adhere to the disciplinary approach of the social sciences. On the other hand, there are those negotiations that could be understood as an integration, an interdisciplinary approach where out of the engagement with the two disciplines a new approach emerges. Despite its difficulty, Paul felt the integration approach had possibilities, potential and opportunities.

What struck me as something that seemed problematic or dangerous in what (...) I seem to be reading and experiencing from people that are making these calls for more performative ways or creative ways is that... [within the social sciences] there seems to be a desire to put restrictions on that, to safeguard against that. To say that: "Well, we want to be able to still analyse it in the way that we analyse things and we want to still be able to engage with it in the way that we engage with it, but we want a different form of presentation." And so because of that, it has to fit in these categories or these boundaries, or it needs to not surpass these limits. Otherwise, we can't work with it and it becomes something different (...) but I also worry that they're... missing an opportunity to do something very different, and actually for the disciplines to combine and for the disciplines to have a shared dialogue over what this could mean. 'Cause I think performance studies and people in those kind of fields could... could analyse the material in this way. (...) I think they can engage with it in a different way, 'cause they've [performance studies] got a language. They've got a discourse that allows them to do that, which I think could prove useful to sociologists and I think... (...) I just kind of feel if you're gonna... step into other fields or step into other disciplines, I feel that there's a responsibility to work in a way that engages with that

discipline rather than turns that discipline off or... shuts [performance studies] out.
(Paul, 30 January 2012)

So to me, it seems a little bit silly to say: “We want new ways, we want new ways, we want new ways, but the new ways have to be in this format, you know. We want something new, we want something different, but [only if]it falls in this very strict rigid thing that we know and we can deal with.” (Paul, 10 November 2011)

In this way, Paul argued that an interdisciplinary approach which acknowledges both disciplines becomes difficult to achieve because it asks for a real investment in change. He voiced concern that there was a lack of openness, knowledge and acknowledgement of the other discipline (Pink, 2003, p. 191). Without a genuine interest in the other discipline, or its background, there is the danger that it was only superficially applied and in a way that was substandard from the other discipline’s perspective – what Pauwels (2000, pp. 12-13) calls ‘the danger of “amateurism”’.

What is important here is that the research has to be done. It cannot just be a theoretical exercise. Paul strongly felt it was necessary to step out of one’s comfort zone. He advocated for a space where disciplines come together and the fear of bad academic research is temporarily suspended in order to develop a common language, understand what the disciplines can do together and discover the weaknesses in doing so.

[I]f you don’t exercise those things, you don’t try it. How do you know what is good and what is useless, you know? So that’s my only worry with those things and in regards to developing it, I think that in a way you’re gonna have to, you’re gonna come across the gimmicky and you’re gonna come across the things that don’t work. But you kind of need to come across that in order to understand what works and why it works. (Paul, 10 November 2011)

It seems to be just a little unclear at the moment within the field as to what is okay and what’s not okay in regards to... (...) I don’t understand why one set of symbols is acceptable and readable, but another set of symbols isn’t and I just think... the only explanation is that there isn’t that... shared communication between the disciplines. You know, there’s not a shared discourse, (...) [and] I just worry that we’re not necessarily gonna make the developments that the people are calling for if we don’t

allow that explanation. If we don't allow and accept, there're gonna be failures. There're moments where it just doesn't work, but that's valuable. If we find that it doesn't work, then that's valuable, because that creates discourse and a dialogue as to why it didn't work, which surely then help us make sense of what does work. (Paul, 30 January 2012)

[T]here needs to be that balance there. That, yes, you need to... you want something that aesthetically works, that's aesthetically pleasing or aesthetically challenging or does something. But at the same time, it needs to be supported by that critical engagement. It needs to be supported by the theory. (...) Yeah, I think it's a balancing act, isn't it? I think there's got to be an awareness of how you can incorporate the two and then, at the same time, an awareness [as] to what the aesthetic can do. (...) Maybe rather than having to tell them what the analysis is, you can show them the analysis and allow them to experience the analysis themselves or allow them to engage with the material in a way that... maybe, hopefully, better allows them to make their own decisions, their own judgements as to what that means. (Paul, 30 January 2012)

The lack of a safe place for researchers to play with performative social sciences impacted on Paul's capacity to publish papers. As he had yet to find a language to combine the two disciplines in a way he felt was good enough, he chose for a more traditional way of writing. This was, firstly, because he was comfortable with writing in that way and, secondly, for fear of being otherwise rejected by journals. This meant that his own attempts at understanding the two disciplines remained unpublished, which has stood in the way of developing performative social science as Paul would have liked it.

I don't think I've come up with a better terminology or language that explains why this is useful, which, I think, is why the discipline [is] still in that stage, our discipline is still in the stage of struggling with that or trying to articulate that to some extent. (...) I don't know, there just seems something missing from it yet, that I don't feel I've found. So I don't feel that I could... I don't feel comfortable or confident enough at the moment to publish something that says [that] I'm using this particular method, because of XYZ, and it's grounded in this theory or it's based on this or the other. (...) [W]hereas the other way, a more standard paper... it seems on the surface, it seems

easier to do because it seems as though there's a history of that. It's accepted. We don't need to qualify in the introduction why we're writing a paper in a particular way because it's cemented in our discipline, you know. We're.... we're so accustomed to it that it doesn't need an explanation. (Paul, 30 January 2012)

'Cause my biggest worry with all this, and still, is really, because... the danger is, is that I don't want it to become gimmicky. (...) And to be a thing [that makes people think]: "Well, okay that's great that you can do that, but what about the research? We like us to engage with the research." And so that's my biggest fear (...) that balance won't be right. That [people will think]: "Yes, it might be visually stunning. Yes, it might be entertaining, it might be engaging. But we're not really learning anything, so... You know, maybe that's better placed to be, say, for a YouTube channel. You know, we can engage with your research in that way." (Paul, 10 November 2011)

Despite his fear of rejection and lack of confidence in writing this way, Paul used performative elements in his thesis and in at least one journal article. Moreover, he was reaching out to other researchers in the field to create a network of people with whom he could exchange ideas and share attempts at combining the two methods. He was thus actively creating a community of practice with which to negotiate practices for performative social sciences. Notwithstanding his efforts, this section also shows how the communities of practice Paul was involved in at the time and the way he negotiated their reifications tempered his attempts at developing performative social sciences. In this way, his personal negotiations become a way in which creative methods emerge.

During the time I was having conversations with him, Paul was also negotiating the anonymity of his participants in his presentations. This was another rough negotiation for him because the different academic backgrounds he drew on have different ideas about anonymity. In performance studies, anonymisation is not standard practice. Currently in the social sciences, it is heavily debated, especially by researchers using creative methods. Most universities in the UK, however, adopt a policy in which anonymisation and confidentiality are the standard. In his presentations, which were mostly at social scientific conferences, Paul kept his participants clearly recognisable and, in an earlier conversation, he had expressed a hope to be criticised for this. His hope stemmed from his inability to find a good reason for anonymity in the social sciences, as he said.

This is all we ever see of people in psychology and sociology, is these little text boxes [quotes of participants]. And we're told that (...) you know, it's for ethical reasons. We don't have that history in drama. We don't do that in drama, like to show people talking on television in drama is, it's second nature. Of course you would, and we don't see that as a problem in regards to all those poor people, you know, their lives must be ruined now. We don't see it like that. We don't have that same concern. Not that we're not ethical; we don't have the same worries. So I've wanted to present because I've looked in the books, I've tried to find out why, from the sociology and from psychology books and the narrative books, why DO we offer this anonymity? Why? What's the reason? And I've not found it. I can't find a good reason why. And so I deliberately started presenting the video with these formats, because I thought I'm bound to get it. There's bound to be some professor or some academic who's gonna have a bit of a tantrum or kick a bit of fuss and be like: "You can't do that. This is an abomination of the discipline," and give those reasons. And then I'd be like: "Oh and now I understand the reasons. I can decide for myself whether I should or shouldn't be doing this." But it's not come. It's been the opposite reaction. It's been like: "It's so great to see the participants. It's so great to hear and see them and not just read them." (Paul, 10 November 2011)

Paul was actively seeking how to work with the different practices concerning anonymity in the disciplines he engaged with. He studied the different standpoints, but, more importantly, united them in the dissemination of his research in order to see what would happen. He was hoping for help in his negotiation concerning anonymity, but did not receive it in the way he expected. Rather than a reprimand and an explanation in favour of anonymity, he actually touched upon others' wishes for the participants 'voices'. In a later conversation, though, Paul brought the topic up again. He now talked more about if participants would be able to handle it if they were recognised because of his research. Some of the participants Paul worked with were professional athletes, who actually strived for and often already had publicly recognisable identities. For some of his participants, thus, acknowledging their public personae was important. He could therefore freely use the video recordings of their interviews and show them in the presentations. In addition, it felt impossible, or nearly impossible, for him to get his arguments across without revealing the site and the participants in doing his description and analysis of them. By contrast, some amateur participants were still public

figures who had given their informed consent but could not necessarily foresee the consequences of being recognised in public. They lacked experience in handling the media or encounters with strangers. Unlike the professionals, they did not have some sort of support mechanism in place to help them with such potential situations. Fortunately, however, Paul had the technical skills to anonymise his participants without interfering with the storyline or images he wanted to show.

One of the things I thought [of] as a development for that is the use of the editing software for the videos. There's so many filters and so many things that you can do with the footage in the post-editing. Once you've got it in its original format and they're just very simple things. Like you can transform, you can pixelate the video, transform the video so that actually what's being shown you can transform your whole footage into cartoon like material, so that it's... so that the visual is a cartoon image. You can distort it so that it actually still moves and it still speaks, and the audio is still there of what you've filmed, but the visual look is different. You see an almost pencil outline of the individual which of course... makes, provides another layer, maybe, of anonymity of sorts to those individuals. (Paul, 30 January 2012)

Paul's negotiation of this ethical issue was not straightforward. He had to juggle the different expectations of the disciplines, his own responsibilities towards participants and ambitions for his presentations to really engage the audience. His negotiation happened in a context of debate about ethical practices. They arose in relation to the growth of visual methods, in particular, but also to a growing unease within the social sciences with principle-led ethics (Macfarlane, 2010). Such discussions take place in research articles and papers (as discussed in Chapter Three's *On-going Ethical Considerations* section), but also at conferences. For example, at the Second International Visual Methods Conference at the Open University in Milton Keynes, there was great diversity in how participants presented visual materials, to the frustration of some and the relief and exhilaration of others (field notes, 13-15 September 2011). Furthermore, the development of academics' technical skills is on the rise. For instance, in my former department at Loughborough University, researchers who used video recordings for conversation analysis learnt how to anonymise whilst still retaining the data's value.

This shows a development in Paul's thinking about anonymity, where in its last stage at least there was a tendency more towards anonymisation. This is not a simple change of

preference, but a decision made in relation to the position of his participants and the technical skills he had developed. Still, he made a shift in his thinking and practice, which is part of how creative methods emerge.

Paul's background in drama and sports sociology made his approach to creative methods an interdisciplinary one in which he tried to engage with both disciplines. As such, he was much more aware of the negotiations he would have to do. He anticipated them much more than Liza, Richard or Louise did. Although less of a surprise to him, they were not necessarily easier. Striking a balance between being aesthetically pleasing, without becoming gimmicky, and being supported by the theory and critical engagement was not always an easy one. Paul would therefore refrain from bringing them together in some instances, for example, in a research paper.

Conclusion

This chapter showed how each participant learnt new practices by using creative methods. This was not by having 'just read about it', as Liza said; it was only when they actually applied them that the knowledge became embedded within their practices. In this learning process, my participants had to negotiate ideas they had experienced as conflicting in their own practices. The challenges they had with simply adopting the methods determined the shifts they made. Although the reworking of these practices was individualistic, there were also interactions – sometimes invisible – with other people's practices; my participants used the reifications of their communities of practice, which have evolved out of the negotiations between others' practices. They may or may not have completely agreed with them, but they nonetheless related their own work to these reifications.

The issues differed, depending on their academic backgrounds and the communities of practice they were engaged in. Becher and Trowler (2001, p. 97) point out the difficulties in these negotiations.

Methodological innovations, provided they are reasonably straightforward and do not demand a substantial amount of special training, are also relatively easy to take up. Significant changes in theoretical orientation and requirement to adopt complex new techniques (...) are another matter, in that they demand a major reinvestment of time and effort.

Their background and the practices they had been taught throughout the years determined which issues my participants experienced and which negotiations they had to do, but also how they dealt with those negotiations. What was clear, again, was that they resolved those negotiations by trying to embed the methods within their own practices, by approaching the issue from that angle, looking at it with that framework. Again, this shows that change is a gradual process rather than a break with the established – which means this process is better explained by Ingold and Hallam’s (2007, pp. 1-24) concept of improvisation than the concept of innovation (Dodgson and Gann, 2010, pp. 13-14; Wiles, Pain & Crow, 2010). Paul was the only one in between two disciplines and, as such, tried to integrate the two disciplines. Yet, he, too, adapted his work to the discipline he was addressing in order to be understood by the specific discipline. In addition, his negotiations were no harder or easier because of that.

My participants’ improvisations helped shape these methods and methodologies. This chapter clarified how the methods were embedded and how the original framework and the way research is ‘supposed to be done’ (as I discussed in Chapter Two and/or as actually experienced by them) work as reifications. These reifications, then, are the starting points from which the participants approach the method. As such, this chapter provides answers to sub-question two and three.

Chapter Six: Creative Methods and Presentations

Introduction

Presentations are an important part of research dissemination (Rowley-Jolivet, 1999, pp. 4-5; Rowley-Jolivet & Carter-Thomas, 2005, p. 45). Therefore, in this chapter I focus on presentations by the postgraduate research participants and examine how they used creative methods within them. I argue that creative methods were embedded within well-known presentation practices. All presentations used elements and modes familiar in an academic context. This showed how creative methods were still connected to the standard ways of conducting research – reifications – of the communities of practice my participants were part of. Embedding the creative methods in well-known presentation practices helped those creative methods to get accepted because, as Gilbert (1976, p. 287) states, “[n]ovel procedures will be justified in terms of more traditional and more generally accepted ones”. At the same time, my participants needed to make improvisations to the accepted procedures to accommodate the use of photography, film, poetry, performative and narrative elements. This chapter thus furthers my argument that innovation and innovative are unhelpful terms when talking about these creative methods, and that ‘improvisation’ is a more apt word to describe the incremental changes the participants made.

I illustrate this through analysing how the group of postgraduate research participants (Louise, Liza, Richard, Hilde and Paul) portrayed or used their research methods in presentations, thereby partly answering my second sub-question – how social scientists negotiate creative methods into their practices – and third sub-question – how creative methods emerge in that process, as outlined in Chapter one. I used a multimodal approach (Jewitt, 2013; Kress, 2010) to describe in detail the context (institutional and material settings of the presentation, the kind of audience) and the presentation itself: content, layout, design, use of audio-visual aids, kind of software used, the presenter’s gestures and any verbal and written explanations. The multimodal approach enabled me to show how meaning was created in the presentation through my participants’ use of various modes and that the way in which they portrayed and used creative methods was connected to standard presentations (Dodovski & Petreski, 2012, p. 11; Golash-Boza, 2011; Hyland, 1998a; Shalom, 1993; Ventola, 2002, pp. 29-30; Wiertzema & Jansen, 2004, p. 50). As discussed in Chapter Three’s *Transcription of the Presentations: A Multimodal Approach* section, a mode “is a socially shaped and culturally given semiotic resource for making meaning. Image, writing, layout, music, gesture, speech, moving image, soundtrack and 3D objects are examples of modes used in

representation and communication” (Kress, 2010, p. 79, italics in the original). The choices the postgraduate research participants faced were determined by the modes available and the reifications relating to academic presentations. To understand those choices, however, I first need to consider academic presentations more generally, paying attention to what modes and shared repertoires of presentations are common in the social sciences.

Presenting Research: An Academic Practice

Presenting research is an important established academic practice within research communities (Rowley-Jolivet, 1999, pp. 4-5; Rowley-Jolivet & Carter-Thomas, 2005, p. 45).

It is part of a system in which academic knowledge is constructed, disseminated and negotiated so as to build on and push the production of knowledge forward (Lomnitz, 1983;

Rowley-Jolivet, 1999). In addition, it is a way to build up a network of peers and potential collaborators (Byström & Schulz, 2013, p. 21). For both the presenter and her research to be accepted within an academic community, it is therefore important to give good presentations.

A good presentation obviously requires that the research itself be done well, but being able to convey the research process may be even more important. Researchers have to show that they know how to ‘do academia’ when conducting as well as disseminating research. By engaging with their discipline’s discourses and practices, they become part of that specific community and simultaneously perpetuate it (Hyland, 1998b, p. 439; Swales, 1990, pp. 21-32). This engagement with disciplinary discourse and practices is what makes the production of academic knowledge possible. Hyland (1998b, p. 439) writes: “Academic communication is a social activity which functions in disciplinary cultures to facilitate the production of knowledge. Writers must [therefore] organise data and observations into meaningful patterns for readers.” This does not just apply to written, but also to oral disseminations, such as the presentation. The meaningful patterns enable the audience to understand and engage with the presentation and the knowledge presented. These patterns enable them to discuss that knowledge and take it forward. The meaningful patterns in a presentation differ from what is observed in a written dissemination, but, as I show in this chapter, they do contain similar elements.

I understand Hyland’s (1998b, p. 439) “meaningful patterns” to be similar to the shared repertoires of communities of practice (Wenger, 1998) as discussed in Chapter Two’s *Communities of Practice* section. These meaningful patterns or shared repertoires are shaped by and, at the same time, shape the way the presenter can convey certain messages. To do so,

she can tap into the modes at her disposal. These modes and how they are applied “have been shaped by their social, cultural and historical use” (Jewitt, 2013, p. 251), and their availability determines the kind of meanings that can be conveyed. Their uses and meanings are not fixed, and improvisations happen all the time to suit the needs of the individual research. These improvisations extend and adapt how modes can be put to use and the meanings they convey, making small changes to the meaningful patterns (Ingold & Hallam, 2007).

The meaningful patterns in a presentation are shaped by what and how this is presented. The content of the presentation is structured in a certain way by a presenter who may or may not use audio-visual aids to convey that content. Choosing specific modes and how they are put together comprise an important aspect of how meaning is conveyed in a presentation, and many graduate schools therefore offer training sessions on *how* to present. These trainings help polish presentation style, discussing, for example, body posture, tone of voice and slideshow design (be it PowerPoint or not, its fonts, background, colours, contrast, amount of words per slide and slide transitions) (e.g. Workshop Conference Presentation Skills, Graduate School, Loughborough University; Presentation Skills: Good Practice, Skills Team, University of Hull). Students are taught what appropriate modes are and how they should be combined to convey their message. The Skills Team from the University of Hull (Bartram, n.d., pp. 3-4), for instance, advises students to use high-contrast colours and simple fonts and to avoid animation and having too many words per slide. Further, they posit that a slide should function like a paragraph of academic text. The title, in clearly legible text, contains the argument; a visualisation of the evidence appears underneath; an explanation is relayed by the presenter. They advise against using slides with a simple title followed by a bullet point list because research has shown that it is too hard for the audience to read and listen simultaneously. However, as they also state, this is the way it is mostly done:

This is a very **hard lesson** for people creating academic presentations who feel that a good slide has a simple title then a list of points about that title. Most students will only ever see presentations where the majority of slides use that format and most will only ever create presentations which use that slide format. (Bartram, n.d., p. 4, bold in the original)

Even though this is perhaps not the best way to convey meaning, it is *a* way the modes of writing, layout and speech are often configured and, as such, become a meaningful pattern.

