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Pondering on methods

A variety of methodological concerns

Katarina Jacobsson & Katarina Sjöberg (eds.)

Faculty of Social Sciences
Lund University
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Introduction

Katarina Jacobsson and Katarina Sjöberg

This book is the outcome of the course *Qualitative Methods* offered as an interdisciplinary course within the PhD programme in social science, given during the months of February-March 2010 and 2011. The ambition of this book is to present a plethora of methodological concerns. The contributors come from various disciplines, including Health and Society, Human Geography, Media and Communication, Political Science, Social Work, and Sociology. In the context of the authors' specific research fields, they reflect upon and discuss methodological issues of great concern not only to their own projects but to qualitative research traditions in general.

The book is divided into three parts. In the first part, titled "Capturing the field," the contributors grapple over issues regarding how to define and demarcate the field of research. *Cecilia von Schéle* takes on the challenge of grasping an urban void or "undefined" urban space, the case of "Ödetomten" in Malmö. She stresses the importance of approaching the task with multiple methods. *Roberto Scaramuzzino* discusses the problems of studying organizations that lack properties central to organizations in a general sense. The author points out the particularities inherent in civil society organizations, emphasizing the need for methodological flexibility and creativity in the process of gathering material. The focus of *Gabriella Scaramuzzino's* contribution is research on the Internet, providing an overview of questions regarding the definition and interpretation of empirical data in this forum, dealing specifically with problems connected to observing the "unseen". In response to the common notion that qualitative interviewing should be conducted face to face, *Lotta Järgervi* challenges the idea that telephone interviewing is an inferior method. She points out instances when the advantages of telephone interviewing outweigh the disadvantages.

The second part is titled "Relations in the field". In this part the authors deal with methodological concerns regarding the relationship between the researcher and the researched. *Niklas Altermark* addresses the politics of methodology, focusing particularly on inclusive research methods among disabled individuals. Starting with an overview of literature discussing individuals with learning disabilities, the author identifies, among other things, a tendency to cling to normative ideals of inclusion, instead of facing practical challenges to the realisation of the ideal. *Maria Heintz* discusses ethical concerns with regard to studying children in school. She argues that it is necessary for the researcher to act as an "out-of-the-ordinary-adult", implying that the researcher should avoid roles

associated with an ordinary adult or roles in which the researcher pretends to be a child. *Karin Lindsjö* deals with ethical dilemmas when studying vulnerable groups, focusing on people with HIV and AIDS in Tanzania. Among other things she questions the unspoken taken-for-granted idea that research in developing countries ought to incorporate the notion of also doing good.

The third and final part of the book, titled "Theory as methodology" contains contributions that emphasize methodological implications resulting from theoretical choices. *Susanna Magnusson* investigates basic theoretical and methodological assumptions embedded in different views of culture, especially the role of emic and etic. The remaining two contributors address discourse as an applied research method and as an analytical tool. *Martha Kolankiewicz* takes her point of departure in a presentation and analysis of Laclau and Mouffe's discourse theory, on the one hand and, on the other hand, Fairclough's critical discourse analysis. *Ståle Holgersen* uses critical discourse analysis to highlight the discursive and the extra-discursive in the context of change in an urban development in Malmö city.

Capturing the field

To capture the void: Gathering material on undefined urban space

Cecilia von Schéele

Abstract

Understanding urban space as a phenomenon that is equally physical, political and social is theoretically a mind-bender, and empirically it implies a true practical challenge; even more so if the space at stake is described as a 'void' or a 'space between'. The present chapter addresses this problem – how to gather empirical material on a kind of urban space termed the 'urban void', a concept referring to an administrative unit as well as a physical phenomenon. The urban void is the left-over space of the city, a space without a clear definition, put on hold until plans are made and development is furthered. With the task of investigating the presumed ambiguousness of the urban void, the case of 'Ödetomten' in Malmö is studied through one interview, through the analysis of planning documents and by conducting observations. Three dimensions are introduced in order to analytically capture the different, yet interconnected, facets of urban space. These are: 'use', 'function' and 'design'. The exploration of the different methods and of the site in Malmö shows that none of the methods can alone provide useful information for all three dimensions, yet the total assemblage of empirical material offers valuable information on all three dimensions.

Introduction

The urban fabric is seldom without holes: The focus of this chapter is directed towards the city's leftover space, the 'urban void' – a space in spatial as well as temporal transition. The urban void as a concept refers to an administrative unit as well as a physical phenomenon, representing a nothingness in the formal city plans. With a research interest in this kind of space, the question soon arises: How can one seize this presumed nothingness when undertaking a study of the phenomenon? There are no given ways of how to conduct an empirical study of the urban void, a problem that the present chapter engages in.

The methodological problem at stake is one of gathering empirical material: How can one capture the void? Three ways are tested: interviewing, reading planning documents and doing observations. These methods are tried out on one case of an urban void in Malmö, 'Ödetomten.'

The overarching aim of this chapter is, as stated, to elaborate on the methodological problem as presented above. However, there would be no problems whatsoever had there not been a puzzle of a theoretical nature to solve, which is why the following part (2) briefly introduces the research field, the theoretical problem and the case. Part 3 elaborates on the three chosen methods, while part 4 contains an analysis of the material gathered, and an evaluation of the respective methods. The fifth and final part concludes the investigation.

The politics of urban space

The study of the city as a political space has recently grown in popularity within the social sciences (Badersten 2010). However, even if the city as a political arena has gained more attention, the urban space *as such* is still often considered a given in political science. Urban space, both public and private, is a matter of physical production *and* social construction, which is to be understood and analysed not as separate logics but as an entity (cf. Lefebvre 1991). Understanding urban public space from such a point of view has its point of departure in the idea that public space reflects the prevalent social and political climate of a society. In other words, the spatial and physical structure of a city mirrors the political structure of the society – the city as a phenomenon can be perceived as the built image of society (Badersten 2002:26). Hence, to fully grasp the city as a political unit, one must further the understanding of this built image: the physical structure of the city.

On the urban void

As the expansion of a city seldom follows a simple pattern where zone by zone is completely developed and integrated in the urban fabric, there are sometimes areas that, as the city grows, are left out/left-over. In many towns and cities of the post-industrial society there are often large areas relatively close to the city centre where former industrial areas are now waiting to be turned into something else (Salzman 2009:9). These are one type of urban void.

There is no given denomination for this sort of space, and a variety of terms are found in the literature: 'residual space' (Swedish: *impediment*), 'brownfields', 'space between' (Swedish: *mellanrum*), 'ephemeral landscape' (Swedish: *efemära landskap*) (ibid). In this text, these spaces will be referred to as 'urban voids' or 'voids'.

In my master's thesis (2008), where I discussed how public space can be understood as a spatial dimension of power, I set up three different terms to analytically capture the different, yet interconnected, properties of urban space, namely: 'function' (Swedish: *funktion*), the planned and prescribed use of a certain urban space; 'use' (Swedish: *användning*), the actual use of a space, and 'design' (Swedish: *gestaltning*), the design of space which is to be

understood both as the physical shaping of the space and the construction of norms and rules that characterize it.

Previously I have only used the three different dimensions as an analytical framework to study public spaces, i.e. spaces planned to fulfil ideals of continuity and accessibility, which makes them qualitatively different from the urban voids. This discrepancy between the types of spaces makes it interesting to use the three dimensions as a framework in the present context.

Public spaces in cities of stable societies are often found to show coherence in the mentioned three aspects of space, i.e. 'function', 'use', and 'design' have the same characteristics. Any central square of a western European city could serve as an example; let us call it The Central Square: The *function* of The Central Square is to serve as a meeting place/a market during certain given hours, as a place for (permitted) public meetings. The citizens *use* it as a meeting place/a market during certain given hours, as a place for holding (permitted) public meetings. The *design* of the square implies that it is an easily accessible place suited to host the given activities. It is easy for a visitor to understand that The Central Square is the central square. In other words, the characteristics of The Central Square correspond to each other.

I suspect that urban voids, on the contrary, are instead ambiguous spaces, in the sense that they have different main characteristics of 'function', 'use', and 'design' respectively. The 'urban void' is sometimes discussed in terms of a 'terrain vague', a space that "continues to resist all straightforward definition, because its semantic emptiness turns out to have less to do with an absence of codes than with a multiple presence of codes that are superimposed, that clash, or even destroy each other" (Borret 1999:240). Kristiaan Borret's description offers a way of understanding this space as one that is seemingly 'empty' or a 'non-space' – rather like a prism. The "multiple presence of codes" generates the ambiguity of the urban void. Mapping the urban void in terms of ambiguity will further the understanding of its presumed unordered logic; this, in turn, will be a vital component when moving forward to study the void as a space that is inherent to the ordered city.

The case: Ödetomten

Norra Sorgenfri in Malmö is an area of workshops, garages and small industries that will soon be under development for the creation of an area of diverse functions (housing, businesses, workshops and cafés). In Norra Sorgenfri there is a vacant demolition site at Kvarteret Brännaren (the formal name of the property), in popular speech "Ödetomten", (Eng. ≈ *derelict piece of land*) and sometimes "Stäppen" (Eng. *the Steppe*). During the 1980s and 90s all but one of the buildings of the block were demolished. Left at the site is a small office building, today a ramshackle. In the two last decades Ödetomten has at times been inhabited by homeless people living in tents or trailers, and frequented by graffiti artists, guerrilla gardeners and skateboarders.

Ödetomten can be described as an urban void. It is a space in transition – a new detailed plan is currently in the process of being made. The fact that the site is widely denoted as "Ödetomten", and by some known by the name of "Stäppen", indicates that its

wasteland character is commonly known and obvious to anyone passing by. It is as such a quite distinct example of the phenomenon of urban voids and therefore an interesting and straightforward case to use in this study.

Methodological problem: gathering empirical material

The methodological problem of this chapter, to try different ways of gathering empirical material, implies setting out to examine these as separate methods. However, since they are all carried out in parallel, during a short time span, by the same researcher – the information gathered from one source will inevitably have a spill-over effect on another; information received from one source will alter the understanding of the material collected from another source. This should not be considered a problem, but something to have in mind when evaluating the result of the different methods of collecting material.

Another concern is quality versus quantity. The aim of the present study is to investigate what different qualities the different methods can provide in terms of empirical material. Nevertheless, quantity is to some extent inherent to quality when it comes to material. This chapter has the character of a pilot study and the material gathered will be collected in small samples. Hence it should be stressed that the methodological focus here is on *what kind* of information can be provided by the different methods.

Conducting interviews

When designing a study it is elementary to use the aim of the study as a guiding-star (Aberbach & Rockman 2002:673). What kind of information one wants to gain from an interview should determine what type of interview that is deemed suitable. In this case, where the degree of prior research is low, a 'semi-structured interview', which refers to an interview situation that is neither an everyday talk, nor built on a strictly structured questionnaire, is a useful approach (Aberbach & Rockman 2002:674; Kvale 1997:32). The logic is that the greater the amount of knowledge that exists on the subject the easier it is to define the questions, and consequently, a small amount of knowledge on the subject makes it hard to formulate the right questions (ibid). The interviewee in this study was chosen on the grounds of her knowledge of the subject, and open-ended questions provide greater opportunities for the interviewee to organize answers in her own framework. If the aim of the interviews is to collect a consistent source of data that could be compared across interviews, a semi-structured approach is not the most suitable (Leech 2002:665). However, that is not a problem here, since there is only one interview being made.