The *what* of a presentation is less prescribed in these trainings, as the content of a presentation depends on the research itself. There are, however, a few studies on academic presentations and conferences in general.³⁹ Although they have mainly focused on the language and rhetoric of presentations (Carter-Thomas & Rowley-Jolivet, 2003; Dubois, 1980; McKinlay & Potter, 1987; Rowley-Jolivet 1999; 2002; 2004; Rowley-Jolivet & Carter-Thomas, 2005; Shalom, 1993; Swales, 2004, pp. 197-202; Ventola, Shalom & Thompson, 2002), these studies provide insight into the structure of how researchers disseminate their research. The literature on research articles (Hyland, 1998a; Shalom, 1993; Swales, 1990), another form of dissemination, is also helpful because the structure of presentations is often similar to that of articles, research proposals and in-progress reports (O'Reilly, 2005, pp. 208-210; Rowley-Jolivet & Carter-Thomas, 2005; Weissberg, 1993). There are, of course, differences between the two types of dissemination such as in purpose, length, social setting, language use and the way the verbal and visual work together (Dubois, 1980; Rowley-Jolivet, 2002, p. 21; Swales 1990; Weissberg, 1993, p. 23), which all affect the way each format takes shape. Nonetheless, this literature provides a good starting point to discuss the different elements of presentations and their structures.

In general, the following overall structure for presentations can be distilled from that literature: presenters are expected to cover the introduction, theory and research question, methods, results, discussion and conclusion (Dodovski & Petreski, 2012, p. 11; Golash-Boza, 2011; Hyland, 1998a; Shalom, 1993; Ventola, 2002, pp. 29-30; Wiertzema & Jansen, 2004, p. 50). This is *one* meaningful pattern, but no doubt variations are made possible by adding or omitting elements, depending on the stage of the research process, the time available and the purpose of the presentation. These are the normal improvisations (Hallam & Ingold, 2007) that academics do to negotiate the structure of the presentation so that it fits their own particular research (Wenger, 1998). Despite such variations, it would be strange to completely reverse the order of the presentation: for example, to end with a description of the methods after the presenter has given examples of the data or to create a presentation consisting of nothing more than quotes from participants. The quotes tend to be embedded within a context of theory, method or analysis. This is not to say that such presentations do not exist in an academic context, but they are, at present anyway, exceptions to the rule.

³⁹ This comes in stark contrast to the large amount of literature on poster presentations. For an overview, see MacIntosh-Murray (2007).

Apart from the overall structure, research has been done on the rhetorical moves used in introductions of research articles (Swales, 1990) and conference presentations (Rowley-Jolivet & Carter-Thomas, 2005). This research has shown there are typical ways of introducing the research, be it in an article or a presentation. Again, there are variations in the specific elements and their sequence, but the main idea of the introduction is to provide context for and legitimation of the research so the audience knows how to interpret what is said afterwards. Swales (1990), for instance, has shown that introductions of research articles are often structured according to the Create A Research Space (CARS) model, in which four rhetorical moves are made. First, the field of study is established. Second, previous research is summarised. Third, the gap in the literature is identified. Finally, the research under discussion is situated in a way that helps fill the gap and is then described. Hyland (1998a, p. 26) offers a slightly different sequence: establishing a territory, establishing a niche, occupying that niche.

Rowley-Jolivet and Carter-Thomas (2005) used the CARS model to study the introductions of academic presentations at geology, medicine and physics conferences. They found that speakers introduced their 15- to 20-minute talks as follows: setting up the framework (preparing the audience for what is to come, e.g. saying: “I am going to talk about X today, and my presentation has the following structure...”); contextualising the topic (discussing other research and how it relates to the current presentation); and stating the research rationale (the relevance of the research and the knowledge gap it fills). Each of these three rhetorical moves, as they call them, helps the presenter “to facilitate interpretation of the talk” (p. 46) and “to create an atmosphere conducive to the effective transmission of the discourse from the outset” (p. 65).

Both Swales (1990, p. 142) and Rowley-Jolivet and Carter-Thomas (2005, p. 56) acknowledge that the order of the structure of the introduction is not fixed, but rather context-specific and variations are ample. This has to do with different disciplinary expectations and practices, the specific needs of the individual presentations and the live nature of the presentation, whereby presenters can make impromptu decisions (Carter-Thomas & Rowley-Jolivet, 2003, p. 8). Which modes the presenters use in their presentations and their configuration is not prescribed by the CARS model (Swales, 1990); it only outlines the structure of the content. With each new presentation, improvisations (Ingold & Hallam, 2007) are made to modes and structure of the introduction in order to convey the research as clearly as possible. This structure shows the need to lead the audience to the right interpretation of the research, a need not confined to geology, medicine or

physics, but rather universal across disciplines. What's more, this guiding of the interpretation does not stop at the introduction, but continues throughout the presentation.

In order to convey their research, presenters thus have various modes to their disposal to create meaningful patterns in their presentations, such as body posture, tone of voice and slideshow design, the overall structure of their presentation and the rhetorical moves in the introduction, such as the CARS model (Swales, 1990). These meaningful patterns and the modes available help determine how my participants designed their presentations and how they incorporated creative methods in them. In the analysis of the presentations I use these to describe them with.

Context of Presentations

Next to the rhetorical structures and the content of the research discussed in the previous section, the context in which presentations are given helps determine their structure and shape (Shalom, 2002). "The context shapes the resources for meaning making and how these are selected and designed" (Jewitt, 2013, p. 252). Presentations and conferences are part of the exchange of knowledge (Lomnitz, 1983; Mulkay, 1972), yet how and why they are structured and organised as they are has not been systematically documented, to my knowledge. The following discussion of the context in which the presentations are shaped is therefore based on my own experiences of having organised, presented and worked at numerous conferences.

In some cases, an event is specifically organised around a presentation, but more likely presentations are prepared in response to a call for papers released by an academic association or institute. Such a call invites certain topics and types of presentations, working as a self-selecting mechanism as well as a guideline for the presentation. Researchers respond to a call when they think their research fulfils the description and focus on those parts of their research that fit the theme of a conference. Furthermore, the association or institute organising the conference plays a part in how the event takes shape: not only in the organisational choices they make (e.g. where and when the event takes place, how many presentations there will be and how long they last), but also in the kind of work their reputation attracts. In addition, the event attracts a certain audience (academic or otherwise), which the presenters must keep in mind when preparing the presentation.

Apart from the organisational choices, presenters have to work with the material setting in which they present, thus providing them with certain modes (Kress; 2010, p. 79).

Normally, academic conferences take place at a university or conference centre, where the presentations are given in lecture theatres or seminar rooms. The set-up often consists of a screen at the front of the room on which the slides, photographs or video can be displayed; a table or lectern with a computer and audio-visual equipment; and rows of seats, sometimes with desks. Parallel sessions usually have a chair who introduces the presentation and moderates the question and answer session afterwards. These presentations usually last 15 to 20 minutes (MacIntosh-Murray, 2007, p. 365). Of course, contextual factors can vary. There might be more equipment available, or the sessions may be structured differently, according to the needs of the conference and its presenters. The organisers, delegates and participants form a community of practice, all of whom improvise on reifications emerging from previous conferences to fit the specific needs of this one.

It was within such contexts that my postgraduate research participants had to develop their presentations, providing certain guidelines for the participants to take into consideration. The context offered modes with which their presentations made meaning, and which were accepted or rejected as valuable contributions to academic knowledge. However, there were multiple ways to interpret those guidelines and employ the academic resources. The different backgrounds of the postgraduate research participants, their aims for a particular presentation, their disciplinary field and the event itself helped determine this interpretation and employment, which resulted in the presentation I attended.

Presentations

In this section, I look at the individual presentations of the postgraduate research participants whom I introduced in Chapter Three's *Participants* section. For each presentation, I describe the context in which it was held before turning to the presentation itself⁴⁰ and the role of creative methods in it. The postgraduate research participants mostly used PowerPoint, though also used Prezi and a DVD. All participants embedded creative methods within certain academic practices, such as the traditional overall structure of the presentation and the rhetorical moves in the introduction, such as the CARS model. In addition, they used other academic practices, such as citing references and attesting to the rigour of the methods. At the same time, Hilde and Paul improvised more with the creative methods than Louise, Liza and Richard. Their ability to do so, stems from their personal backgrounds and the communities of practice Hilde and Paul were a part of, which not only encouraged them, but

⁴⁰ I have inserted images of the presentations throughout the chapter, but for a full overview see appendix vii.

also gave them access to skills to improvise more than the others.

Louise

Louise had done her research at a school where she used participant observation, group interviews, and photo-elicitation with the students of the school. In this particular presentation, she focused on photographs produced by three of her participants to show how each demonstrated different discourses on her research topic. She used PowerPoint, although was initially going to use Prezi.⁴¹ The Prezi presentation, however, had a technical malfunction the day before and Louise quickly had to make a new presentation. She used the same content and structure, but resorted back to PowerPoint. The failing of her Prezi presentation was interesting because it showed how her presentation was both enabled and disabled by presentation software and how Louise relied on it to structure her presentation. In addition, Louise's presentation was shaped by the context of the conference. Further, the presentation's structure, its introduction and her discussion of the photographs showed how Louise embedded her use of creative methods in academic practices and improvised on them. She thereby re-inscribed as well as incrementally changed these practices.

Louise presented at an international conference specifically focused on visual methods, which was held at a university. According to the list in the programme book, the delegates were academics ranging from postgraduates to professors, but there were also non-academic delegates such as artists and social workers. This was also reflected in the keynote speeches, which were given by two academics from different disciplines and a film-maker. Louise had responded to the call for papers, which invited proposals from the *broad* field of visual methods. Specifically, it stated: "Please summarise your proposal – whether for a paper, a themed session, a screening performance, exhibition or *something else*" (field notes, programme book; italics added). What the "something else" entailed was unclear from the call, but it indicated that the organisers were open to forms of communication other than paper presentations. According to the programme book, there were exhibitions, film screenings, exhibitions, workshops and book launches next to the parallel sessions. The conference programme committee members (all of them working with qualitative, interpretive and reflexive approaches within visual methods) and the specific areas from

⁴¹ Prezi is presentation software which works with Flash Player. It is possible to show the presentation offline, but it is more user-friendly to do so online. Prezi uses different modes and configures them differently. For example, it works by zooming in and out of the larger canvas', as it is called. The templates are somewhat less formal than PowerPoint; and the order of a PowerPoint presentation is necessarily linear, whereas in Prezi it is not. See, for example, bit.ly/1gbQKhO.

which proposals were solicited (e.g. arts-based visual research methods and participatory visual methods) further indicated the kind of research and presentations that would be welcomed at the conference (field notes, programme book). This showed that a wide variety of formats was possible for the presentation of research. Indeed, besides presentations read from paper, there were presentations resembling the critique session in art school. Another example I saw was a pre-recorded presentation whereby the researchers had made a kind of documentary in which they interviewed each other and showed images of their research topic while a voiceover explained more about the research (field notes). Louise used a format she herself called “fairly traditional” and which was similar to the overall structure of presentations found in the literature (Dodovski & Petreski, 2012, p. 11; Golash-Boza, 2011; Hyland, 1998a; Shalom, 1993; Ventola, 2002, pp. 29-30; Wiertzema & Jansen, 2004, p. 50).

I did it in what I consider to be a sort of fairly traditional way. So: like an intro slide, like the background to the study, a bit of theory, method, like two slides on – one on methodology and one on... sort of sample and participants and what I got them to do – and then I had a few slides on findings and then a conclusion slide. (Louise, interview, 8 July 2011)

Because it was a well-known format within academia, the traditional overall structure was one way in which Louise provided a meaningful pattern for her audience, embedding her research in academia. The rhetorical moves in the introduction of her presentation were another meaningful academic pattern as she followed the CARS model almost to the letter: establishing a field of study; summarising previous research; identifying the gap in the literature; and finally placing the current research in the gap (Swales, 1990). She used different modes to do that (Jewitt, 2013; Kress, 2010). For instance, in her spoken introduction, she said:

My name is [Louise]. I'm from [X] University and I'm at the department of Sport Science and particularly within the research group of Sociology of Sports... And I'm going to present today some of the research that I completed for my PhD thesis and... my work is about [participants' group experiences of [X]]. And what I did was a collaborative photography project with young people in [setting X].

Louise established her field in this introduction by stating her department and particular research group, the topic of her research and the kind of project she had done: a collaborative photography project. In addition, she mentioned that it had been research done for her PhD, emphasising the academic nature of her research within the context of this particular conference, which was broader than academia.

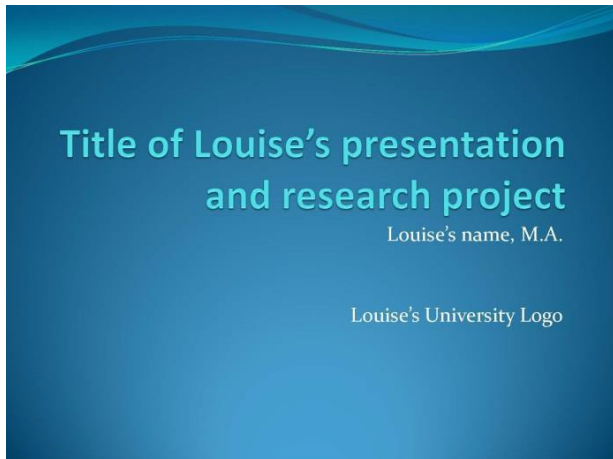


Image 2

This spoken establishment of the field was supported by her slides (see image 2). The slides were formatted in one of PowerPoint's templates and she used this theme throughout the presentation. The title of her presentation appeared at the top; in the bottom right was her name, her Master's title and the logo of her university. These three items connected Louise's research to academia, a connection which at this conference was not evident. The title of her presentation, "Using participatory photography to explore [X] and [Y] among young people", was a reiteration of Louise's spoken introduction.

The use of modes was somewhat interchangeable here, but they complemented and emphasised each other in establishing the field. Citing her department and research group, positioned herself in a specific field. The title of her presentation on the slide and Louise's short spoken introduction of her research topic and method further specified her position within that field.

Louise then summarised previous research (step two of the CARS model) with which she also identified the gap (step three of the CARS model) (Rowley-Jolivet & Carter-Thomas, 2005; Swales, 1990). On two slides, the summary was visualised as follows (see image 3). Each slide had a theoretical concept as its heading. Underneath, a bullet point list indicated different aspects of this theoretical concept. Each bullet point was a one-sentence summary, followed by in-text literature references. The second slide also contained a quote from that literature.

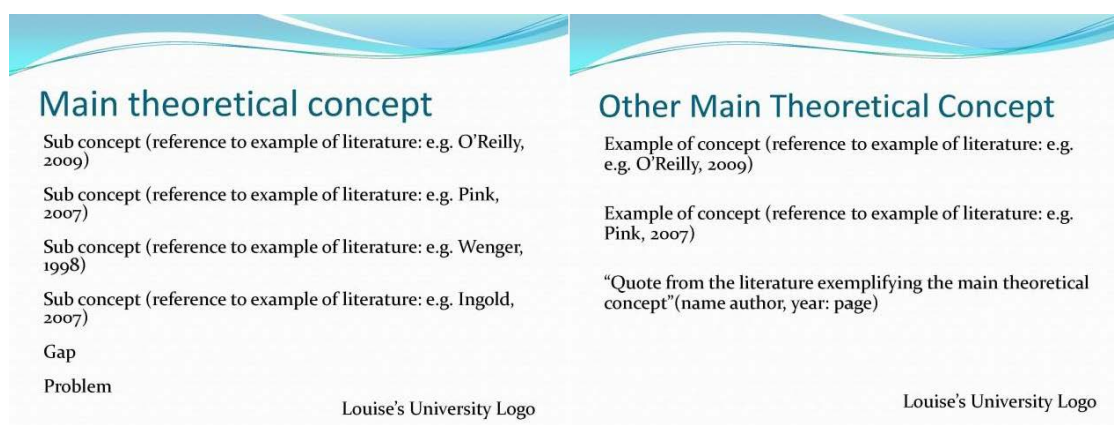


Image 3

Naming these concepts and the in-text references to the work of others are an important aspect of academic practice. Dubois (1988), Gilbert (1977) and Hyland (2004) argue that citations help persuade the audience of the claims made in the text. Citing research provides justification for arguments because it situates the work in a particular tradition and acknowledges other research. The quality of the works referenced can reflect on the researcher's own work (Gilbert, 1977 p. 116), and it provides the audience with the means to scrutinise the interpretation as they can look up the references (Spencer, 2007, p. 447). Additionally, citing helps to identify where there are gaps in the literature. Citations are thus significant in providing the research with credibility and legitimation (Hyland, 2004, p. 20; Spencer, 2007, p. 447).

Louise's verbal explanation mostly reiterated what was on the slide, though also helped bring these concepts together in her research questions. This led the audience to understand these concepts as her specific focus, as the niche she was going to occupy. The following sentences are examples of how she did that, discussing each bullet point, summarising previous literature: "For my research I drew on a range of literature in [sub-discipline X] and [sub-discipline X] that finds that [concept] is [X]." "There is a lot of research beginning to be done within the sociology of sports that finds a concern that [specific people] in particular are a 'problem'." "Other studies in youth sports find that young people learn about their bodies and ways of moving from the visual."

Louise's explicit use of the CARS model (Swales, 1990), not only provided a meaningful academic pattern, but her specific interpretation of this model for the introduction (the literature references, quoting from the literature, her Master's title, mentioning this research was done for her PhD) made use of academic practices, thereby reinforcing them. Her verbal explanation and the slides connected her topic to social scientific problems, theories and methods. This helped Louise place her findings within

certain traditions, thus providing a context in which they were to be interpreted (Spencer, 2007, p. 447). This meant that when Louise showed them to the audience, the photographs were already well embedded within an academic perspective.

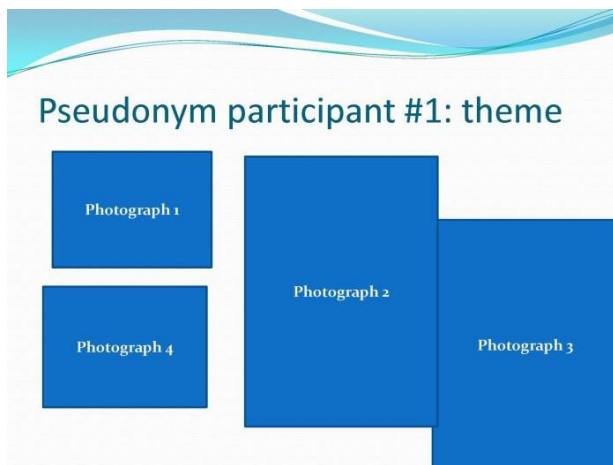


Image 4

The main part of Louise's presentation consisted of her discussion of photographs taken by three of her participants. Here as well, Louise embedded them in established academic practices. Louise discussed each participant individually. Each first slide (see image 4) of a participant's photographs had a title at the top, which was the pseudonym of the participant, followed by a heading, which was the name of the specific theme the participant and their photographs represented.⁴² Each slide contained a different number of photographs, which were placed in various positions across the slide. Louise had anonymised all the photographs by blurring any recognisable faces. This showed another mode Louise applied to adhere to the academic practices required by her university. The practice of providing participants with anonymity and confidentiality is a contested one, particularly with the rise of photography and film in social science research methods (Beddall-Hill, 2011; Boxall & Ralph, 2009; Prosser, Clark & Wiles, 2008; Wiles, Coffey, Robison & Heath, 2010; Wiles, Coffey, Robison & Prosser, 2012). In some cases, an excerpt from an interview with the participant accompanied the photographs. With each set of photographs, Louise explained who had produced them and what the photographs represented, thereby framing their interpretation. Excerpts of interviews were read out loud, recreating the setting of the interview. The description of the photographs was accompanied by her interpretation of them.