As the interviewee, architect Ulrika Lundquist, the planner at Malmö City Planning Office responsible for coordinating the development of Norra Sorgenfri, was chosen. The choice was made on the grounds that she was expected to have good insight into, and knowledge of, the plans for the site of interest. The result from the interview will in some sense represent an 'official' view on Ödetomten. Indeed, the interviewee does represent the city of Malmö, although one should be careful in treating the view conveyed

in the interview as '*The* Malmö City View'. The idea that interviews can provide a sound and neutral stance is deceptive for at least two reasons. Firstly, both the interviewer and the interviewee are equally and collaboratively engaged in the knowledge production of interviews. "Being neutral is a mythological (and methodological) interviewer stance. This mythology/methodology of interviewer neutrality has the fundamental effect of 'silencing', and in some cases totally banishing, the very active, collaborative work of the interviewer in producing the talk as it is" (Rapley 2004:21). The interview with Lundquist on 17 May 2011 would most certainly not give the same information if it were conducted at another time or by another interviewer. Secondly, a city or municipality should not be viewed as one coherent actor (Czarniawska 2002). There are numerous entry-points to the organizational body of a city; hence interviewing different public officials would give a variety of views on the issue in question – even if they all represented Malmö City.

Analysing planning documents

The planning documents analysed in this study are constituted more by texts than by plans. Texts are important as mediators of norms and regulations in the society. As such, they have consequences for people's thoughts and actions (Bergström & Boreus 2005:13). Thus, they are useful material in the social sciences. It is also reasonable to think of the plans in this study in terms of text.

The planning documents are interesting from two aspects. First of all, they are official documents stating the official view of Malmö City on, and plans for, the area of Norra Sorgenfri. Secondly, through the way in which the documents describe the area as it is now, and the visions for what the area will/can be in the near future, they contribute in shaping 'a story' of the area. The analytical framework, composed by the concepts 'use', 'function', and 'design' will guide my reading.

The studied texts are all produced by Malmö stad (Malmö City). The valid detailed plan, is dated in February 1991, and covers the whole property named Kvarteret Brännaren. Ödetomten covers about one third of Kvarteret Brännaren. The document *Plans and strategies for Norra Sorgenfri* (2006) was produced in cooperation between the City Planning Office and the Real Estate Office while in the case of *Norra Sorgenfri. Planprogram (*Eng. *Norra Sorgenfri. Planning Programme)* (2008), it is the City Planning Office that is responsible. The latter is a follow-up of the former and serves as a guiding document for the production of the detailed plans to be made.

Doing observations

In political science, there is no tradition of doing ethnographic studies, and I never came across ethnography as an undergraduate in political science. *Why* ethnography is seldom practised in political science is not within the scope of this chapter to discuss in length, but one explanation might be that the topics and problems of political science are rarely constituted in a way that makes ethnography a convenient method to use. In traditional ethnographic studies, the foundation for the analysis is created by living among the studied

group of people and taking part in their practices on an everyday basis (Sjöberg 2011:10). Understood in that sense, ethnography is directed towards an understanding of informal arenas of society, while political science – as the study of politics – often focuses attention on formal arenas. This calls for the question of whether or not ethnography has anything to offer the political scientist. I do think that there are ways in which the study of politics could benefit from using ethnographic methods. However, there are few studies that could serve as models or inspiration to further the understanding of how this could be done.[1]

The distinction between 'ethnography' and 'participant observation' is not obvious. Katrine Fangen maintains that 'participant observation' is often understood as being synonymous with 'fieldwork', and that 'anthropological fieldwork' also goes by the name of 'ethnography' (Fangen 2005:29). I have chosen to use 'observation', since 'ethnography' implies a focus on people ("ethno") whereas my interest in space as a physical, political and social phenomenon is better captured by 'observation' describing a broader concept.

The observations in this study were carried out at the site of Ödetomten at six times: in the morning, in the afternoon and in the evening on a weekday (not necessarily the same) and at the same three times on a day of a weekend, between 15 and 29 May 2011.

Analysis

The analysis has been structured so as to give each method one section, which, in turn, is analysed according to the three different dimensions respectively. To give a recap of the three dimensions: 'function' is the planned and prescribed use of a certain urban space, which is contrasted by the actual use of a space, the 'use'. The 'design' of a certain space is to be understood both as the physical shaping of the space and the construction of norms and rules that characterize it.

Interviewing – talking the urban void

The interview with Ulrika Lundquist was built around 'grand tour questions' (Leech 2002:667), which ask the interviewee to "give a verbal tour" of something they know well (ibid). Lundquist was asked about the Norra Sorgenfri development process in general

1 Even with the help of more experienced colleagues I only managed to find two (fairly recent) Swedish dissertations in political science using an ethnographic method. However, none of the authors, Gun Hedlund (1996) and Katarina Barrling Hermansson (2004), use 'ethnography' as a term to describe the method, but rather 'direct observation' (Swedish: *direktobservation* (Barrling Hermansson)*, direkta observationer* (Hedlund)). Neither of them uses direct observation as her only method but as a complement to interviews. This is not to say that these are in fact the only ones, rather it is a number that should be understood as an indicator of the rarity of examples.

and the status and development of Ödetomten[2] in particular. As the interviewer, I did not mention my research question, nor did I introduce the concepts of function, use and design.