How Louise configured the different modes here was similar to an academic text's paragraph (Bartram, n.d. p. 4). The title conveyed the argument; the photographs and the excerpts from the interviews showed the anonymised evidence for the argument

⁴² See appendix vii for the other slides.

exemplifying the point Louise wanted to make; and Louise explained the argument by describing the aspects of the photographs that the audience should pay attention to and what they meant. The modes thus worked closely together in conveying Louise's interpretation: the photographs were anchored by the written titles, Louise's explanation and sometimes the interview excerpt. The verbal and written explanations needed the photographs to show and thereby support the argument. This combination of modes thus perpetuated academic practices, even in the way Louise used the photographs.

Despite room for doing "something else" in the call for papers, Louise's presentation adhered to a well-known structure for academic presentations (Dodovski & Petreski, 2012, p. 11; Golash-Boza, 2011; Hyland, 1998a; Shalom, 1993; Wiertzema & Jansen, 2004, p. 50). Together with her introduction, a clear example of the CARS model (Swales, 1990), the presentation thus worked as a way to structure the interpretation. Furthermore, she connected her work to academic practices through using, for example, in-text references and her own academic title. Finally, the configuration of the different modes functioned like an academic paragraph, in which the photographs were presented as the visual evidence of the argument she was making (their interpretation already structured by the rhetorical moves the introduction). They represented the different themes of her analysis, similar to how the use of quotes might function in a text. Creative methods enabled Louise to show photographs as her data, but her use of them was well embedded within an academic framework and, as such, presented as an appropriate way of doing research. Louise's use of visual methods thus re-inscribed academic practices and conformed rather than improvised on these practices.

Liza

In her work with young people, Liza had used a variety of qualitative methods: participant observation, life story interviews, focus group discussions, photo diaries, written diaries and a video documentary. In her presentation, she focused on how she used these methods and their advantages and disadvantages. Similarly to Louise, Liza embedded her presentation and her creative methods in academic practices through the rhetorical moves in her introduction and the modes she employed to discuss the methods' advantages and disadvantages.

In terms of context, Liza presented at an annual national conference for postgraduate researchers, which was organised by a group of postgraduate researchers each year at a different UK university. The conference focused on a specific sub-discipline (e.g. migrant studies or leisure and recreation) and invited papers from different disciplines within the

social sciences. The possible themes in the call for papers were specific topics within this sub-discipline, its history and future, but also “data uses and methodological approaches in [X] studies (inc. innovative methods)” (field notes, call for papers). Liza’s research fitted with both the theme of the specific sub-discipline and the methodological approach. Although the methods she used were more often applied within this particular sub-discipline, at this conference, Liza was an exception. From the abstract book and the presentations I attended at the conference, I gathered that the majority of methods used in the research presented was quantitative. Those presenters who did use qualitative methods often used interviews, sometimes in combination with participant observation and document analysis.

She gave her PowerPoint presentation in a lecture room with tiered seating. In the front of the room was a screen and a lectern which encompassed the audio-visual equipment. She started with a standard introduction using Rowley-Jolivet and Carter-Thomas’ rhetorical moves (2005): setting up the framework, then contextualising the topic and finally stating the research rationale. The framework was set up through Liza’s title slide and her spoken introduction. She started by saying: “Good morning everyone, in my presentation this morning I am going to talk about my personal experience of how to use different qualitative methods to investigate everyday experiences of young people in [place X].”



Image 5

Behind her on the screen was the title slide (see image 5), which read: “Combining multiple qualitative methods to explore [X] amongst youth in [place X].” Given underneath were Liza’s university, department of geography and a line saying the research had been made possible through a grant from a geography society. Aiming to promote the advancement of geography, the society supported research through grants for students, both early-career and senior researchers at universities. The slide also showed three photographs with people in different locations. There was no explicit information given about the photographs, but

within this context it was apparent that they showed places and people from Liza's research. She used the different modes of written and spoken words and the photographs to set up the framework by conveying to the audience what kind of research she had done, her topic, with whom her research had been conducted and in what context. Each mode helped Liza prepare the audience for the rest of the presentation and to give a specific interpretation of the rhetorical moves (Rowley-Jolivet & Carter-Thomas, 2005; Swales, 1990).

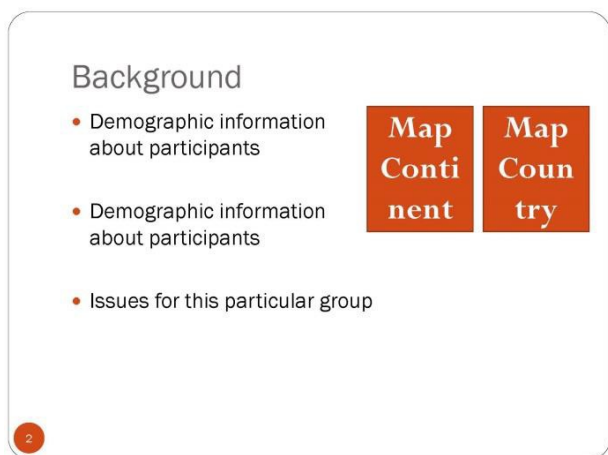


Image 6

Liza went onto the next step – contextualising the topic – by clicking through to the next slide (see image 6). She said: “First of all I want to give you some background information to let you know why I chose to investigate that topic.” On the top left of the slide was the heading “Background” and underneath three bullet points gave information about the participants’ demographics and specific issues they faced. On the top right were two maps showing the area where Liza did her research. One was a larger map of the continent, the other a more detailed map of the country. The information written on the slide told the audience why Liza had done the research, which was complemented by maps showing where she had done it. Liza verbally explained what was on the slide.

Young people with [X] are considered amongst the poorest and most marginalised of the world youth. Nothing is known about their lived experience and how they make sense of their lives. And just to let you know where [place X] and [country Y] is, it is on the (...) coast and I was spending most of my time in [place X], which you can see over here [Simultaneously, Liza pointed to the general direction of the maps without indicating anything specific, to emphasise there was a specific place she had been to do her research. Young people in [place X] face very similar issues in their lives, like access to education, employment, health care and social services. Young people with

[X] in [place X] have more or less the same issues, but in much more complex ways. So, because so little is known about their experiences, I decided to use a qualitative approach.

Having said twice how little was known about these participants' everyday experiences led Liza to the research rationale (step three in the CARS model (Swales, 1990)). On the slide, it read: "Objective and Research Questions" and again, Liza verbally explained through reading aloud what was on the slide (see image 7).

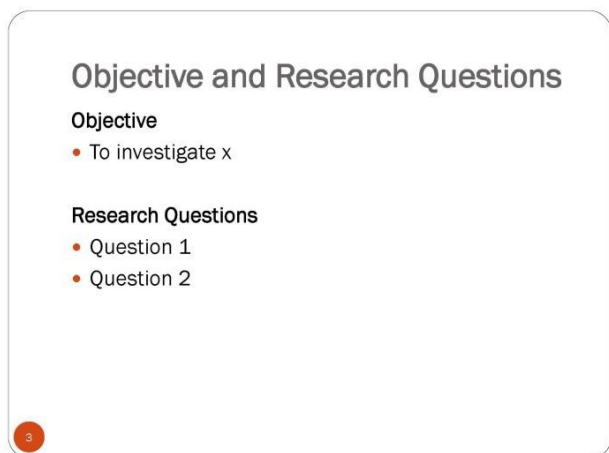


Image 7

In the first three slides ("Title"; "Background"; "Objective and Research Questions") and through her verbal explanation, Liza improvised on the rhetorical moves Swales (1990) and Rowley-Jolivet and Carter-Thomas (2005) described. This introduction helped Liza frame her research as academic, and was supported by her verbal explanation and written information on the slides. For instance, the title slide contained her academic title and the university she was from, the names and academic titles of her supervisors, and at the bottom was an acknowledgement of the society that had granted her a postgraduate research award. The photographs and maps did not necessarily frame the research as academic, but rather added to the contextualisation of the research.

By now her research was well established in an academic framework and Liza started her in-depth explanation of all the methods she had used. This discussion was a mixture of facts and reflections on how she had used them. She discussed each method point by point: what they entailed; where and with whom she had used them; what kind of knowledge she had been able to produce with them; and what she had found their advantages and disadvantages to be. In Liza's discussion of advantages and disadvantages, she used a variety

of modes (Jewitt, 2013; Kress, 2010), such as written text, images and symbols, all of which – through layout, use of colours and typography – worked to embed her work in academia. This helped her convey the appropriateness of these methods. In the following section I discuss only four main examples of how the variety of modes worked together, though Liza employed this throughout the presentation.⁴³ In these examples, Liza’s use of the creative methods conforms to the well-known meaningful patterns discussed at the beginning of this chapter.

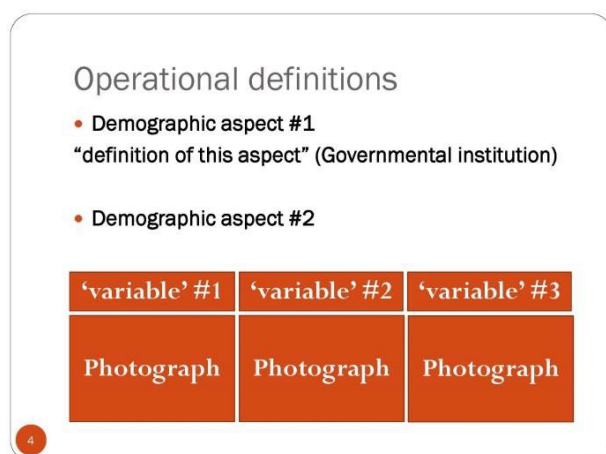


Image 8

A first example concerns the slide on her participants (see image 8). This slide had the heading “Operational Definitions” in the top left corner. Underneath there were two bullet points. The first read “Youth” in bold and underneath was a definition of youth, including a literature reference. The second bullet point was another demographic criteria, written in bold. Within this criteria were three subcategories, exemplified by three photographs Liza had taken for her research. Each photograph had as a heading a specific subcategory and the photograph showed people who fit that subcategory. In addition, Lisa verbally explained the different subcategories. The different modes complemented each other: the verbal and the written words explained what the audience saw and, crucially, linked what was seen in the photographs to substantiated definitions of participant categories (Jewitt, 2013; Kress, 2010). In turn, the photographs acted as illustrations of the participants and field where Liza had done her research. Additionally, they worked as evidence that she had been there. This is a rhetorical strategy Marcus and Cushman (1982, p. 33) call the marking of the fieldwork, which is common within ethnographies. O’Reilly (2005, pp. 213-215) describes this as illustrating “being there”, and showing what Bryman (2008, p. 685) calls “experiential authority”, having “the appearance of a reliable witness”. Thus, both the visual and the written

⁴³ See appendix vii for the full presentation.

modes embedded Liza's work in academic practices, albeit different ones. The photographs were also illustrative in the sense that they let the audience visualise who the participants were.

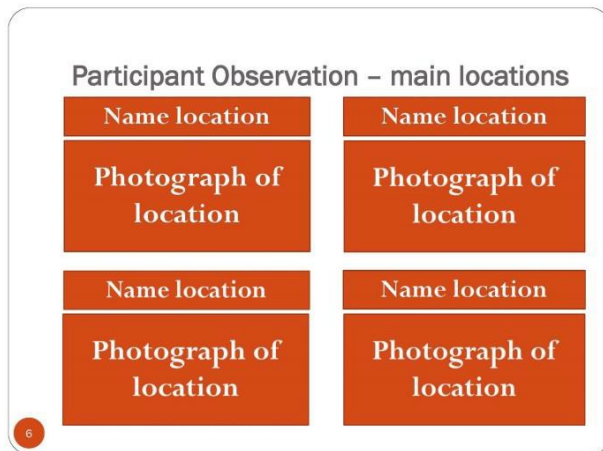


Image 9

In the following example a similar configuration of modes was at work (see image 9). One of the slides on participant observation had a square formation comprising four photographs, each of which had the name of the location written above. In the top left, it read "Participant Observation – main locations". Each photograph showed one of the specific locations where Liza had done her participant observation. The photographs instantly gave a sense of the kind of places these were, similar to how the photographs of the participants served as illustrations of these places and as evidence Liza that had 'been there' (Marcus & Cushman, 1982, p. 33; O'Reilly, 2005, pp. 213-215). In addition, the written words, both the names of the places and the title, made it clear what these photographs represented. The modes conveyed different, supplementary information. What is important to note in both these examples is that because she had used photography in her methods, Liza had this mode at her disposal. As illustrations the photographs to conform to academic practices, but still help to envision Liza's field and participants, which was quite different from a Western perspective.

On other slides, the configurations of modes showed a combination of the written bullet point lists of advantages and disadvantages, quotes, photographs, video and cross-tabulations. In the tabulations, the participants were plotted in cells according to specific demographics. This combination of modes was used by Liza for the life story interviews, focus group discussions, photo diaries, written diaries and the documentary. The photo diaries slide was a nice example of such a configuration of modes (see image 10), where the different modes conveyed different information.

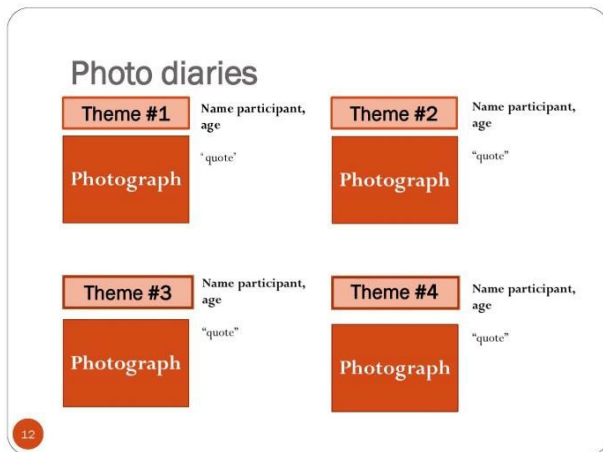


Image 10

Liza explained:

What I also did was photo diaries. I equipped eight people with disposable cameras to take pictures of people, situations, activities and places that they liked and disliked. And then we had interviews afterwards [about] why they took pictures of those places. They could keep [the photographs]. And that was a very good method to get access to places that you normally don't have access to.

Liza also told the audience how the participants in addition to producing the photographs for her research, used them to their own advantage as well. She explained thus how the process of data-gathering went. The slide, on the other hand, showed the results of her analysis. Like the slide on Participant Observation, there were four themes placed across the slide like a square. For each of these, there was an example photograph with the theme as a caption written on top. To the right of the photograph was the name of a participant, his or her age and an interview excerpt relating to the photograph and theme. Liza said very little substantive about the themes on the slide: "And here are a few statements to places, situations, and people and so forth." She kept quiet for a few seconds to allow the audience to read the slide, but then quickly continued to the next slide. Similarly, when she showed the documentary she had made with one of her participants, she used different modes to convey different information. She verbally explained how the process of producing the documentary went, explaining how her participant came up with all the ideas and that she 'merely' followed his lead. She let the documentary show a typical day in the life of one of her participants and the issues he encountered.

Each of these examples showed how Liza used different modes – the photographs,

the cross-tabulations and the written and verbal explanations – to convey how she had done her research. She used her photographs in different ways. In her discussion of the photo diaries, she presented them as examples of her findings, but in other instances, such as participant observation, they showed Liza in the field, doing the research, which made them examples of marking the fieldwork experience (Marcus & Cushman, 1982, p. 33). This was dependent on the configuration on the slide and Liza's discussion of the photographs. The cross-tabulations gave a clear overview of how many participants there had been for a specific method and their distribution along certain demographics. Finally, the verbal and written explanations conveyed the advantages and disadvantages and anchored the photographs.

These configurations of the modes were one way in which Liza embedded her research and the methods she used in academic practices. She also did this by using the rhetorical moves in Rowley-Jolivet and Carter-Thomas' (2005) introduction. As a meaningful pattern, these rhetorical moves helped Liza to set up a framework within which her research could be understood. Likewise, by inserting certain academic elements, such as, the academic titles of her supervisors, the references to the university, her research as part of a postgraduate degree and the acknowledgment of the society supporting her research, she further established her work as academic. Additionally, she used qualitative ways of representation such as the photographs and quotes of interviews.

Her use of photography and film in her research made these modes available to use in the presentation. Therefore, she needed to reconfigure the various modes to include the creative methods in her presentation. Whether or not Liza was improvising by including those creative methods depends on perspective. Within the context of a more quantitatively orientated conference, this can be considered an improvisation. That said, the creative methods were also clearly embedded within reified research practices and, as such, normalised.

Richard

In his research, Richard worked with adolescents and used group interviews, participant-driven photography and video. This presentation focused on his on-going research and Richard showed some of the preliminary data. He used Prezi, and while this made for a different reconfiguration of modes than in PowerPoint, his work was still embedded in academia. This became clear in the introduction, his discussion of the methods and the

presentation of the findings.

Richard's presentation was part of an on-going series of presentations by postgraduate researchers from his department. Every postgraduate researcher in his department was expected to give a presentation about his or her research at these events, usually during the second year. As such, there was no call for papers, but the postgraduate researchers usually talked about their on-going research. The event was held at his university in a lecture room. The room had a lectern at the front with audio-visual equipment, a screen on which to project the presentation and tiered seating. At the event, there were poster sessions as well, but Richard was the only speaker. The audience consisted mainly of people from his department, postgraduate researchers, lecturers and professors. In terms of methods, Richard's department was very quantitatively orientated. Lab experiments were widely used and behaviour change was often one of the research aims. Richard and a few of his colleagues were the exceptions who used qualitative methods.

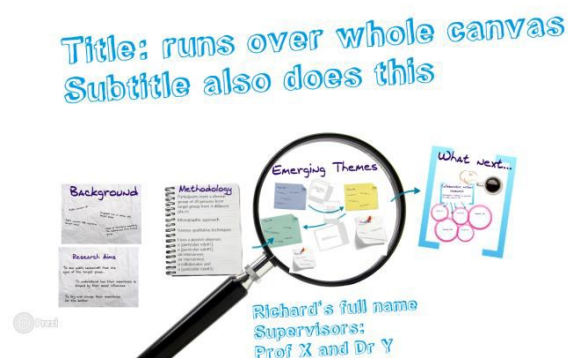


Image 11

Richard started his presentation following Rowley-Jolivet and Carter-Thomas' (2005, p. 46) model discussed above: setting up the framework, contextualising the topic and stating the research rationale. Because he used Prezi, the whole presentation was visible on the screen at once (see image 11). This is Prezi's default setting for the start of a presentation. From left to right, the structure of the presentation was visible, which gives the audience an immediate overview and a glimpse of the different slides. The layout of the presentation was light and playful, with bright colours and images of objects such as a magnifying glass, a cup of coffee, a notebook and sticky notes. In the overview, the title and subtitle of his presentation, his name, those of his supervisors and the magnifying glass with the heading "Emerging Themes" instantly grabbed attention because they were bigger than the other words and images in the overview. Richard only mentioned the academic titles of his supervisors. Because the event was at his own university, it would have been strange for him to say he

was a PhD student there or which department he belonged to. The names of his supervisors were relevant, as not everyone knew who supervised Richard, and they also identified which research group he belonged to and helped the audience situate him theoretically, methodologically and in terms of topical expectations.