Function. For the matter of the current status of function of Ödetomten, Lundquist refers to the valid detailed plan which can be accessed from the Malmö stad website, and she does not go into further details on the subject. Her main interest lies in the process of the development of Norra Sorgenfri, a process in which Ödetomten is an important part. There is a detailed plan in the making for Brännaren 19 (which makes up 50% of Ödetomten), and even though that does not help this study narrow down the current function of Ödetomten, what Lundquist says about it is interesting: the property owners of this part of Ödetomten do not see any need for pushing the prompt making of the detailed plan since they are interested in how the neighbouring plots will develop before they further any development of their own plot (Lundquist 2011). This confirms the status of Ödetomten as waiting for something. Since the valid detailed plan does not reflect today's reality (see section 4.3), and since Lundquist's talk expresses the City Planning Office's commitment to the process *per se*, i.e. how to take the area from what it is now to what we want it to be, it could be argued that the planned and prescribed use (i.e. 'function') of Ödetomten revealed in the interview is "waiting". The City Planning Office is, as of now, first and foremost engaged in the process – the bridge between what was and what is to come. Ödetomten has no actual official use (the 'function'); the function is being a part in the process. It is s a piece in a jigsaw puzzle about to be laid. The primary function of Ödetomten conveyed in the interview is being a part of the process that is the development of Norra Sorgenfri.

Use. There is a certain dynamic to Norra Sorgenfri, says Lundquist, and maintains that one of the reasons is the tension between today's activities in the area and what will be tomorrow. She gives examples of this, and stresses that the mix is interesting, "artists and all sorts of different activities, from schools to offices, a dance school, driving test facilities, and the Migration Board Office, and a variety of other things, as well as more manufacturing businesses and just the fact that the area is in use is important [for the development], but [Ödetomten] falls out of that [picture], since [that is] not being used" (Lundquist 2011). The activities and businesses are, according to Lundquist from her point of view at the City Planning Office, all established, legal and with the paper work in order. From an official point of view, there are no established activities or businesses acting at the premises of Ödetomten, hence it is not in use.

Design: When describing Ödetomten in terms of design, Lundquist does it incidentally and in general terms, still very illustrative: "It's like this area is a bit […] dead […] it's a bit of a vacuum" (Lundquist 2011). This confirms the impression of Ödetomten as a void – an area of nothing.

2 In the interview the terms Brännaren 2 and Brännaren 19 were used, which are the formal names of the two plots that make up Ödetomten.

Planning documents – reading the urban void

Function: According to the detailed plan, the function of Ödetomten is to house office buildings, small-scale industries and crafts. Alongside Industrigatan, a small strip of Ödetomten could in addition house stores on the ground floor (Malmö stad 1991). The detailed plan provides information on the function in a very dense form and virtually represents the narrowest definition of this dimension – the planned and prescribed use of an area.

Use: In the documents *Plans and strategies for Norra Sorgenfri* and *Norra Sorgenfri. Planprogram* there is little information on the current use of Ödetomten, probably partly due to the fact that these are plans for Norra Sorgenfri, an area of which Ödetomten only comprises a part of the whole area, and partly due to their visionary purpose. However, in a section headed "Identity" in the Planning Programme (*Planprogram*), the creative use (the presence of "artists and youth") is commented as a resource for the future development of the area. The skateboard ramp/bowl (at Ödetomten) is mentioned for having drawn positive attention to Norra Sorgenfri (Malmö stad 2008:13). "Over the past few years Norra Sorgenfri has become an area that has attracted a large number of artists and other creative expressions" (Malmö stad 2006:19).

Design: With regards to the physical part of the design dimension: From the detailed plan, it is possible to read where at Ödetomten the buildings were (or were to be) built and how many storeys to which the buildings were restricted (Malmö stad 1991). In the other planning documents, with regards to design, the focus is on what is to come, more than on what is today. When the present design is mentioned, it is done in a general sense, and about the whole area. Ödetomten is never specified in terms of design, except for one sentence about the only house on the spot; the old brick office building is mentioned as a characteristic building worth keeping (Malmö stad 2008:52). The lack of information on the design of Ödetomten in the planning documents shows that Ödetomten lives up to its name: it is a void, an inversion of design. The photos and maps published in the plans show, with one exception for the old office building, an empty space. As if there is nothing there.

Observations – encountering the urban void

Function: The function of Ödetomten is not possible to trace from observations.

Use: The use of Ödetomten is multifaceted. One aspect of the use is visible through seeing what people actually *do* at the site; other aspects are visible through the clues of what has been left on the ground. Found artefacts such as remnants of cables gives the criminal dealing of copper away, piles of trash close to the fence expose the fact that some consider this place to be a refuse dump, a number of sofas tell the visitor that some people like to hang out here. When it comes to what people do at Ödetomten, one thing worth mentioning is that *people* at Ödetomten have at all times of observation been *men* (with the exception of myself). What men do at Ödetomten: they cross over the plot, using it as a short cut; they do construction work on the skateboard ramp, and they move sofas around.

Design: The design of Ödetomten is inversed design. Concerning the physical aspect of

design, which, in this case, should be understood as the built result of a (man-made) plan for a space, there is none – in the same way as nature (by this definition) does not have a design. Ödetomten is a place where design has been removed, where the traces of torn down buildings and railway tracks are the only fragment of design left. The neighbouring properties and streets define the shape, the contour, of Ödetomten. It has no (designed) shape of its own. Yet, design is not absent at Ödetomten, the skateboard ramp is indeed designed, and so is the still standing open-air oven that a (male) artist built at the site a couple of years ago. It is a space designed in fragments.