Richard introduced himself and gave a short outline of what he was going to talk about. As said, the whole presentation was visible on the screen, and while Richard was taking the audience through the structure, he would move from left to right in front of the screen. He would stretch out his arm, pointing towards the different elements he was talking about and each time signalling a move to the next step in the presentation. These different elements were titled “Background”, “Research Aims”, “Methodology”, “Emerging Themes” and “What next...”. This was a variation on traditional presentation structures (Dodovski & Petreski, 2012; Golash-Boza, 2011; Hyland, 1998a; Rowley-Jolivet & Carter-Thomas, 2005; Shalom, 1993; Swales, 1990; Wiertzema & Jansen, 2004, p. 50). His gestures in combination with the overview of the presentation and the introduction of himself and the presentation helped Richard set up the framework (Rowley-Jolivet & Carter-Thomas, 2005, p. 46).

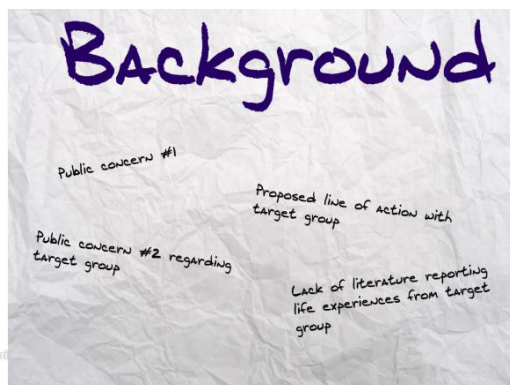


Image 12

Richard went to the next step of the introduction, contextualising the topic by saying: “First a bit of context then...” Simultaneously, the Prezi presentation zoomed in on the part of the canvas containing the background ‘slide’ (see image 12). It was designed to look like a crumpled piece of paper, on which notes had been scribbled here and there. Although it looked less formal than a PowerPoint slide, it still resembled a slide with a title and four points elaborating that title. Richard’s verbal explanation mirrored the text on the slide.

I’m interested in physical activity and health and young people, which remains high on the agenda for public health. Young people have been targeted for an intervention and we know there is a big drop off, especially with [group X]. And it is an

interesting age to see what is happening.

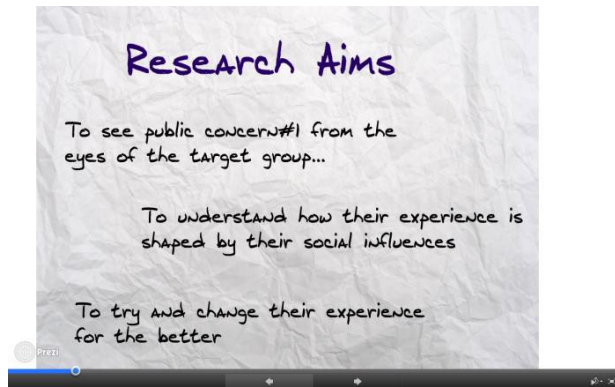


Image 13

Moving on to the next slide (see image 13) indicated the third step of the introduction: stating the research rationale. This slide also resembled a crumpled piece of paper with the title given on top "Research Aims" and three research aims underneath: understanding participants' ideas on and experiences with topic X; how these ideas were shaped by their social environment; and trying to change their experiences related to topic X. Again, Richard articulated what was on the slide:

The research aims are really to look at [X] through the eyes of the [participants], to see what sort of social discourses are kind of negotiated, what social structures impact on how they view the world, how they make [X] meaningful in their lives. And how that impacts on their behaviour. And secondly, for the second stage – but I'm not going to be talking about that too much today – was to try and change their experiences for the better. Not sure [though], because the research is still on-going at the moment.

Despite a more playful layout of the presentation than in PowerPoint, Richard still used a standard approach of filling up the slides (Bartram, n.d., p. 4). Further, the beginning of his presentation was one of those traditionally used introductions within academia (Rowley-Jolivet & Carter-Thomas, 2005, p. 46). Although he did not summarise academic literature, the overview of the presentation, the first two 'slides' and Richard's accompanying explanation made clear that his research was academic. He clearly delineated a research field within which he would contribute specific knowledge. Furthermore, the setting, the structure

of the presentation and his supervisors' names further framed his work as academic.

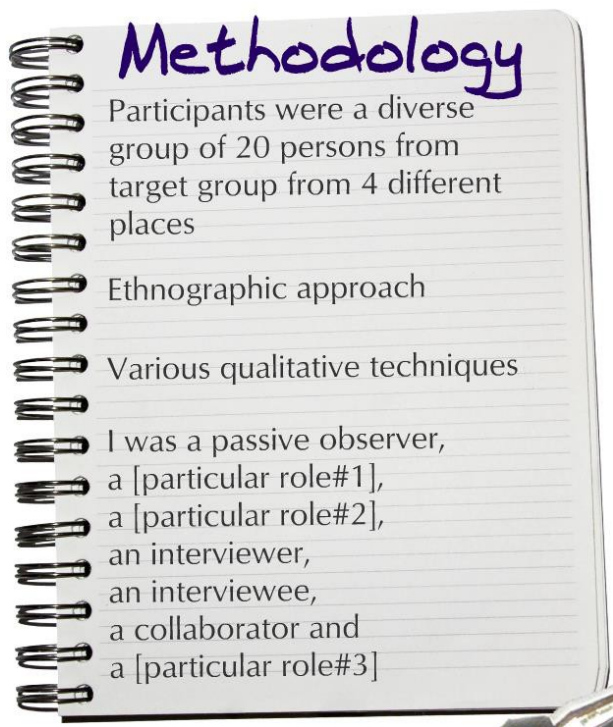


Image 14

The presentation of his methods was another way in which he embedded his work in academia. First, he did this through content of the slide, which showed a page in a notebook with “Methodology” written at the top of the page (see image 14). Underneath the title were four sections, from top to bottom: a description of his participants; “Ethnographic approach”; “Various qualitative techniques”; and a description of the various roles he had had in the research. Richard’s verbal discussion of his methods framed his work even more clearly as academic by referring to correct ways of doing research (Bryman, 2008; Seale, 1999). For example, he started out talking about the different field sites he visited and the diversity amongst his participants in their attitudes towards the research topic and demographics, such as gender, ethnicity and socio-economic status. Showcasing this diversity, he indicated he was working with a representative group of people (Field, 2009, pp. 34-35). His discussion of how he had applied the ethnographic approach was, again, filled with references to what he considered correct ways of doing research (Bryman, 2008; Seale, 1999). He indicated, for example, how long he spent in the field (about six to seven months) and how often he was there. Richard continued, saying he interviewed the students every week, thus having a longitudinal and in-depth approach, which meant he was getting rich data. Prolonged engagement and persistent observation were a way to establish what Guba and Lincoln (1989, pp. 236-241) call credibility of the research, one of the criteria with which to assess

research. Richard also spoke about the relationship with his participants, whereby he tried to break down the hierarchy between them and himself. This showed he was sensitive to ethical issues, but also to how knowledge was produced (Bryman, 2008, p. xxix; Seale, 1999, pp. 189-192). Richard thus demonstrated he had done correct and thorough research. At this point in the presentation, the audience did not know yet that Richard had used visual methods because he had only mentioned doing participant observation.

Rather than explaining the methods and reflecting on them, he demonstrated that he had done unbiased, representative and therefore good research. As such, he did not treat any of his methods as different or meriting special attention, but rather provided the audience with the same criteria as other qualitative and even quantitative methods on which to judge his research (Guba & Lincoln, 1989, pp. 233-250; Seale, 1999, pp. 140-148). This introduction framed the ensuing discussion of his results and the use of creative methods as academic. That this was the beginning of his analysis was emphasised in a variety of ways.



Image 15

For instance, the slide showed a magnifying glass within which several sticky notes and three Polaroid photo frames were placed (see image 15). At the top of the magnifying glass was the title “Emerging Themes”. These were all symbols of doing research: a magnifying glass to take a closer look at something and the sticky notes to write ideas on. Within the magnifying glass, the emerging themes were represented by the sticky notes and Polaroids. Each sticky note represented a theme and contained short descriptions pertaining to the data, namely how to interpret the data. The Polaroids displayed the data: the photographs, video and interviews. In his verbal introduction of the material, Richard emphasised the preliminary nature of his results, stating:

So, themes are very much emerging at the moment. I haven't done any thorough analysis on them yet. [I look forward to] getting your feedback today. There are

about five themes that start to make sense to me and I'll talk through them quickly after I show you a couple of clips to start with.

Interestingly, Richard talked about his data and these clips as something that just came to him, as something emerging. However, the clips he showed were compiled by him and thus put together in a certain way. They were not data found 'out there'. Nonetheless, they felt unprocessed, because he had not yet organised them according to these five themes. He showed all three clips consecutively and used the original media to present the material.

The Prezi zoomed in on one of the Polaroids to show the first clip. It was a video collage Richard had put together of photographs his participants had taken. The video showed each photograph for about four seconds before it shifted to the next one. Richard introduced the clip by explaining that his participants: "were given the task of taking photographs of [topic X], and these are some of the things that they came up with." As such, the photographs seemed to show the audience directly what the participants had produced, rather than Richard's analysis of what they had photographed. However, Richard's running commentary, which contextualised the photographs or gave a description of what the photograph represented, steered the interpretation of the photographs in a certain direction. Of course, this was not a full analysis, but the configuration of these different modes still led the audience to see the images in the frame Richard provided with his verbal contextualisation.

This was also the case for the second and third clips. The second was a compilation by Richard of different video footage his participants had filmed. In the third clip, he played segments of the different group interviews he had done. Again, Richard had made decisions about what to show or play in these clips, but in both cases also added information. For the second clip, Richard verbally added information that could not be conveyed by the videos made by the participants, for instance, how a particular activity seen in the clip was mostly done by a particular subgroup of his participants. For the third clip, Richard had transcribed what he considered the key quotes. These appeared on the screen at the same time that they were uttered by the participants as a way to emphasise them. The use of the written mode helped indicate which segments in the interviews needed extra attention. Both these examples show how Richard steered the interpretation by his selection of materials and adding extra emphasis.

After playing the three clips, Richard presented his interpretation of the material. On

the screen were five sticky notes, each with a theme as a heading on top and underneath those came different examples and descriptions of the theme. Because his analysis was still in the early stages, his discussion of the emerging themes remained very descriptive and basically mirrored what he had written on the slide.

The three clips and Richard's explanation of them, both verbally and on the slides, worked together in conveying his research (Jewitt, 2013; Kress, 2010). Because Richard and his participants had been working with audio, photography and video, he had these modes at his disposal to show his research in audio and audio-visual clips. Then there were different ways in which Richard pointed out which elements in the clips should be paid attention to: first, verbally in his running commentary during the first two clips and his explanation afterwards; second, in a written manner, in the pieces of transcription coming up during the audio-clip of the group interviews and in the sticky notes on the screen after the three clips. The descriptions and examples on the sticky notes were those elements he had noticed in his material, which he further explained in his interpretation of the material. They worked as evidence for the emerging themes just as quotes from an interview would. As such, they had an exemplary function. Even though the interpretation was not fully fledged yet, with the visual object in Prezi, the written text, audio-visual recordings and verbal explanation of the material, Richard put together the equivalent of the academic paragraph – an argument at the beginning, examples in the middle and an explanation at the end – with each mode conveying part of the research (Baram, n.d., p. 4; Jewitt, 2013; Kress, 2010)

Like Louise's and Liza's presentations, Richard's showed how the visual material was firmly put into an academic context. He started his presentation setting up the framework, contextualising the topic and stating the research rationale (Rowley-Jolivet & Carter-Thomas, 2005, p. 46). The build-up of the presentation thus started with an academic context from which the audience could start to interpret its meaning. This continued on in his explanation of his methods, where he made clear how he had done correct and thorough unbiased research by describing the diversity within his group of participants and his prolonged, diverse and in-depth interaction with them. In the presentation of the audio, visual and audio-visual data, the different modes (Jewitt, 2013; Kress, 2010) worked together to convey his research as academic. The verbal and written explanations anchored the interpretation of the audio-visual materials. Simultaneously, Richard's use of creative methods added modes to use in the presentation which were able to convey the experiences of the participants visually. Nonetheless, these too were embedded in meaningful patterns of academic presentations.

Hilde

In her presentation, Hilde introduced and showed the documentary she produced with twelve participants as part of her PhD research. The documentary focused on a geopolitical issue in the lives of the participants. Hilde's presentation differed from the three previous presentations on two points. One was that she used neither the structure of one of the introductions I outlined above nor the typical structure for a presentation. Second, her configuration of the various modes was different, with PowerPoint playing a supporting, contextualising role for the documentary instead of the documentary supporting the arguments made in PowerPoint. Yet, there were also points of similarity. For instance, Hilde made use of academic practices to convey her research such as references to academia, the configuration of modes within the presentation, the contextualisation of the research and the process of making the documentary.

She had told me in an interview that she and her participants tried to show the documentary as widely as possible. Her research was as much about the process of producing the documentary as it was about the documentary itself and getting it out into the world, so people would learn about the situation Hilde's participants were in. For this event, Hilde had been invited to screen the documentary and talk about the process of producing it. The invitation came from a social sciences-based research group specifically interested in the topic of her research. Later on, another research group, which was mainly interested in her research methods, joined in organising the event. As this event was initiated specifically to screen her documentary, there was no call for papers or peer review, but there were still expectations and negotiations in the run-up to the event. For instance, it was assumed by all parties that Hilde would introduce the documentary and that the PowerPoint presentation she had made was peer-reviewed by the one of the research groups. Although she did not change the content of the slides, she made small adjustments to structure, such as adding some explanations so the audience could better understand the context. Moreover, the setting in which the documentary was going to be shown was academic as opposed to cultural, unlike the atmosphere at a documentary film festival. The event was advertised by the two research groups, using their mailing lists to inform their members, and posters had been put up at the research groups' departments, which meant the audience was going to be academic, most likely from the social sciences.

The screening was in a lecture room at a university, where chairs had been arranged

into rows. There was a dropdown screen and a table to the side which was used as a lectern. An audience of about 25 people attended. The event consisted of two main parts: the introductory presentation and the documentary. While the members of the audience were entering the first slide of Hilde's presentation was already showing (see image 16).

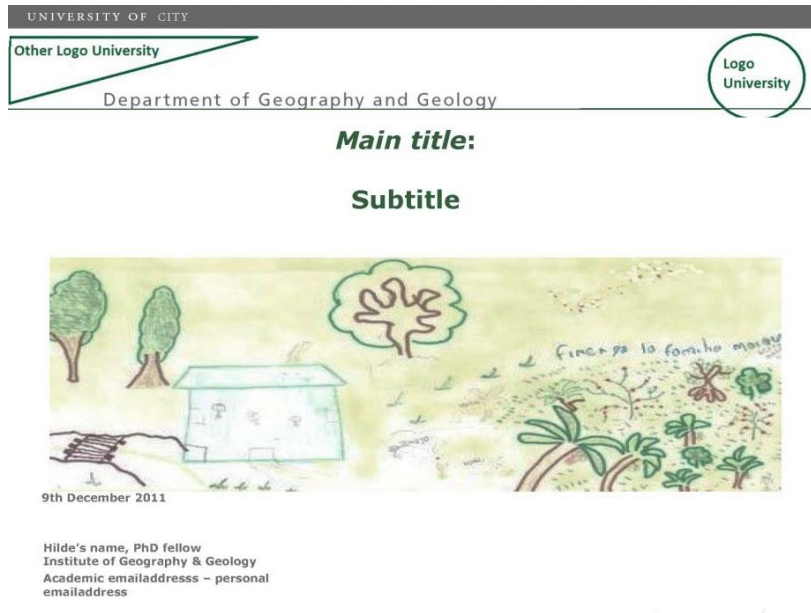


Image 16

The title slide was slightly different from the content slides, but they all used her department's PowerPoint template, showing the university logo and name as well as her department. The title slide also gave her name, her academic affiliation as a PhD fellow and her e-mail address, which included her university's domain name. These were all signs of academia. In addition, the slide showed the title of the presentation and a drawing of a house, trees and a field. Hilde did not explain the drawing, but it made sense in the context of a participatory action research approach and the region where she had done her research. A member of the organising committee introduced Hilde, gave her academic affiliation and stated that she had produced a documentary in collaboration with her participants for her PhD. This setting and the verbal introduction placed the event and Hilde's work within an academic context.

The whole of Hilde's PowerPoint presentation was an introduction to the documentary, which did not use the rhetorical moves of one of the introductions I described above (Hyland, 1998a, p. 26; Rowley-Jolivet & Carter-Thomas, 2005; Swales, 1990). Except for the first content slide in which Hilde gave an overview of the presentation, there was no similarity to the other introductions. Rather, Hilde provided the context in which the research was done and thus gave no overview of the field, previous research or a gap in the literature.

With the title slide showing, Hilde started her presentation, stating: “Thank you, everyone, for coming. We are going to talk first about this documentary that was produced in [place X]. And the presentation has these different parts.” Projected behind her was the first content slide which gave an overview of the structure of the presentation (see image 17).



Image 17

I'm going to start by making a geographical context of the place where the documentary was produced. Then I'm going to talk about the [geopolitical issue] that is taking place in this local territory. And then I'm going to talk about what the technological accessibility is for these local young people and what the [social and political situation] is in this place. Finally, I'm going to talk about the stages of producing this documentary.

Clearly, the PowerPoint presentation did not adhere to the template of introduction, theory and research question, methods, results and conclusion (Dodovski & Petreski, 2012; Golash-Boza, 2011; Wiertzema & Jansen, 2004, p. 50). Nonetheless, Hilde contextualised the documentary, thus managing the expectations of the audience. For instance, the contextualisation of the environmental and political context clarified the societal relevance for her research as well as the specific circumstances in which Hilde and her participants had to negotiate their research. This enabled them to understand better what the documentary was about and how it was produced. Hilde spent quite some time on discussing this context being a departure from an everyday Western experience. In this way, the PowerPoint played

a supporting role to the documentary.