The norms and rules (also a part of the design dimension) that characterize Ödetomten are also fragmented, or rather, contradictory. As Tomas Wikström notes, "[r]esidual space offers opportunities to withdraw from the formal and informal control of public space to a less controlled territory. However, this lack of vigilant control does not imply the non-existence of rules" (Wikström 2005:54). On the one hand, Ödetomten, by being a field (or a dirty meadow), invokes the feeling of being in the countryside: when I was crossing Ödetomten diagonally one afternoon, I met a man at the narrow path leading through the birch tree forest and as we passed each other it felt appropriate to nod "hello" to the stranger just as it does when you meet someone in the countryside. On the other hand, Ödetomten is a living room: when approaching the skate ramp and two men of my own age that were doing construction work on the ramp, I immediately sensed that I had crossed a line when I came too close – as if I had entered into someone's private garden. It is significant that I had previously spotted the same men carrying a sofa, putting it down close to the ramp – the sofa contributing to turning the place into a living room. The norms that rule that part of Ödetomten are not the ones valid in a public space, since entering it is entering the private sphere of strangers.

Conclusions

It is apparent from the analysis that none of the methods applied alone could provide useful information for all three analytical dimensions, yet the total assemblage of empirical material offers valuable information on all three dimensions. However, this is not enough to conclude how to capture the void. Two issues need elaboration: One is the paradox of capturing *a void* – how to treat the lack of information on the urban void? Does "no information" reflect the nothingness of the urban void or is it just no information? Another issue is the risk that the gathered information on the different dimensions shows ambiguity due to the fact that the information is gathered with different methods, so that the results show a dissonance between the methods rather than an ambiguity between the dimensions.

As an illustration to highlight the first issue, the paradox, the results of the analysis have been summarized in a matrix, (see Figure 1 below). The ticked boxes indicate for which analytical dimension the different methods could provide valuable information. The matrix has been designed so as to only allow for "yes"/"no" answers and it is as such indeed a very schematic presentation. The contrast between the ticked boxes and the unticked ones is illustrative in its deceptiveness: The tick denotes the presence of valuable information that

is visible or audible in the empirical material. The lack of a tick could *either* signify the actual absence of valuable information, for example that observations could not provide information on the 'function' of Ödetomten, *or* it could hide/show information that is neither visible nor audible but informative in its mere absence, and only comprehensible together with other information.

Figure 1

	Interview	Planning documents	Observations
Function	√	√	
Use		√	√
Design	√		√

One example in this study is the lack of information on 'use' provided by the interview. This lack of information, especially from a rather official source, highlights the information on 'use' gathered from observations – that the activities taking place at Ödetomten are not discernible from the municipal point of view. Hence, the non-coherence between 'use' and 'function' is stressed in a way that would not have been as clear without help from both the observational information *and* the lack of information in the interview. This contributes to the conclusion that a combination of methods for gathering material on the urban void is necessary. Knowledge is also needed on when and how "no information" is valuable as such, a comprehension only to be reached through the combination of methods and the way they contrast each other. As a result of this discussion, the matrix is revised, (see Figure 2).

Figure 2

	Interview	Planning documents	Observations
Function	√	√	
Use	Valuable lack of info	√	√
Design	√	Valuable lack of info	√

The matrix (in Figure 2) can also illustrate the second issue: the risk of the non-coherence revealed being a result of using different ways of gathering material. The question here is whether or not the different methods, being qualitatively different from each other, give qualitatively different information and, whether or not that implicitly gives a non-coherency between the three dimensions. Certainly, the three different methods give diverse kinds of material on the urban void. However, just as the three analytical concepts of function, use, and design mirror distinctive features of the urban void and are all therefore needed in an effort to provide a detailed description of the place, the variation in information provided by the three methods has proven useful in contributing to the accumulated knowledge on the characteristics of Ödetomten. Nevertheless, the risk of confusion between theoretical and methodological input is only avoided if more than one box per analytical dimension

is ticked. That way it is possible to contrast the information received from the different sources with each other, which not only offers a contextualization but also enables deeper knowledge to be drawn from the material.

The empirical study of Ödetomten showed that, in contrast to a stable public space, there is no coherence to be found between the dimensions of function, use, and design respectively. The function of Ödetomten is outdated and in a state of transition into something else, renewed. The use is sometimes illegal, sometimes the everyday activity of a skate park, or that of a rural meadow. The design does, in fragments, cohere with the use, but an overall applicable design is absent.

Moreover, the study showed non-coherence *within* the different dimensions. The function is both of yesterday (the detailed plan) and tomorrow (the interview). According to the interview, there is no use at Ödetomten, while it is strikingly clear from doing observations that Ödetomten is frequently used. The design can be expressed both in terms of vacuum or absence as well as in terms of being artistically very well thought out.

Hence Ödetomten steps into the presence of the researcher as an ambiguous space that does not lend itself to being categorized. The urban void is the space between.

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Challenges when studying diffuse forms of organisations in civil society

Roberto Scaramuzzino

Abstract

The aim of this chapter is to discuss some of the methodological implications of using qualitative research methods to study civil society organisations with a weak organisational structure. Acknowledging the starting point that organisational properties affect the research method, the chapter revolves around two major methodological issues: 1) the delimitation of the field and 2) the choice of data and data collection technique. The first issue brings forth the problem of how to define an organisation and how to distinguish individual action from organisational action, the second of how to access data through interviews, observations and/or documents. The distinction between "naturally occurring" data and "manufactured" data is used in the attempt to highlight strengths and weaknesses of different kinds of data. The discussions are exemplified through cases from my PhD study and focus mostly on immigrant organisations. The delimitation of the research field can be challenging because of the diffuse form of organisation that characterises some parts of civil society. Moreover when data at the organisational level are difficult to collect, a flexible approach towards data collection and research design might be the most viable method.

Introduction

The aim of this chapter is to discuss some of the methodological implications of studying civil society organisations by using qualitative research methods. The question was raised during my PhD project when I came into contact with the object of the study: immigrant organisations. The major challenge was that some of the organisations, even if presented as "associations", "organisations", "unions" etc. were so diffusely structured that it was difficult to determine whether they should actually be considered collective actors and thus be included in my study. Moreover, these organisations were difficult to grasp since

the boundaries between the organisation and the individual actors involved were blurred.

In fact, civil society is composed of a large variety of organisations, some of which show clear boundaries between supporters, members, representatives and staff as well as a clear distinction with regard to decision making processes based on organs such as the assembly, the board etc. These organisations might also be recognised by public authorities and produce documents on a continuous basis such as reports, plans, applications, evaluations etc. In contrast, other organisations might be made up of loosely connected networks of people working together for a more or less specified common goal with an unclear leadership, no records of members, no clear decision making processes, without any recognition by the surrounding society and without any production of documents at all. In this chapter I will try to present and discuss, from a methodological point of view, the challenges of studying civil society organisations that are closer to the latter type of organisation.

David Buchanan and Alan Bryman (2009:5) elaborate a system of influences on the choice of organisational research method and one of the aspects they highlight is "organisational properties" as "…one of the properties of the organisational research field, which can systematically, and unavoidably, influence choice of method". Among the "organisational properties" that affect the research method the authors point especially to the following dimensions: size, location and sites; professional bureaucracy; role and structure, stability and instability. Taking this into consideration my point is that the large variety of organisational forms and properties among civil society organisations might stress the way in which such organisational properties affect the choice of method. How can one approach an organisation that lacks a proper location, is based entirely on volunteer work, is an organisation in which personal and organisational roles become mixed up, lacks a clear structure and is unstable? In this chapter I will discuss two different methodological issues: *the delimitation of the research field* and *the choice of data and data collection techniques*.

Whether this becomes a methodological issue or not depends of course on the study object and the research question. I will give some practical examples of situations from my research when approaching the organisational field has been especially challenging. Discussing the delimitation of the field I will refer to two quite different ways of approaching civil society organisation so as to exemplify, on a more practical level, the methodological challenges. The first one aims at *mapping the organisational "landscape" of civil society organisation* in a specific context such as a municipality, a neighbourhood etc. The second aims at *studying civil society organisations in a specific setting* when they interact with other organisations, social institutions or structures. Addressing the second topic regarding the choice of data and data collection methods I will make use of the distinction between *"naturally occurring data"* and *"manufactured data"* to discuss the different "qualities" of the data.

My interest in the topic originates in my research interests in the role of civil society in the welfare state in general, and especially the way in which socially disadvantaged groups mobilise and organise themselves to affect their own situation through both advocacy and self help.

The first part of the chapter addresses, on a more theoretical level, what it is that characterises a civil society organisation as an object of study. The second part addresses the question of how to delimit the research field by defining the study object. The third part addresses the question of how we study the chosen object and what kind of questions we can get answers to.

Civil society organisations as study objects

Civil society is sometimes illustrated as a specific social sphere distinct from the public sector and the private corporate sector (Somers 1995). When it comes to the public sector and the private corporate sector it is easy to see how rules and legislation create formal organisations that are recognised by voters and taxpayers and by customers and stockholders (cf. Wijkström & Einarsson, 2006). This facilitates the assesment of whether the phenomenon we are interested in is to be considered an organisation or not. However, many studies of civil society organisations that aim at mapping the so called "third sector", struggle with defining the study object. One of the most famous and internationally acknowledged definitions of a civil society organisation is the one formulated in the John Hopkins study (Salamon et al. 2004). This definition is built on five structural-operational features that define a civil society organisation: the organisation should be organised, private, not profit-distributing, self-governing and voluntary. The first feature, that the organisation should be "organised", has many analogies with the dimensions highlighted in the above mentioned "organisational properties". A civil society organisation, to be defined as one, should in fact have a permanent and regular organisation which is reflected by elements such as regular meetings, membership and decision-making procedures that members recognise as legitimate (Salamon et al. 2004, Lundström & Wijkström 1997).

Given that civil society organisations are not regulated by law (for Sweden see the discussion on freedom of association in Prop. 2009/10:55), they can take many different forms and structures, work under different names and labels and under different conditions. This means that the social landscape includes a grey-zone of organisations that might or might not fit into our definition of a civil society organisation. This was also the experience of the researchers in the John Hopkins study (Salamon et al. 2004). Thus when studying organised civil society the organisational properties might not only affect the technique of collecting data but also the identification of the study object and the delimitation of the field. This might especially be the case for those organisations that you could call "we for us" organisations (Meeuwisse & Sunesson 1998) i.e. the users' own organisations. Here we find the organisations of the oppressed and afflicted, the disabled and the marginalized, as well as ordinary care consumers. Immigrant organisations can also be placed in this category at least those representing groups that are socially disadvantaged, vulnerable or marginalized.

To delimit the scope of the research work it is necessary to reflect over the nature of the object of the study, in this case organisations. Göran Ahrne and Peter Hedström (1999) explain the difference between organisation, organising and institution in the following

way: organizations are formal associations of certain identifiable individuals and may be actors as organizing means action. Institutions are more a cognitive phenomenon consisting in for example knowledge, beliefs, regulations and legislation. An organisation can thus be seen as an actor, but it is nevertheless true that only individuals can perform its actions. Sometimes it can be difficult to distinguish individual acts from organisational actions. If we observe an organisation at a great distance, the effects of the coordinated actions which the individuals perform might seem very harmonised. It may even be that the individuals disappear. Up close, however, the individual actions become more visible but the pattern, the big picture, may become more blurred and difficult to recognize (Ahrne 1999).