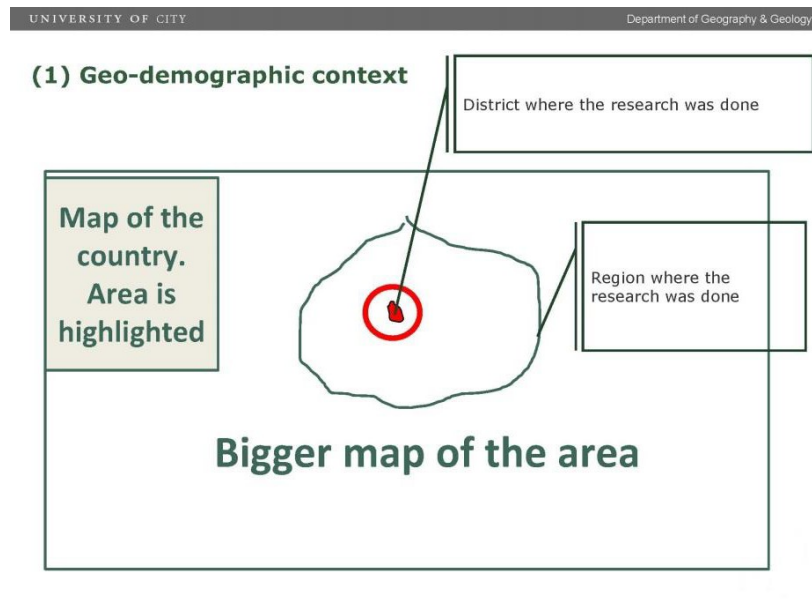


Image 18

In her introduction, Hilde used various modes to elaborate on her participants, the area they lived in and the issues that they faced on a daily basis. Behind her on the slide (see image 18) were two maps. On a smaller map of the country in which Hilde had done her research, the region where she had done her research was highlighted. Another bigger map showed the area in more detail. Additionally, the slide contained text, giving information about the area's demographics. While Hilde read this information from the slide to the audience, she also pointed to the map, indicating where her research was done. This gesture emphasised that this was a specific place on the map, a specific place where she did her research. This slide clearly showed the modes worked together to add specific information (Jewitt, 2013; Kress, 2010).

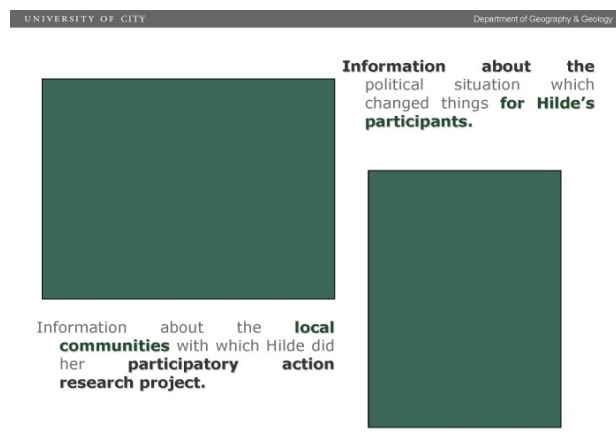


Image 19

This was similar for the ensuing slides in which written information explained the area's

geopolitical issue, access to technology and her participants' technological knowledge, the social and political situation in the area and the stages of the documentary's production. The slides each had a title at the top with several bullet points or a definition underneath. Hilde emphasised the importance of certain words in those bullet points or definitions by using a different colour and bold font. For instance, the local issue was connected to the stakeholders' activity (see image 20).

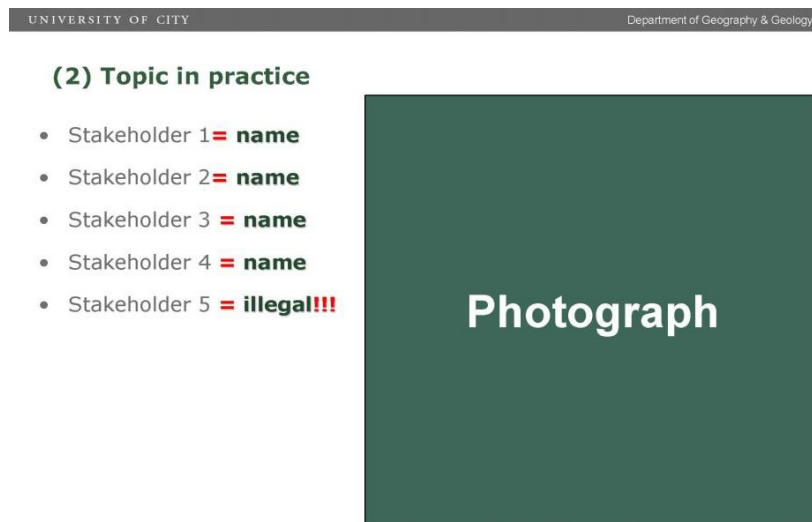


Image 20

Hilde would mirror this information in her verbal explanation, but each slide also contained photographs she had produced during the research. These conveyed other aspects of the research – for example, showing what it was like in that area and what the inhabitants looked like – which would have been difficult for Hilde to describe. For instance, one of the last slides showed a photograph of a few of the participants and read: “**PDP** [Participatory Documentary Process] was an **active** and **creative** way to **Oppose, Mobilize & Contribute!**” Here, too, Hilde made use of bold font and the colour red for emphasis. Hilde also verbally explained why such an approach was valuable and appropriate in this context while still connecting it to the societal relevance it had had for her participants. In addition, she explained that for the research, this approach would provide her with knowledge about the experiences of her participants and their ways of dealing with the issues. Despite not adhering to conventional structures of the introduction or a typical academic presentation, the presentation did provide a context within which the documentary was understood as academic.

After the final slide, which thanked all the people who collaborated on the project,

the documentary was shown. About an hour long, the documentary showed interviews with local people as well as footage from activities relating to the local issue. Clearly, the documentary used a different configuration of modes, where the emphasis was on the audio-visual, showing directly what the issue involved and being explained by the people dealing with it, rather than described by the researcher. In addition, the documentary itself was the main part of the presentation, it was the research instead of an example supporting the arguments made in the PowerPoint presentation. Yet, it did not refer to gathering or analysis of data, nor did the documentary provide the viewers with a conclusion which referred back to theoretical concepts or a research question.

In conclusion, Hilde embedded her work within academic practices, but also improvised on them. Her presentation, for example, provided an introduction to the documentary framing its interpretation. The introduction contained information about Hilde's participants and the production process of the documentary. It also employed an often used layout for slides (Bartram, n.d., p. 4) with bold and coloured font for emphasis. Plus, Hilde would verbally repeat what was on the slides. This configuration of the verbal and written modes in the PowerPoint presentation mostly reiterated messages. The photographs, however, added a visual sense of the research. Here the photographs had a supporting role in conveying what the area where Hilde did her research looked like. By contrast, the documentary had a leading role in which the audio-visual was the most important and the PowerPoint presentation had a supporting role.

On the one hand, the documentary was embedded within an academic framework through the introduction and the aspects of the research addressed in it and the contextualisation of the project. On the other hand, there was no explicit research question or theoretical framework, no references to literature or concepts, no analysis or conclusion. Most importantly, a documentary as data and research is in itself an unconventional dissemination of research in the social sciences, apart from within the discipline of visual anthropology. It was possible for Hilde to improvise in this way, because the event was specifically organised for the presentation of her documentary. Another reason was that Hilde had a background in participatory action research and was involved in various communities of practices as discussed in Chapter Three's *Participants* section and Chapter Four's *Hilde: Negotiating Methodological Approaches* section. This made her passionate to conduct and disseminate her research in this way.

So while some elements of the presentation re-inscribed academic practices relating to presentations, others, like the documentary itself, created new interpretations of how

presentations could be done. Still, by embedding the documentary within an academic framework, it became perceived and accepted as such. Hilde's presentation is thus a combination of both reified and improvised elements.

Paul

In his research, Paul used an ethnographic approach, doing participant observation, engaging in the same activities as his participants and interviewing them a number of times. He filmed the interviews as well as some of the activities going on at his research sites. In his presentation, Paul focused on the performative aspects of his participants' athletic practices. His presentation was a multimedia performance, where a DVD with footage from the interviews and activities at his field site played alongside and interacted with his performance. Whereas each presentation made use of various modes with which to convey the research, Paul's configuration of them was different to those of my other participants. In addition, Paul used his own body in a more performative way to convey meaning. Yet, his presentation also contained academic elements, which in fact resembled certain ethnographic styles of writing, as I argue below.

Paul was able to improvise in this way, because he had the skills and drive to pursue this way of doing research. In our interviews, he told me he thought a lot about doing presentations and that he was deliberately trying to change how presentations were done. Besides having a social sciences background, Paul had a degree in theatre studies and had done performances before. During his PhD, he had become aware of performative methods being used in the social sciences as a way of gathering data, but more importantly, as a way of disseminating research. At one of his first presentations as a postgraduate research student, he had been encouraged by a professor who also worked with performative methods on similar topics to pursue this line of research dissemination.

Paul's presentation was at a postgraduate conference supported by the British Sociological Association (BSA) and was organised by postgraduate research students from a sociology department at an English university. The call for papers asked for a specific topic (e.g. disability), which was regularly researched and presented in a performative way. The call said: "We welcome traditional papers, preformed [sic] panels of papers, workshop proposals and other forms of performance – recognising that different disciplines express themselves in different mediums. Submissions are sought on any aspect of [topic X], including the following (field notes, call for papers):..." The "following" consisted of

subcategories of topic X, but nothing more specific on the kind of methods they welcomed. In the programme there was a workshop planned on “Research, [topic X] and ‘New’ Methodologies”, but the professor leading the workshop could not attend and it was cancelled on the day itself. Regardless, these were indications that the conference organisers were open to other forms of presenting⁴⁴ and, in fact, there were others at the conference who used performative elements. One presenter, for example, asked the audience to engage in a physical exercise to experience the topic of his research. This context provided a space where presenters were encouraged to improvise on the traditional meaningful patterns such as the overall structure of the presentation or the rhetorical moves of the introduction.

Indeed, Paul did not use any of the rhetorical moves for the introduction outlined above (Hyland, 1998a, p. 26; Rowley-Jolivet & Carter-Thomas, 2005; Swales, 1990). Neither did he use the traditional overall structure of a presentation (Dodovski & Petreski, 2012, p. 11; Golash-Boza, 2011; Hyland, 1998a; Shalom, 1993; Wiertzema & Jansen, 2004, p. 50). However, Paul’s presentation did resemble another form of dissemination: the written ethnography. For example, he started his presentation with what Geertz (1973) referred to as a detailed “thick description” of the scene when he entered the field. His rendering of the site was sensorial; the audience could almost see, hear, smell and feel the place through the details in his verbal description of the site. He also used his body to do this. Paul imitated the voice of the gatekeeper (Walsh, 2000, pp. 224-225), the man who granted him access to the site. This was followed by Paul physically showing the audience an example of what happened at this field site through a dramatic re-enactment of his field notes. His body moved around, imitating the movements of one of his participants. His speech changed, too, and took on the accent and demure voice of the participant. This vivid way to demonstrate his work was enhanced by the interaction with another mode, the DVD. Paul had timed his speech and actions to it and, at given times, the DVD took over, showing footage from the field site and clips of the interviews with some of his participants. Paul did not just reconfigure the modes, but applied them differently. Paul’s performance and the DVD were not standalone modes, but they interacted with one another. His body and gestures had leading rather than supporting roles, such as indicating something on the screen or emphasising certain words.

This opening was reminiscent of how a monograph starts, which is another example of the marking of the fieldwork experience (Marcus & Cushman, 1982, p. 33), of showing

⁴⁴ Paul had other experiences, too. He had told me in an interview how once he had responded to a call for papers explicitly for performative presentations. He had been accepted, but when he contacted the organisers about some of things he needed for his presentation, they were very apprehensive and difficult.

the researcher's actual presence in the field (Marcus & Cushman, 1982, p. 33; O'Reilly, 2005, pp. 213-215) and concomitantly claiming authority (Bryman, 2008, p. 685). By adopting a well-known form of narration within the anthropological discourse, but transferring it onto another mode of dissemination within a different disciplinary setting, Paul improvised with the frameworks available to him. As such, he negotiated changes to what was considered a conference presentation. The physical re-enactment of his field notes and the interaction with the DVD extended the ways in which knowledge was conveyed. His presentation did not so much address the cognitive, but much more the sensorial and emotional capacity of the audience, allowing them another engagement with his research.

There were other elements within his presentation resembling the ethnographic monograph. Arguably, the dramatic re-enactment and clips were also comparable to descriptions of events from field notes or block quotes from an interview. Paul would subsequently interpret and connect them to the literature through naming a concept and its author, whilst the DVD would show the concept or give references to the literature Paul was citing. This worked as an aid for emphasis. This interaction thus placed the performance within the academic practice of situating his work in the theoretical debates and certain traditions of thinking of his field (Gilbert, 1977; Hyland, 2004).

Another academic practice Paul applied concerned anonymity. Just before he started his presentation, he gave a caveat about the clear visibility of his participants in the DVD. He explained his rationale for why they were not anonymised. This caveat did two things. In giving a rationale for his participants' visibility he was simultaneously engaging in other practices – namely, arguing certain choices, explaining his lines of thought and engaging in debate (Bryman, 2008, pp. xxix, 66-76, 664; Seale, 1999, pp. 189-191). The caveat also placed his presentation within the ethical discussion concerning visual methods and anonymity (Beddall-Hill, 2011; Boxall & Ralph, 2009; Prosser, Clark & Wiles, 2008; Wiles, Coffey, Robison & Heath, 2010; Wiles, Coffey, Robison & Prosser, 2012). As Paul told me later in an interview, he hoped social scientists might be critical about his choice, but at the time no one had yet spoken out about it (see for an extensive discussion Chapter Five's *Paul: Negotiating Different Disciplinary Reifications* section).

In some ways, Paul's presentation was like a performance in a theatre, re-enacting scenes from the field, mimicking his participants' voices and postures. At the same time, it contained well-known rhetorical moves from ethnographic literature and other elements of academic practice. The marking of the fieldwork experience, the description of field notes and quotes from interviews as evidence, the connection to the literature, his analysis of

events in his fieldwork and the disclaimer about his participants' anonymity were all elements of academic practices. These elements made sense within an academic context and simultaneously made Paul's presentation acceptable and understandable as research. His use of creative methods also affected his interpretation of the modes on two closely related levels. The first concerned the way he employed the individual modes. This meant, for instance, that he very deliberately used his body, voice and gestures to convey a scene from his research in addition to a sensorial description and that he showed video clips from the interviews rather than transcriptions. This interpretation of the modes also changed their configuration – the second level – giving those modes a more prominent role in the presentation, which in this case led to more interaction between the modes.

Conclusion

In this chapter, I have shown how the postgraduate research participants presented creative methods both reinforced and changed the ways research was presented. In this process, they negotiated the creative methods into their own practices, in which concomitantly the creative methods emerged. This then leads to creative methods becoming part of accepted academic practices and sedimenting formats for presenting creative methods (Holland, Lachicotte, Skinner & Cain, 1998, pp. 17-18).

My participants needed to make improvisations in the presentations to accommodate the use of photography, film, poetry, performative and narrative elements. Those improvisations were the results of my participants' negotiations with the reifications of the different communities of practice they were engaged with. At the same time, they presented the creative methods in ways compatible with academic presentation practices.

All the presentations of the postgraduate research participants I attended were given at events organised in an academic context. They were held by social science departments and often supported by organisations or associations promoting social research. This obviously meant my participants envisioned their presentation in such an environment and focused on the research as academic.

All participants embedded their use of creative methods in the presentations within an academic context. They did this in various ways. For instance, they used various modes to reference academia, such as academic titles, the affiliations of the postgraduate research students and both verbal as in-text references to academic literature. Using the rhetorical moves for the introduction, such as the CARS model (Hyland, 1998a, p. 26; Rowley-Jolivet

& Carter-Thomas, 2005; Swales, 1990) or the traditional overall structure for presentation (Dodovski & Petreski, 2012, p. 11; Golash-Boza, 2011; Hyland, 1998a; Shalom, 1993; Ventola, 2002, pp. 29-30; Wiertzema & Jansen, 2004, p. 50) was another way to provide meaningful patterns (Hyland, 1998b). Either structure contextualised the research in societal relevance, social scientific theories or the circumstances in which the research was done. These elements then worked as a framework with which to interpret the data that was presented. Louise followed this structure to the letter, whereas Liza and Richard made small improvisations on the structure of the introduction and the whole presentation. Hilde improvised more extensively from these structures. She did not use a typical structure for the introduction or presentation. Neither did she present her audience with an analysis of her documentary. Yet, the presentation worked as a way to contextualise her documentary because she described the geopolitical issue, her participants and their technical background and skills as well as gave an outline of the production process. The presentation provided a framework with which to understand the documentary. Paul also refrained from using either structure. He improvised on another academic practice, the ethnographic monograph, and used rhetorical strategies such as thick description, adopting it from the written to the bodily and verbal mode.

This showed how the postgraduate research participants used reified academic practices to present their work. The use of creative methods inevitably required improvisation because the creative methods added to the modes available to use in the presentation. This was clear from all the presentations, as they all used visual and/or audio-visual material to convey their research. That changed not only the way the modes were put to use, but also their configuration with other modes in the presentation. In some cases, photographs, film and performance were used as illustrations. The images had a supporting role, rather than a leading one. They were incorporated “into an already established methodology (...) [in which images] must subscribe to the dominant discourse in order to be incorporated” (Pink, 2007, p. 5). In those instances, they worked as proof that the researcher had been there and had done the research. Or, they worked as proof for the analysis, serving as examples of the themes or discourses. Even so, in these cases the use of audio-visual modes changed the meaning that was conveyed, as these modes showed rather than indirectly described locations, activities or experiences. These were small changes to social science research practices. In other cases, the creative methods and thus the audio-visual modes played a more prominent role. Hilde’s documentary was the main part of the presentation for which the PowerPoint presentation played a supporting role in giving the

documentary a context. Hilde's research was conveyed in the documentary, not in the PowerPoint presentation. In Paul's presentation, his body and gestures, the DVD and his verbal sensorial description interacted with one another to convey his research, but none had the sole main role.

To be able to use the modes in this way, the context of the event needed to allow for using the modes in this way. Liza presented at a mainly quantitative conference, whereas the events where Louise and Paul presented explicitly invited other forms of presentation. The presentations of Richard and Hilde then again were invited to present their research, which was nonetheless shaped by the context of the department in which they gave their presentations.

More importantly, the researchers themselves needed to possess or learn the skills to present creative methods differently. Using creative methods came with having to learn a whole set of new skills to be able to implement them at the different stages of gathering the data and the dissemination of the research. Paul, for instance, had learnt the skills to do his performance in his undergraduate degree. Hilde worked with a team of people who knew how to make a documentary. The documentary in Liza's research was made entirely by her participant. She told me she was 'merely' the one who held the camera. From these three examples it is clear that having such skills, or having access to them, made it easier to use creative methods.

The use of creative methods in these presentations therefore leads to the conclusion that labelling such methods as 'innovative' (Pain, 2009; Taylor & Coffey, 2008; Wiles, Pain & Crow, 2010; Xenitidou & Gilbert, 2009, pp. 60-65) is not useful. It became clear from the way my participants used them in their presentations that the creative methods were always embedded within academic practices. Rather than radically departing from those practices, my participants improvised on them to fit their presentations. Those small changes were the inevitable improvisations (Ingold & Hallam, 2007; Wenger, 1998) researchers must make to accommodate each new research project, but which are always connected to earlier practices.