Individual or organisational actions?

What should one focus on then when studying organisations? Should we focus on the individuals' actions or on what seem to be the more coordinated, collective actions? Let me give an example of a research situation when these questions become relevant: a meeting with an ethnic leader who describes his organisation as a "couple of hundred" people who help each other when someone is in need. Such a description gives us information on a phenomenon that might be problematic to call an organisation. Of all activities going on among these couple of hundred people with the same background, what should be considered as individual actions and what as collective, organisational actions? Is it an organisational act or an individual one when the "ethnic leader" calls for raising funds for a person in need; and what about the single donations?

The tension between the individual and the organisation varies in different organisational contexts and can be considered part of the "organisational properties" and more specifically, connected to the dimension "professional bureaucracy" (Buchanan & Bryman 2009). In organisations that are based on wage labour, it is reasonable to assume that the tension is greater than in organisations where the work is based on voluntary action and where the workforce often shares the organisation's goals and values (Ahrne 1999). For immigrant organisations in Sweden, voluntary work is the most common labour resource (Dahlstedt 2003) and the identification with values and goals can be assumed to be relatively strong, at least for the people that are active in the organisation. The organisations are in fact seldom professionalized, often weak in resources and oriented towards recreation, social activity, information and representation of group interests (Odmalm 2004).

The lack of strong tension between the individual and the organisation in many immigrant organisations could lead to a reduced possibility to distinguish individual acts from organisational effects. In many immigrant organisations activities are mainly based on the efforts and commitments of certain individuals. These efforts are, because of the flat, network-like structure and the minimal bureaucracy and administration, often poorly integrated with the rest of the organisation, such as the board or assembly, etc. This may reinforce the notion that boundaries between individual and organisation are blurred. In the case of immigrant organisations we might have to deal more with an organising process than a formal organisation. The study of this kind of organisational phenomenon puts us face-to-face with the methodological problem of delimiting the research field: When

does collective action become organisation and how does one distinguish individual from organisational action? To discuss these questions in the next chapter I will use the two above-mentioned research approaches as examples: the mapping of an organisational landscape and the study of organisations interaction in a specific context.

Delimiting the research field

The issue of defining civil society organisations and their actions is not always relevant in third sector research. The research approach aiming at mapping the organisational landscape often makes use of different registry data. It is, for example, very common to use data from administrative agencies such as Statistics Sweden (SCB) which collects data on civil society organisations that have economic activities (cf. Vogel et al. 2003, Wijkström & Einarsson 2006, SCB 2011). Also among studies of immigrant organisations we find attempts of mapping the local or national immigrant organisational field describing the number of organisations, membership, leadership, activities, financing etc. (cf. Bäck 1983). In the study of immigrant organisations, which usually lack relevant economic activities, it is common to use registry data from public authorities that administrate public grants, both at a national level (cf. Bäck 1983, Dahlstedt 2003), and at a local level (cf. Dahlstedt 2003). Other databases might also be available where registration is voluntary and performed at the initiative of the organisation itself.[3] The use of registry data can make the issue of defining civil society organisations irrelevant because the field has already been delimited in the registry that is intended to be used.

Third sector research is not only interested in mapping the size and scope of the sector, it often focuses on organisations interacting with their environment when, for example, mobilising resources (McCarthy & Zald 1977) or when adapting their activities to external pressure (cf. Di Maggio & Powell 1983). In this case the research field is narrowed down to a more concrete context through research questions focusing on certain dimensions of organisational life. Also studies of immigrant organisations highlight the role of the broader political system for the development of an immigrant organisational field. Flemming Mikkelsen (2003) writes about an institutional tradition especially concerned with existing institutional practices with a focus on rights, duties and financing and, generally on the models and organising principles of membership in society, as well as on so called "institutional gatekeepers" such as trade unions, political parties, religious and humanitarian organisations, etc. (see for example Ireland 1994, Soysal 1994).

In the last decade, the use of the concept "political opportunity structures" has become more common in the study of immigrant organisations (Bengtsson 2010). The definition I have chosen for my thesis is formulated by Bo Bengtsson (2010:27) who defines political opportunity structures as: *Structural and institutional conditions – including consistent actor constellations – that provide opportunities for politically and non-politically oriented collective action, within civil society, with direct or indirect political impact*

3 See for example www.immi.se accessed 19-09-2011 10:01

Examples taken of political opportunity structures with which immigrant organisations interact could include the system of public organisational grants, the system of political participation through organised civil society and other networks of organisations that give access to different types of resources (money, facilities, knowledge etc.) When focusing on the organisations and their relation to political opportunity structures the delimitation of the field becomes less problematic. In a sense the interaction with the political opportunity structures is sometimes limited with regard to time (the work of a council) and sometimes also to space (the meeting room). Thus one way of delimiting the research field might be to choose an example of such a structure as a delimited context.

Furthermore, these opportunity structures are in many ways formal. The organisations that interact with them are often required to be quite structured. The municipal funding for civil society organisations handled by the authority for leisure (Fritidsförvaltningen) in Malmö, to mention one example, requires that the organisations that want to get access to the funding must register and hand in many documents such as charter, budget, yearly plan of activity and show that they are well managed in a democratic way (Odmalm 2004). These kinds of conditions for accesing the opportunity structure create some kind of threshold which sorts out more diffuse forms of organisations.

Challenges and limits

In both of the above mentioned research approaches (the mapping of an organisational field through registry data and the study of organisations through a political opportunity structure context) we risk a "blind spot".