Chapter Seven: Conclusion

Introduction

Although this chapter is called the conclusion, it should be obvious by now that this chapter is part of an on-going discussion, an on-going negotiation of what practices of social scientists look like. You, as a reader, might contest my conclusions and your criticisms would further these negotiations. Or you might agree and, through your affirmations (Gilbert, 1977) help reify these viewpoints, similarly to how references support research. Most likely your response falls somewhere in between. To open up this discussion, I now draw my conclusions from this work. I start by going back to the research questions and the previous chapters. I summarise each chapter and pay special attention to the way the chapters answer my research questions and how this relates to the theories I have drawn upon. I then continue to discuss the wider implications of my findings. All the while, it is important for the reader to keep in mind the way that I understand creative methods – as discussed in Chapter One – and my own personal background – as discussed in Chapter Three – in order to realise there are inextricably limitations to this social scientific research, even though it is never possible to be aware of all of those limitations and how they affected the research (Rose, 1997).

Summary of the Chapters

Despite increased attention to and calls for creative methods, there has been less attention to their adaptation in actual practice. This is not always an easy process as it involves more than receiving factual knowledge (Lave & Wenger, 1991). At the same time, there is a lot of demand for these methods and for knowledge about them (NCRM 2011; Taylor & Coffey, 2008, p. 7; Wiles, Bardsley & Powell, 2009, pp. 9-10). My research topic was thus about creative methods and the way they were taken up. For the research, I had one main question and three sub-questions. The main question guiding my inquiry was:

How are creative methods (specifically, visual lens-based arts, poetry, performance and narrative) negotiated in social scientific research practice?

And my sub-questions were:

- *How do social scientists negotiate the use of creative methods with other members of their research communities?*

- *How do social scientists negotiate the use of creative methods into their own research practices?*
- *And how do creative methods emerge in the process?*

To address these questions, I laid out my theoretical framework in Chapter Two, *Practice, Improvisation, and Innovation*. I drew on the concepts of communities of practice (and the accompanying concepts of reifications, negotiation and participation), boundary-work, power, improvisation, innovation and the way research is ‘supposed to be done’. These concepts provided me with a framework with which to look at the ways in which my participants applied their creative methods. This theoretical framework offered an understanding of the practice of research as ever emerging and on-going, improvisational and relational. I argued that practices are always in relation to earlier reifications and, thus, that change is incremental rather than revolutionary. This meant that terms such as ‘innovation’ or ‘innovative’ were inappropriate to apply to creative methods. Furthermore, I emphasised that researchers need to pay attention to everyday research practices because change is constantly occurring, and slow but profound shifts in epistemologies, methods and knowledge production can otherwise be overlooked.

In Chapter Three Methods, I started with a discussion of reflexivity. After discussing its development and some of its pitfalls, I argued that reflexivity was a rational, relational, dialogical and emotional process (Burkitt, 2012). In addition, being reflexive was not about making the research more objective or better, but rather to further explain the process of research production. One aspect of that was my own academic background, which I described in order to contextualise how I approached my own and my participants’ methods. I then discussed the ethnographic approach I took, whereby the construction of my field and the relationship with my participants were important. In addition, I introduced the individual participants. As my main focus were the research practices of academics, I worked with two groups of participants: experienced academics and postgraduate research students. The diversity of the participants – in the creative methods they used, the stage of their careers and their institutional affiliation – enabled me to see creative methods in development from various perspectives. Next, I discussed the variety of methods I used, giving me an in-depth understanding of the way my participants had engaged with creative methods – in this study, specifically visual lens-based arts, poetry, performance and narrative methods.

First, through in-depth interviews, I gained knowledge about how my participants learnt about creative methods, the way they had applied them and if they had experienced any

struggles in the application. The on-going interviews with the postgraduate research participants, because of their longitudinal nature, clearly showed this process of learning and negotiating the methods. Second, the filming of the postgraduate research participants' presentations enabled me to look in detail at a presentation's content, design, sequence, words and the setting in which it was held. Finally, I did participant observation, which was not always easy, and forced me to negotiate my own practices and reifications. This process of negotiation continued in my transcription of the interviews and presentations as well as my analysis of them. Initially, I reverted to forms of transcription and analysis that I had used before, but they were not effective, because my data differed from what I gathered in the past. They thus required negotiation on my part, which consisted of reflecting on my personal practices, reading literature on multimodality (Jewitt, 2013; Kress, 2010) and engaging with the materials I had. After the analysis, I discussed the ethics of my research and elaborated on a specific conversation I had with Richard, in which power and knowledge were negotiated. This was also one example in which my participants and I co-constructed knowledge, and throughout the chapter, I gave more examples of such situations. In a final section, I discussed some ways in which the participants share in co-constructing the knowledge can be acknowledged.

In Chapter Four, *Making, Breaking and Negotiating Boundaries*, I started with an explanation of the concept of boundary-work and how academic practices are full of opportunities for boundary-work to be performed. I then briefly discussed experiences of slight tension surrounding my participants' creative methods, which was followed by the more detailed description and analysis of four examples of boundary-work (Gieryn, 1983). Each example showed boundary-work done in a different way by different people. The examples showed how power was present in these negotiations, but also how it was subverted. Power was thus not an attribute or resource, but something that affected the way my participants and their peers tried to influence their use of creative methods. All performances of boundary-work are done on the basis of reifications on how research is supposed to be done. The examples of Hilde, Bart and William and Mary showed how boundary-work ultimately opened up ways for creative methods to emerge and develop. Hilde's participatory approach of visual methods and Bart and William's collaborative performative writing both created precedents: the publications coming out of the collaboration between Hilde's participatory approach, on the one hand, and her Western European, more "pure and orthodox" supervisors, on the other; and the acceptance of Bart and William's collaborative performative thesis. Mary's bibliography and her chapter on peer review

showed how she embedded poetic inquiry more firmly into the academic practices of peer review and referencing, thereby providing others with tools to perform their own boundary-work. In addition, it showed how she was well aware of academic practices and used them to her advantage. These examples, furthermore, showed a variety of power relations and how change occurs because of perceived power and its resources (Allen, 2003). Sophie's example seems to be more ambiguous, as she dismissed certain usage of visual methods and performed boundary-work within her own community of practice of visual methods. At the same time, it is arguable that she was safeguarding visual methods for more severe boundary-work. Moreover, she was actively developing the criteria with which to judge these methods and thus pushing the development of the methods forwards. As a member of various communities of practice, in both her positions as professor and teacher, Sophie's participation in these communities necessarily saw her negotiate her interpretations of these criteria with others of the community, thereby pushing the emergence of creative methods.

The processes in these examples thus answer the first sub-question and start to provide an answer to the third sub-question. As for the first sub-question: social scientists negotiate the use of creative methods with other members of their research communities in the following ways: through making little shifts or improvisations in the research approach (Hilde), which are subsequently negotiated with their peers; through negotiating different interpretations of the guidelines (Bart and William); through embedding them within academic practices (Mary, Sophie). My participants also negotiated the use of creative methods when they experienced minor disdain towards the creative methods. They did so through avoiding potential exclusion of their research (Adrienne); or through specifically pushing their work into other (potentially unwelcoming) communities of practice (Bart). What further became clear is that the negotiations were never between vastly different standpoints, and that improvisations were possible. Finally, the examples in this chapter showed how the creative methods my participants used and the way they used them were connected to the ways research was 'supposed to be done', as described in Chapter Two (for example, referencing, being critical). This pointed to the development of these methods as an incremental and connective process of change, rather than one of radical breaks of innovation.

Chapter Five, *Creative Methods and Negotiating Personal Practices and Reifications*, dealt with the personal struggles some of my participants had in negotiating the creative methods for use in their own practices and provided answers to my second and third sub-questions. Liza, Richard, Louise and Paul all struggled in their own ways with aligning the theoretical expectations, their personal practices and conducting the research in practice. Liza

struggled with the collecting of data and the participatory and ethical aspect of her use of photography. Richard similarly struggled with the collection of his data and the instructions he gave his participants, but differed in the sense that he was disappointed by the data he received and what it showed him. Louise had difficulties in finding an appropriate method of analysis for her photographs. Finally, Paul was trying to find a way to combine both of his disciplinary backgrounds in drama and sports studies. The struggles of my participants emerged from the discrepancies they experienced between the reifications of their communities of practice and the actual application of the methods. This showed how they negotiated the methods into their practice, addressing my second sub-question. The creative methods were thus embedded within the practices with which my participants were already familiar though they had to improvise to make the methods applicable within that framework. Rather than adopting factual knowledge, they learnt through employing and connecting these methods (Lave & Wenger, 1991). They had to make little shifts in their theoretical and methodological convictions in order to be effective in their research methods. Observing how they did this also helps answer my third sub-question. On the one hand, the emergence of creative methods was restrained by my participants' backgrounds; on the other hand, they changed their old practices, which, in turn, determined the emergence of the creative methods. Creative methods thus emerged through embedding them into earlier practices.

In Chapter Six, *Creative Methods and Presentations*, I described the presentations of the postgraduate research participants. These presentations were examples of how my participants negotiated the reifications relating to research presentations. The presentations showed examples of improvisation as well as reaffirmations of the reifications. I showed this by using a multimodal approach (Jewitt, 2013; Kress, 2010) to discuss each presentation in detail, paying attention to the wider setting of the conference (the broad theme, the call for papers, the delegates), the setup of the room, the content and design of the screened presentation (the slides or film) and the spoken words of the presenter. I showed how Louise's, Richard's and Liza's presentations all used the more standard overall structure of presentations (see for instance introduction, theory and research question, methods, results, conclusion) (Dodovski & Petreski, 2012, p. 11; Golash-Boza, 2011; Hyland, 1998a; Shalom, 1993; Wiertzema & Jansen, 2004, p. 50) and rhetorical moves in the introduction (Hyland, 1998a; Rowley-Jolivet & Carter-Thomas, 2005; Swales, 1990) the creative methods they used were all embedded in and connected to academia. Their photography and film worked as examples of the themes that represented the data in a similar way that quotations would. In addition, the use of photography and film in the presentation was surrounded by signifiers of

proper research: references to the literature; names of supervisors; use of the rhetorical technique of ‘being there’ (O’Reilly, 2005, pp. 213-215) in order to claim ‘experiential authority’ to analyse the situation (Bryman, 2008, p. 685); use of qualitative and quantitative indicators of the diversity of the participants and thereby claims of representative data (Field, 2009). Nonetheless, the use of different modes to convey academia, as well as some of the photographs, documentary and recordings of the participants *showed*, rather than told, the audience the participants’ experiences. The presentations by Hilde and Paul also used elements of academic practice, but in a different way. Hilde introduced the documentary with an account of how it was made: the context in which it was produced, the methods she and her participants used and its societal relevance. Paul used an opening reminiscent of monographs in anthropology, where the ethnographer enters the field, thereby claiming his authority. In his description and analysis of the field, Paul connected his interpretation to earlier work in his field.

Nonetheless, my participants’ presentations moved away from the standard presentation in a number of ways. Hilde did not present her data in a systematic way or gave any analysis; the documentary was the data, analysis and conclusion in one. Paul did not use the standard structure, but rather presented a narrative which was interjected with re-enactments of his field notes, showing rather than telling his argument. In addition, there was an interaction between his performance and the moving images of the DVD. So, in all these presentations, my participants made connections with reified practices of doing presentations and representing research, in general, while at the same time improvising from those reifications to make their presentations effective. Chapter Six thus provided an answer to my second and third sub-questions. Similarly to the previous two chapters, it showed how the participants used and negotiated the methods in reference to the reifications of their respective communities of practice. Their use of creative methods forced them to make improvisations, but they nonetheless connected to established academic practices. This meant the use of creative methods only incrementally changed their practice. This chapter also reiterated that creative methods emerge from the improvisations the participants made in relation to those reifications.

Answering the Research Questions

In summarising the chapters of this thesis I indicated how each chapter fully or partially answered one or more of the sub-questions. In this section, I draw on these partial answers to

answer the sub-questions more elaborately and then answer the main research question.

First sub-question: How do social scientists negotiate the use of creative methods with other members of their research community?

The answer to this question is most concretely given in Chapter Four, where I discuss the performances of boundary-work. In those instances of the boundary-work, the negotiations between my participants and their peers are obvious because they disagree about their research methods. These are the rough negotiations. However, creative methods are also smoothly negotiated. To repeat a quote from Wenger (1998, p. 53), “even routine activities (...) involve the negotiation of meaning, [even though] it becomes much more apparent in situations that involve things we care about or that challenge us”. So relevant to this sub-question are both the challenging situations in which the participants were “shaken out of their comfortable framework” (Roe & Greenhough, 2014, p. 51), which was emphasised in Chapters Four and Five, and the “routine activities” (Wenger, 1998, p. 53) ‘shaped by habitual, learnt practices and techniques’ as described in Chapter Five.

The rough negotiations my participants had to do with others include the boundary-work by Hilde and by Bart and William. Both examples demonstrated the participants’ distinct reservations towards their research approaches: Hilde had to perform extra work to convince her supervisors; Bart and William’s supervisor negotiated on their behalf with the examinations office about the regulations of joint thesis submission. The rough negotiations also included those Liza and Richard had to make with their own reifications about how the relationship with the participants should be, what proper data is and how the creative methods could provide proper data. Although in Chapter Five the emphasis was on my participants’ negotiations with their own personal practices and not necessarily with others, which is the focus of my second sub-question, these negotiations were needed because in the application of their creative methods, Liza and Richard had rough negotiate with their participants. Liza, who wanted the research to be collaborative, found it difficult to engage her participants in such a collaborative way because they did not understand what she wanted from them. Likewise, Richard’s participants went off and did their own thing with the photographs and video, rather than engaging with the visual methods in the way Richard had hoped. This meant Liza and Richard had to adjust their engagement with them. In Liza’s case, it was obvious that her participants were eager to help achieve her goal. They wanted to do it right. This was less so the case for Richard. Even though he had worked hard not to have an

authoritative position towards them, he felt some of them produced photographs and videos primarily because he asked them to. It was clear from these examples that the negotiations happened within different communities of practice, which were not all academic. Liza and Richard showed how participants were also important in these negotiations.

The smooth negotiations were less the focus of these chapters, yet in the descriptions of the participants' backgrounds or stories of how they started working with creative methods, many smooth negotiations were done. For instance, Liza, said she read about the creative methods because one of her supervisors had advised her to and discussed the methods with a former PhD student of this supervisor who had used the methods as well. Similarly, in the preparation for her methods, she mentioned how she had asked her supervisors for specific advice on what kind of guidance to give her participants when doing the photo diaries, but they left her to find out for herself. The same went for Paul, who was hoping for criticism from others on the lack of anonymisation in his videos. Although Paul was expecting to have to defend this choice, his audiences actually enjoyed seeing the faces of the people he had worked with. Other examples of smooth negotiations include: the collaborations between Sophie and the photographers; the experience of Richard with his supervisors, who did not really comment on his methods, as they believed it to be some of his strongest work. These negotiations were very smooth because there was no opposition involved. Yet, they were nonetheless negotiations in which the acceptance of the research was just as important as it would be if boundary-work were involved. These rough and smooth negotiations provide insight into how creative methods emerge, which I return to when answering the third sub-question.

Generally, the rough negotiations occurred when different sets of reifications were used because different communities of practice were involved. Smooth negotiations occurred either when both parties adhered to the same set of reifications, or the sets of reifications aligned on the issue. It is important to realise that there are more communities of practice and different sets of reifications my participants engaged with, negotiated about and participated in. Other members of those communities have their own interpretations of these reifications and would correspondingly judge the work done with creative methods. To reiterate, these do not necessarily have to be academic communities of practice.

In both types of negotiations, my research showed how the participants made small improvisations from the reifications because the work was always embedded in earlier practices, connected to earlier research. This meant the practices developed through these negotiations, being relational to those earlier practices, but also to the critique or

encouragement of others and the negotiations themselves.

Second sub-question: How do social scientists negotiate the use of creative methods into their own research practices?

For this sub-question, Chapters Five and Six provided the most important material. Again, smooth and rough negotiations were determined by the practices and reifications my participants were familiar with. This means that the academic background and affiliations of the participants played a role in how creative methods were integrated, giving them the knowledge, skills and experience to do apply creative methods.

The smooth negotiations were barely mentioned because these practices were invisible and remained invisible since the creative methods did not challenge the reifications of these practices. The presentations described in Chapter Six are a good example of where the implementation of the creative methods made for little shifts in terms of how they were integrated into the already established structure of academic presentations. Louise, Richard and Liza embedded the methods seamlessly into the findings sections of their presentations, where they worked as substantiations ('proof', some might say) of the arguments.

Another example is the issue of anonymisation, which is heavily debated within the social sciences and specifically in research using visual methods (Beddall-Hill, 2011; Boxall & Ralph, 2009; Prosser, Clark & Wiles, 2008; Wiles, Coffey, Robison & Heath, 2010; Wiles, Coffey, Robison & Prosser, 2012). Louise, Richard and Hilde did not mention, let alone address, anonymisation in their presentations. They followed the practices from their respective communities, which meant Louise and Richard anonymised their participants. Hilde did not, as she was deeply involved with a community of practice adhering to a participatory action approach, which involved giving voice to participants.

Paul did engage in this debate and, in so doing, exemplified rough negotiations. Paul was part of two communities of practice with different practices of dealing with anonymity. He was actively negotiating his own practice on this point as he changed his opinions about how to deal with anonymity over the course of my conversations with him. Other examples of rough negotiations were the steps Richard took following disappointment with his data and Louise's struggle to find an appropriate method of analysis for her photographs.

These encounters with different practices and different communities of practice enabled or even forced change: in the practices of my participants, in creative methods, in standard ways of doing research. Again, however, this change was relational because the

changes were made in reference to the reifications of the participants' established practices. The creative methods were embedded within the practices my participants were already familiar with.

Third sub-question: How do creative methods emerge in the process?

The first two sub-questions gave a partial answer to the third. Indeed, it is through these negotiations that creative methods emerge and, as such, are determined by the practices of those who employ them. As I have shown, internal and external negotiations can be smooth or rough. This depends on how well the reifications of the different communities of practice align with one another, which determines the improvisations that need to be done. When they do not fit well together, boundary-work occurs, either by those who disapprove or support – for example, through providing tools for integration the way Mary did. But when they do fit well, negotiations may still take place through the mere application of creative methods. For instance, Liza, who was not the first to use these methods in her field, still enabled creative methods to emerge simply by using them and presenting them to her peers at conferences.

It also became clear that creative methods were continuously embedded within already established academic practices. Again, the presentations are a case in point, but the boundary-work of Mary and Sophie was also relevant here. Both of them made creative methods more academic by providing the tools for peer review. Mary used her knowledge of academic practices to enable creative methods.

From this, I again conclude that the emergence of creative methods is relational, but also dialogical, rational and emotional (Burkitt, 2012) because my participants used these creative methods in relation to the reifications they were familiar with. They used them in dialogue with others, such as their supervisors, fellow researchers and participants. Change in the use of creative methods thus happened through the meeting of different communities of practice, where negotiations took place, boundary-work sometimes occurred and researchers were actively engaged in trying to change practices or in fact using practices that seemed appropriate. By embedding creative methods in standard academic practices, they became part of academic practice. This was reflected in the inherent tension within research between connecting the work to earlier studies from the field and adding new knowledge to that field. This thus leads to the final and main research question.

Main research question: How are creative methods (specifically, visual lens-based arts, poetry, performance and narrative) negotiated in social scientific research practice?

There are two main conclusions I draw from the answers to my sub-questions. One is that the negotiations the participants did were always in relation to the reifications of their communities of practice (this meant in relation to the criticism of their peers, the theories and methodologies they used, the audience they addressed). The other is that creative methods emerged from those negotiations and, as such, their development was relational. Creative methods were thus relationally negotiated. This connects back to the argument Hammersley (2011, p. 10) made.

[I]t is essential to recognise that the process of producing academic knowledge is a collective one. I argue in this book that such knowledge is not discovered or constructed by individual researchers, each working on her or his own, but rather is generated through dialectical processes within research communities: through discussion, both oral and written, that is designed to come to conclusions about what is true, what is false and what is currently uncertain. The work of any individual researcher takes place against this background, and necessarily engages with it.

This collective character of enquiry places additional obligations on researchers, as regards how they present their work, how they respond to criticism and how they treat the work of colleagues.

As is clear from this quote as well as my research, the work researchers do is never independent, but always connected. It is a collaborative effort. Furthermore, researchers have a responsibility in engaging with the other members of their communities of practice. These engagements are not casual, but rather demand an engagement with the practices and the reifications on which they are based.

This realisation leads to another important conclusion. Change in practices is always incremental and the use of the creative methods can therefore never be innovative – it is improvisational. This means that calls for innovative research – and more specifically, innovative research methods – are problematic. If creative methods are truly innovative, they risk being dismissed for not adhering to proper research practice, as my research showed. In addition, calls for innovation put the emphasis on a false image of academic research. As I

have explained, there is an inherent tension between change and stability in academic practice, but the call for innovation forgets this, and portrays innovation as an unproblematic and desired goal. However, this thesis shows that this is not the case. Change and creativity within the social sciences are bounded by reifications, boundary-work and peer review. Nonetheless, these methods are ever emerging and evolving through improvisations and negotiations, which happen in small shifts. Still, these small shifts run the risk of going unnoticed. It is therefore important that researchers also attend to everyday research practices so as to see the slow and small but still profound changes over time in epistemologies, methods and knowledge production. Doing so would mean applying a reflexive approach which takes into consideration the rational, relational, dialogical and emotional aspects of doing research (Burkitt, 2012).

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Appendices

Appendix I Email Call for Participants

Hello everyone,

I am hoping that you could help me with finding participants for my research. I am looking for Ph.D. students or early career researchers within the social sciences (broadly/widely defined) who are working with one of the following arts-informed methods:

photography or video

poetry

performance

or fiction

They can be using these methods in their fieldwork or in their representation of their research. I would like to understand how these methods are actually applied, and therefore it is important that the research is still ongoing. Purely for practical reasons participants have to be based in the UK, as I am too.

The research would involve a few interviews over the course of the following months/year - obviously arranged at the participants' convenience. I would also be keen to observe data collection, analysis or dissemination of the research.

This is just a very short introduction to the research, but if you're interested and would like to know more, you can email me at P.J.C.-Van-Romondt-Vis@lboro.ac.uk

Obviously I would also appreciate it very much if you would forward the email to people who you think might be interested.

Thank you!

Pauline van Romondt Vis

Appendix II Overview Fieldwork

Experienced researchers

Sophie	7 December 2011	35 minutes ⁴⁵
Matthew	7 December 2011	55 minutes
Adrienne	31 January 2012	2 hours 35 minutes
Mary	29 February 2012	1 hour 30 minutes
William	1 March 2012	1 hour 10 minutes
Ally	19 March 2012	1 hour 45 minutes
Bart	29 March 2012	1 hour 45 minutes
Marjory	16 April 2012	1 hour 10 minutes

Postgraduate Research Students

Hilde

16 November 2011	Interview	30 minutes
30 November 2011	Interview	50 minutes
9 December 2011	Presentation	45 minutes (excluding the screening of the documentary)
19 January 2012	Interview	30 minutes
20 January 2012	Interview	1 hour
February – August 2012	Ongoing email exchange	Ongoing
8 August – 13 August 2012	Casual conversations – participant observation	Ongoing

Liza

27 November 2011	Interview	1 hour 15 minutes
14 December 2011	Chat Interview over Skype	2 hours
15 January 2012	Interview	1 hour 20 minutes
2 February 2012	Interview	1 hour 5 minutes
19 February 2012	Interview	1 hour 5 minutes

⁴⁵ Times are rounded to 5 minutes.

18 March 2012	Interview	1 hour 10 minutes
6 April 2012	Interview	50 minutes
29 April 2012	Interview	55 minutes
22 June 2012	Interview and presentation	45 minutes
18 July 2012	Interview	45 minutes

Louise		
7 April 2011	Casual conversation ⁴⁶	1 hour 30 minutes
14 April 2011	Casual conversation	1 hour 40 minutes
25 May 2011	Interview	2 hours
13 June 2011	Interview	1 hour 35 minutes
8 July 2011	Interview	1 hour
17 August 2011	Interview	1 hour
13-14 September 2011	Interviews, presentation – participant observation at the conference	1 hour 10 minutes (interview and presentation) – ongoing
25 October 2011	Interview	1 hour
2 December 2011	Interview	1 hour 10 minutes
21 March 2012	Interview	45 minutes
8 May 2012	Interview	1 hour 40 minutes
29 July 2012	Interview	1 hour 25 minutes

Paul		
3 June 2011	Casual conversation	1 hour
28 August 2011	Interview	4 hours
24 October 2011	Interview, presentation – participant observation at conference	45 minutes – ongoing
10 November 2011	Interview	1 hour
30 January 2011	Interview	1 hour 40 minutes
January 2011 – July 2012	Ongoing email exchange	Ongoing

⁴⁶ Because I had not yet asked Louise to be my participant, this is not an official interview. I therefore call it a casual conversation.

Richard		
2 December 2011	Interview	50 minutes
9 December 2011	Interview	45 minutes
6 January 2012	Interview	45 minutes
14 February 2012	Interview	1 hour 25 minutes
6 March 2012	Interview	35 minutes
4 April 2012	Interview	1 hour 25 minutes
2 May 2012	interview – filming him doing the posters	50 minutes – 1 hour
23 May 2012	Presentation	25 minutes
27 June 2012	Interview	45 minutes
28 July 2012	Interview	1 hour 25 minutes

Additional participant observation, interviews at seminars, conferences:

- Liquid Lab sessions
- Visual Dialogues 13 May 2011
- ‘Opening the multiplicity of possibilities’ Conference June 2011
 Jessie delegate at – ‘Opening the multiplicity of possibilities’ conference June 2011, was possible participant. Talked to her extensively on that day and interviewed her once. She agreed to participate, but came back on her decision later on.
- Exploring the Use of Visual Media in the Communication of Research Findings 12 July 2012 Cardiff
- Methodological Innovations 2012, 13-14 November 2012
- Visual Methods Conference 2011
- Research Method Festival 2012
- Wiserd Annual Conference 2013

Appendix III Examples of Topic Guide

General Topic Guide Postgraduate Research Participants

- Description of the research
 - When, how, why (theory, practice, methodology), What
 - What kind of material did you gather?
 - How did you analyse it?
 - How did or are you going to report it? (→ collaboration with others?)
- What kind of problems did you run into? How did you solve them?
- What did you know about the method before you started using it?
- How come you started using it? What do you think of the data/knowledge it produces? How is it different from the other methods?

Topic lists Liza

24 November 2011

- Research (what, how, why, and now?) specifically related to method
- Is this also how you intended it?
- How did you acquaint yourself with the method?
- What are you doing now? How is that going? What kind of issues did you run into?

15 January 2012

- What's been happening with your research in the last month?
 - Girls with cameras
 - the interview with the other man whose pictures you had developed
- How do you feel about the methods that you are using
- How do other academics react to your research?
- Conferences
- Do you feel confident using this method?

Topic Guide Experienced Researchers

- When and why did you first start using photography/fiction/poetry/performance as a way of doing your research?
- What kind of issues/challenges did you come across? How did you overcome these? (This refers to both the method itself and structures within academia. Probe for both)
- How do you start your research?
- How did you disseminate your work?
- You produce different kinds of work. How do you decide what kind of work you'll produce?
- When did you feel ownership of this way of doing research?
- How did and does the method add to your work?
- How has your method changed? How has your experience with the method changed?
- What do you see as challenges for this method?/How can this way of doing research be taken forward? Possibly follow up with: Is that being prevented in any way? How?
- Are there any other people you think I should talk to about this particular subject?

Appendix IV Participant Information Sheet Experienced Researchers

Creative methods, the making of..

Participant Information Sheet

Supervisors:

Karen O'Reilly, Department of Social Sciences, Loughborough University, Loughborough, Leicestershire LE11 3TU, K.OReilly@lboro.ac.uk +44 (0)1509 228353

Sarah Pink, Department of Social Sciences, Loughborough University, Loughborough, Leicestershire LE11 3TU, S.Pink@lboro.ac.uk +44 (0)1509 223878

PhD student:

Pauline van Romondt Vis, Department of Social Sciences, Loughborough University, Loughborough, Leicestershire LE11 3TU, P.J.C.Van-Romondt-Vis@lboro.ac.uk +44 (0)77 8075 4757

What is the purpose of the study?

In the last couple of years, more and more researchers have started to use arts-based methods within the social sciences. These methods form part of a wider phenomenon of innovation within both qualitative and quantitative methods in the social sciences.

With this study I hope to find out how the arts-based qualitative methods are developed. How do researchers make their way in these unknown territories? But also what kind of frameworks enable these developments?

Who is doing this research and why?

This research will be conducted by Pauline van Romondt Vis as part of her PhD project. She is supervised by Karen O'Reilly and Sarah Pink (see above). This project is funded by the Social Sciences Department of Loughborough University.

What will happen?

I anticipate we will have a conversation about your views on innovation within qualitative research methods to better understand how innovative methods are developed. This can take between half an hour and an hour and a half. It will be tape-recorded if you are comfortable

with that. Once I have transcribed the interview I will send the document to you for your feedback.

Once I take part, can I change my mind?

Yes! After you have read this information and asked any questions you may have I will ask you to complete an Informed Consent Form, however if at any time, before, during or after the sessions you wish to withdraw from the study please just contact the main investigator. You can withdraw at any time, for any reason and you will not be asked to explain your reasons for withdrawing.

Will my taking part in this study be kept confidential?

Yes, your name will be anonymised if you so wish and any clear references to other researchers, institutes, research etc. Furthermore, the interviews and audio recordings will be kept in accordance with Loughborough University Ethical Advisory Committee Guidance Notes.

What will happen to the results of the study?

I will use the results for my PhD thesis. Hopefully I will also be able to write some academic articles and use them as part of an arts-based form of dissemination.

I have some more questions who should I contact?

Pauline van Romondt Vis (see above for contact details)

What if I am not happy with how the research was conducted?

The University has a policy relating to Research Misconduct and Whistle Blowing which is available online at [http://www.lboro.ac.uk/admin/committees/ethical/Whistleblowing\(2\).htm](http://www.lboro.ac.uk/admin/committees/ethical/Whistleblowing(2).htm). Please ensure that this link is included on the Participant Information Sheet.

Appendix V Participant Information Sheet
Postgraduate Research Participants

Innovative methods, the making of..
Participant Information Sheet

Supervisors:

Karen O'Reilly, Department of Social Sciences, Loughborough University, Loughborough, Leicestershire LE11 3TU, K.OReilly@lboro.ac.uk +44 (0)1509 228353

Sarah Pink, Department of Social Sciences, Loughborough University, Loughborough, Leicestershire LE11 3TU, S.Pink@lboro.ac.uk +44 (0)1509 223878

PhD student:

Pauline van Romondt Vis, Department of Social Sciences, Loughborough University, Loughborough, Leicestershire LE11 3TU, P.J.C.Van-Romondt-Vis@lboro.ac.uk +44 (0)77 8075 4757

What is the purpose of the study?

In the last couple of years, more and more researchers have started to use arts-based methods. As these methods are still quite new, guidelines are scarce. With this study I hope to find out how researchers make their way in unknown territories. More specifically I hope to find out what kind of methods researchers are using, why and how they are using them.

Who is doing this research and why?

This research will be conducted by Pauline van Romondt Vis as part of her PhD project. She is supervised by Karen O'Reilly and Sarah Pink (see above). This project is funded by the Social Sciences Department of Loughborough University.

What will happen?

I hope to interview you on a number of occasions over the course of the next year. On these occasions we will talk about your research: what you've been doing, why you've been doing it and how you've been doing it. I would like to tape-record these interviews if you are comfortable with that. Once I have transcribed the interview, I will send it to you for your feedback.

I also hope to come along to any of your data gathering/analysis or dissemination session, but obviously only if that is possible and you feel comfortable with that. That also applies to me possibly tape- or video-recording these sessions.

These sessions will be scheduled on an individual basis. That means they take place when, where and how long it suits you. In general you will not need to prepare anything for these sessions.

Once I take part, can I change my mind?

Yes! After you have read this information and asked any questions you may have I will ask you to complete an Informed Consent Form, however if at any time, before, during or after the sessions you wish to withdraw from the study please just contact the main investigator. You can withdraw at any time, for any reason and you will not be asked to explain your reasons for withdrawing.

Will my taking part in this study be kept confidential?

Yes. My data will be securely stored by the university. In the dissemination of my research people will be anonymised where requested and details of the research will be respected. Both written documents containing interviews and field notes as well as audio and visual recordings will be kept for 10 years in accordance with Loughborough University Ethical Advisory Committee Guidance Notes.

What will happen to the results of the study?

I will use these for my PhD thesis. Hopefully I will also be able to write some academic articles and use them as part of an arts-based form of dissemination.

I have some more questions who should I contact?

Pauline van Romondt Vis (see above for contact details)

What if I am not happy with how the research was conducted?

The University has a policy relating to Research Misconduct and Whistle Blowing which is available online at [http://www.lboro.ac.uk/admin/committees/ethical/Whistleblowing\(2\).htm](http://www.lboro.ac.uk/admin/committees/ethical/Whistleblowing(2).htm). Please ensure that this link is included on the Participant Information Sheet.

Appendix VI Informed Consent Form

Creative methods, the making of..

INFORMED CONSENT FORM

(to be completed after Participant Information Sheet has been read)

The purpose and details of this study have been explained to me. I understand that this study is designed to further scientific knowledge and that all procedures have been approved by the Loughborough University Ethical Advisory Committee. I can contact the investigator on 077 8075 4757 or email her at P.J.C.Van-Romondt-Vis@lboro.ac.uk

I have read and understood the information sheet and this consent form.

I have had an opportunity to ask questions about my participation.

I understand that I am under no obligation to take part in the study.

I understand that I have the right to withdraw from this study at any stage for any reason, and that I will not be required to explain my reasons for withdrawing.

I understand that all the information I provide will be treated in strict confidence and will be kept anonymous and confidential to the researchers unless (under the statutory obligations of the agencies which the researchers are working with), it is judged that confidentiality will have to be breached for the safety of the participant or others.

I agree to participate in this study.

Your name

Your signature

Signature of investigator

Date

Appendix VII Images Presentations

Louise





Main theoretical concept

Sub concept (reference to example of literature: e.g. O'Reilly, 2009)

Sub concept (reference to example of literature: e.g. Pink, 2007)

Sub concept (reference to example of literature: e.g. Wenger, 1998)

Sub concept (reference to example of literature: e.g. Ingold, 2007)

Gap

Problem

Louise's University Logo



Other Main Theoretical Concept

Example of concept (reference to example of literature: e.g. e.g. O'Reilly, 2009)

Example of concept (reference to example of literature: e.g. Pink, 2007)

“Quote from the literature exemplifying the main theoretical concept” (name author, year: page)

Louise's University Logo



Questions

- Research question
- Research question
- Research question
- Research question

Louise's University Logo



Methodology

Participatory visual ethnography

explanation and more information on the exact approach

Collaboration with students

Reason for doing this (reference to literature)

Reason for doing this (reference to literature)

Reason for doing this (reference to literature)

Reason for doing this (reference to literature)

Louise's University Logo

Data Collection

Sources

Observations

Researcher photography

Group interviews (# groups)

Participants photo-diaries

Question to guide the photographs

Question to guide the photographs

Participants and Setting

Information on the specific place Louise did her research and demographics of the participants.

Photo-
graph 1

Photo-
graph 2

Photo-
graph 3

Louise's University Logo



Analysis

- Aspect 1
- Aspect 2
 - Subaspect 1
 - Subaspect 2
- Aspect 3

Pseudonym participant #1: theme

Photograph 1

Photograph 4

Photograph 2

Photograph 3

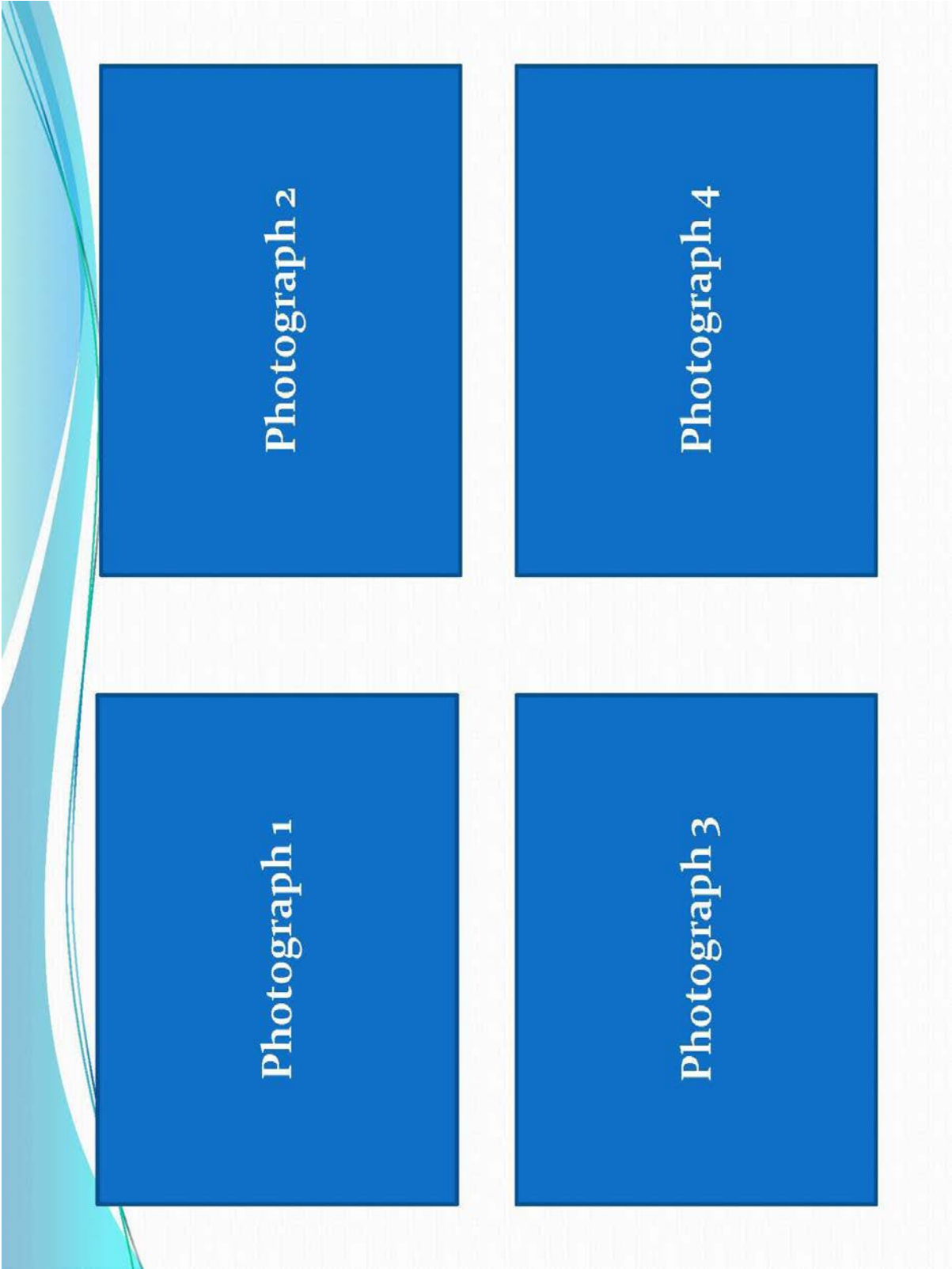


Pseudonym Participant #2: theme

**Dialogue from the
interview with the
participant**



Photograph





**Long excerpt from the interview with
the participants about this particular
photograph**



Photograph

**Pseudonym
participant #
3: theme**

photograph

photograph

photograph

photograph

photograph

**Excerpt from the interview
with the participant about
photographs**

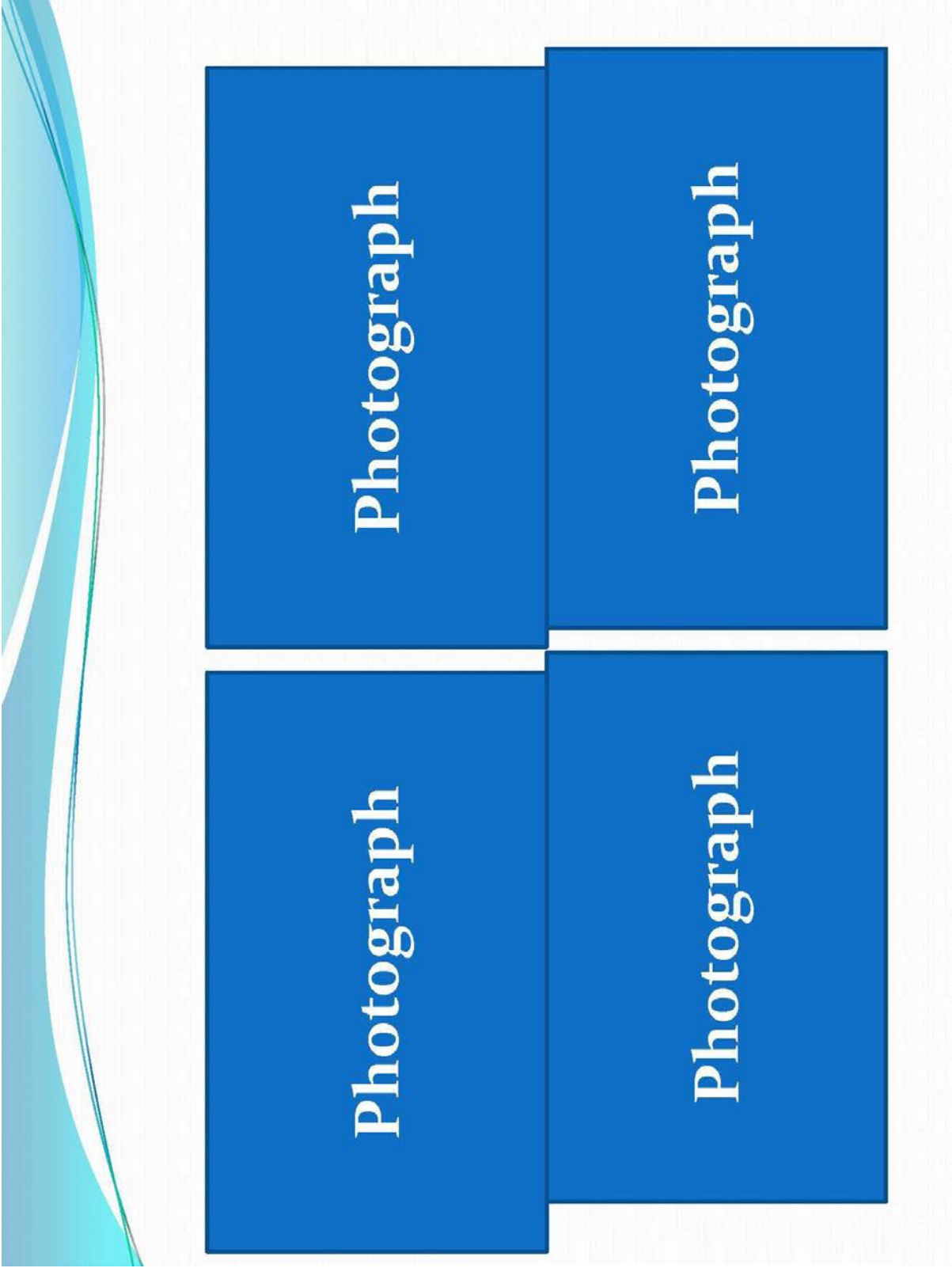


Photograph

Excerpt from interview

Photograph

Louise's University Logo





Photograph



Raising questions

- Question/critique in relation to method + elaboration
- Reflection on method
 - Point one
 - Point two
 - Point three

- Logo Louise's University

Title of Liza's Presentation

Liza's name (PhD Student)
Department of Geography, University X
Supervisors: Dr. X and Dr. Y.

Photograph

Photograph

Photograph

Background

- Demographic information about participants
- Demographic information about participants
- Issues for this particular group

Map
Conti
nent

Map
Coun
try

Objective and Research Questions

Objective

- To investigate x

Research Questions

- Question 1
- Question 2

Operational definitions

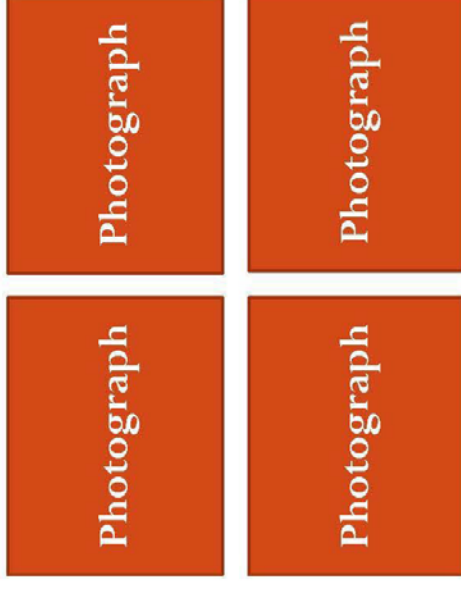
- Demographic aspect #1
“definition of this aspect” (Governmental institution)

- Demographic aspect #2



How to investigate [participants with X] in [country]?

- Qualitative multi-method approach combining different observational, oral, visual and written methods
- Methods used in this study
 - Participant observation
 - Life story interviews
 - Focus group discussions
 - Photo-diaries
 - Written diaries
 - Video- documentary



Participant Observation – main locations

Name location

Photograph of
location

Name location

Photograph of
location

Name location

Photograph of
location

Name location

Photograph of
location

Participant observation

p Advantage #1

p Advantage #2

p Advantage #3

⊘ Disadvantage #1

⊘ Disadvantage #2

Life story interviews (LSI)

	Female	Male	Total
Demographic #1	14	12	26
Demographic #2	5	7	12
Demographic #3	2	2	4
Total	21	21	42

Photograph

- Advantage #1
- Advantage #2
- Advantage #3 which is exemplified by the quote below:

“Quote of one of the participants”

Life story interviews

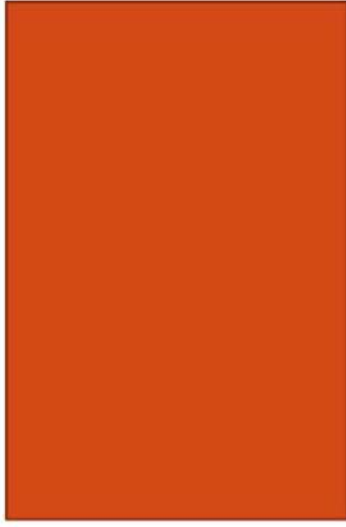
Participant's (age) life story

- Event #1
- Event #2
- Event #3
- Event #4 → lead to situation x
- Event #5
- Event #6 → lead to situation x
- Event #7

photograph

Focus group discussions

	Female	Male	total
Demographic #1 group 1	2	2	4
Demographic #1 group 2		4	4
Demographic #2	2		2
Demographic #3 group 1	3		3
Demographic #3 group 2		3	3
Total	7	9	16



Focus group discussions

- **Advantage #**
- **Advantage #**
- **Disadvantage #**
- **Disadvantage #**

Photo diaries

Theme #1	Name participant, age “quote”
Theme #2	Name participant, age “quote”
Theme #3	Name participant, age “quote”
Theme #4	Name participant, age “quote”

Photograph

Photograph

Photograph

Photograph

Written diaries

	Female	Male	total
Demographic #1	3	2	5

- Advantage #1
- Advantage #2
- Advantage #3
- Disadvantage

Photograph of
written diary

Highlighted text in
diary

Video-documentary

**Embedded video of a day in
the life of one of Liza's
participants**

conclusion

- Advantage of using these methods #1
- Advantage of using these methods #2
- Advantage of using these methods #3
- Final conclusion

Poem

TITLE OF THE POEM

POEM

(Reference)

Thank you very much!

Photograph

Photograph

Photograph

Title: runs over whole canvas
Subtitle also does this



Background

Public concern #1

Proposed line of action with target group

Public concern #2 regarding target group

Lack of literature reporting life experiences from target group

Research Aims

To see public concern#1 from the eyes of the target group...

To understand how their experience is shaped by their social influences

To try and change their experience for the better



Methodology

Participants were a diverse group of 20 persons from target group from 4 different places

Ethnographic approach

Various qualitative techniques

I was a passive observer,
a [particular role#1],
a [particular role#2],
an interviewer,
an interviewee,
a collaborator and
a [particular role#3]



What next...



UNIVERSITY OF CITY

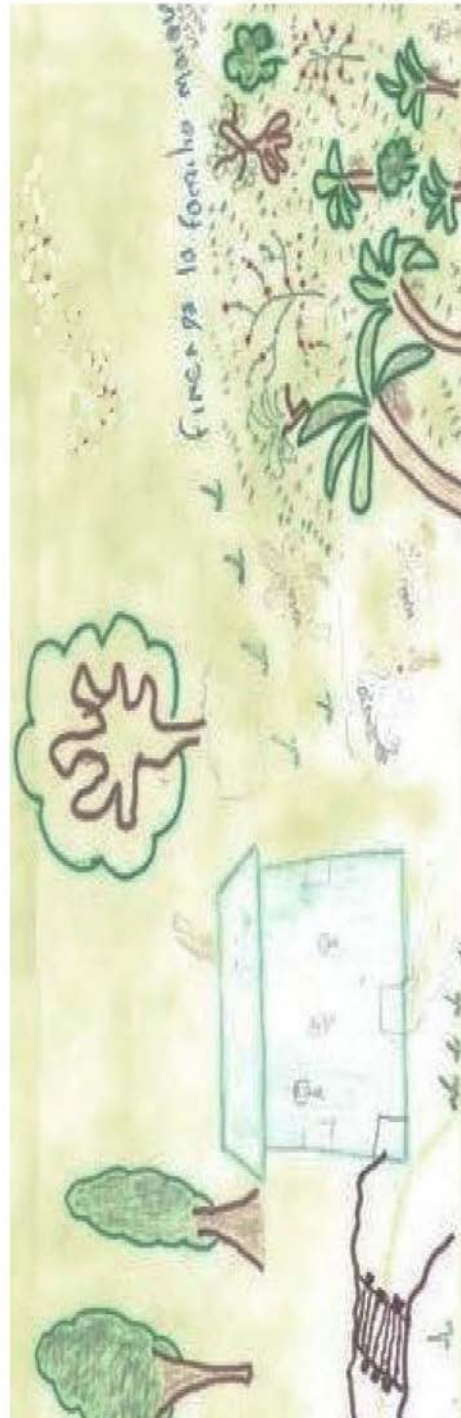
Other Logo University

Department of Geography and Geology

Logo University

Main title:

Subtitle



9th December 2011

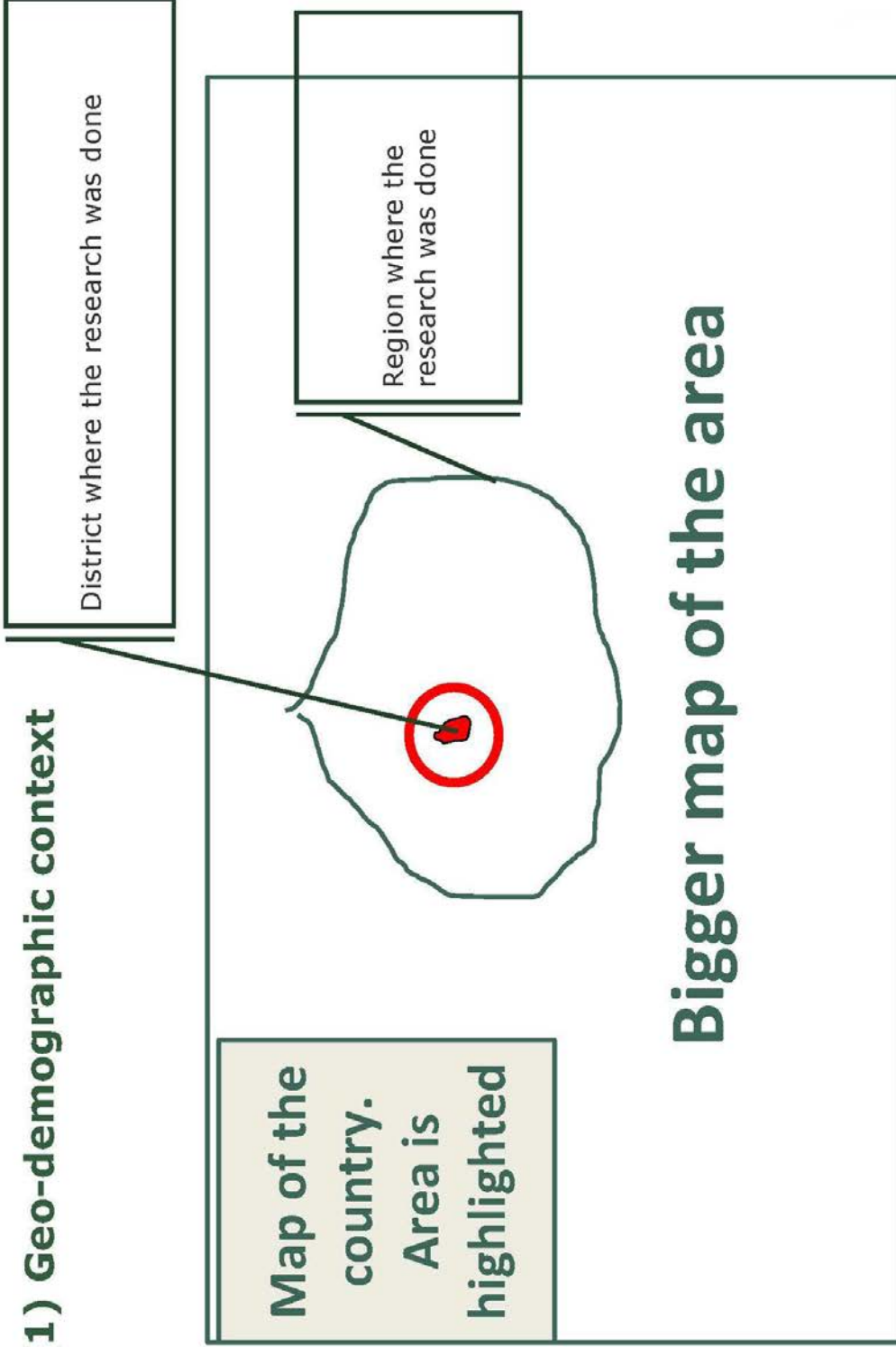
Hilde's name, PhD fellow
Institute of Geography & Geology
Academic emailaddress - personal
emailaddress

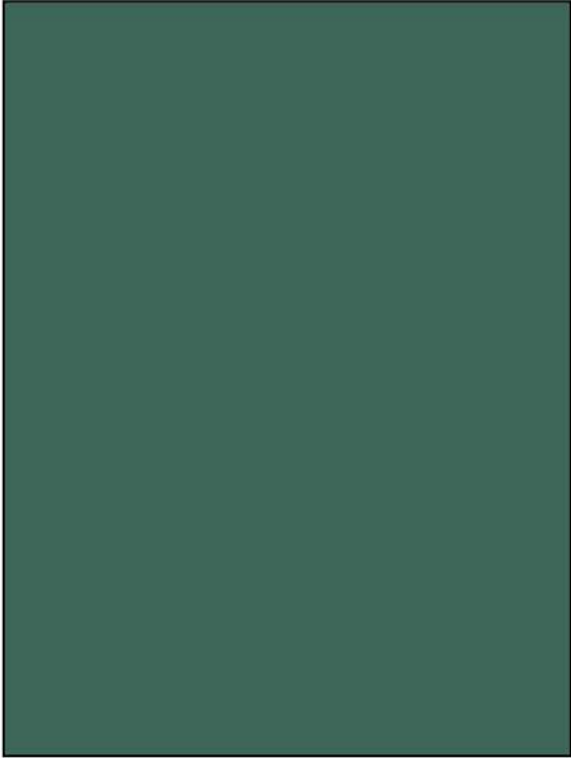
Order for the presentation

- 1. Geo-demographic context**
- 2. Topic in practice**
- 3. Technological context**
- 4. Political Context**
- 5. Stages in the Participatory
Documentary Process (PDP)**



(1) Geo-demographic context





Information about the political situation which changed things for Hilde's participants.



Information about the **local communities** with which Hilde did her **participatory action research project.**

(2) Topic in practice & the research focus

Participatory Action Research project to understand **socio-environmental conflicts** in specific district

Making a participatory documentary was an attempt to **work with local youth** in the project.



Photograph

(2) Topic in practice

- Stakeholder 1 = name
- Stakeholder 2 = name
- Stakeholder 3 = name
- Stakeholder 4 = name
- Stakeholder 5 = illegal!!!



Photograph

Local issue is based on the **activity** of stakeholders.

Photograph

As a response, local communities have become very active in **social organization** and **mobilization**.

Photograph

(3) Technological context

Participants had **never** seen a **documentary** before.

~~Photograph of camera~~

~~Photograph of camera~~

Participants **never** had a video **camera** in their hands before.

Participants have very **limited access** to (national) **television**.

(4) Political Context (March - July)

Political context by Group 1

Political context by Group 2

Political context by Group 3

Political context by Group 4

(5) Stages in the Participatory Documentary Process (PDP)

- **3 thematic workshops** to build the script
- **2 technical workshops** on audio-visual production
- **5 days to film** in The District **+** **2 days to film** in City X and City Y
- **Technical Editing** with one local representative **+** **1 Participatory Editing workshop**
- The **documentary** is **copyright free**

Photograph

For young
participants...

**PDP was
an active
and
creative
way to
Oppose,
Mobilize &
Contribute!**

I am very **Grateful** to:

The **12 young participants**

Community from The District (generous and brave)

The people Hilde collaborated with

The Collective Hilde was a part of & You all!!!