In the first case we miss those civil society organisations that are not part of the registry because they have no relevant economic activity or because they have not bothered to register for public grants. In this sense we risk leaving out those parts of organised civil society that are not recognised or do not want to be recognised by the state. A representative of an organisation by and for women with experience of selling sexual services argued that because of the stigma that comes from making their background publicly known they cannot formalise their organisation (Scaramuzzino & Scaramuzzino 2010). When it comes to immigrant organisations we find that in 2010 the state granted public financing to 53 national umbrella organisations while an online database where immigrant organisations can freely register included twice as many (Scaramuzzino 2010).

The political opportunity structure approach might in a sense solve the field delimitation problem but if we study the interaction between the organisations and the structures we still know very little about the "entrance gate" and the threshold for participation. Why do some immigrant organisations not apply for public grants in Malmö? Furthermore, so-called public-private partnerships can be composed by several circles of organisations involved at different levels, some of them without formally participating in the application, the decision-making or the activities (Scaramuzzino et al. 2010). These informal interactions or non-interactions might of course not be relevant depending on the research questions but the attempt to answer these question brings us back to the above mentioned "grey zone" of organisations that are "invisible" at least when limiting our scope to the formal opportunity structure.

An example from my own research where the delimitation of the field became a challenge is when I wanted to study immigrant organisations' interaction with local opportunity structures at a municipal level in Genova (Italy). There were no previous studies published and no public records on immigrant organisations to support my approach to the field. I had to rely on informants from the public sector and the third sector that had been working with immigrant organisations as well as on research on the Internet on forums, blogs and websites with news for immigrants. I got in touch with six organisations and managed to interview representatives for five of them. Nonetheless, I believe my sample only represents the tip of the iceberg. In fact, five of the six organisations I got in touch with knew each other and were known to the public authorities and had a previous history of cooperation. One of the organisations which I got in contact with through the internet was not known to the others and its existence could not be verified more than through the words of the leader. Getting in contact with the less visible parts of the immigrant organisational field would have required a more time-consuming approach with observations of the urban spaces and places where such organisations might leave traces. A study conducted in 2009 identified, through such a method, 15 immigrant organisations in Genova (Soresini forthcoming).

The question of which kinds of traces the organisations leave behind is relevant not only for the delimitation of the field but also for the second theme discussed in this chapter: how the organisational properties of some civil society organisations influence the data and the data collection techniques available. The next section will address these issues using the distinction between naturally occurring data and manufactured data to exemplify different qualities of the data.

Data and data collection techniques

A major distinction is sometimes made between "naturally occurring data" and "manufactured data". The distinction is summarised by Silverman (2007) in a somewhat polemical question: "What kinds of phenomena can you see by using your eyes and ears on the world around you that you might miss by asking questions of interview respondents" (p. 38). The answer might, for example, include organisational activities and products such as meetings, demonstrations and also written documentation.

I do not interpret the distinction between "naturally occurring" and "manufactured" data as a distinction between good and bad data. The relevance of the distinction when it comes to civil society organisations is, in my opinion, more connected to the above mentioned tension between individual and organisation. As it is composed by individuals doing things in a more or less coordinated way on behalf of a collective actor, the organisational level might in fact be difficult to grasp. Especially when people are active in an organisation on a voluntary basis it might not always be very clear when they are doing what and in which role.

With the concept of "role" I want to address the tension between individual and organisation and take into account how individuals, acting in different contexts can be related

to the different roles these people hold in different social settings (Goffman 1959/1998). It is interesting to notice that it is common among representatives for immigrant organisations (and in the organised civil society in general) that people simultaneously hold different roles in different organisations. When these roles are also mixed up with roles in private life it is difficult to delimit the research field by trying to focus on organisational activities. In many of my interviews with representatives of immigrant organisations, when talking about organisational activities the topic of the conversation moves back and forth between more clearly formal organisational roles and private roles connected to families and friends, especially when the interviewee's involvement in the organisation is voluntary alongside a paid job.

An interview with a representative of an immigrant organisation might provide important information about the organisation but the data could be difficult to interpret on its own. How is the individual connected to the organisation? What purpose might the interviewee have in participating in the study? Even more problematic might be the use of a questionnaire that has been sent to the organisation's e-mail address and has been sent back.

What kind of data is accessible and how?

A problem in studying civil society organisations is that they may lack a proper office or headquarters. This is often the case when it comes to immigrant organisations (Dahlstedt 2003). They might keep their records at the private home of the chairperson and also hold their meetings there. Furthermore, they are usually not particularly good at keeping records. Also, their activities might not be so frequent which makes it hard to grasp the organisation's life within a reasonable time schedule. One could say that the weakness of the organisational structure makes the naturally occurring data rare and difficult to identify especially in the case of organisational activities. Some examples of naturally occurring data might still be available. For example an annual activity plan could be seen as a product of organisational activity and in this sense as reflecting a phenomenon at an organisational level rather than at an individual one independently of whether it has been written by one or more persons.

Another problem in the data collection is that organisations are seldom wholly defined by exactly what they are doing right now. They are also a product of what they have done, of their history, which is not seldom part of the common history of the people involved in the organisation. Studying those organisations that produce written documents on a regular basis a researcher might actually be able to get a good picture of the organisational structure, history, activities and goals through "naturally occurring data". (cf. Silverman 2007). But when it comes to some civil society organisations these records might only exist inside the memory of the people that have been active in the organisation for a long time.

Naturally occurring data and the ethnographic approach

A study of these organisations through naturally occurring data might actually look very much like traditional ethnographic studies in which "...the researcher explores how people act and react to the structures and institutions of the societies of which they are part, both as individuals and as a collective" (Sjöberg 2011:11). An ethnographic approach with an in-depth study of the everyday lives of the people involved might give access to important data that are observable. And, as is the case with traditional ethnographers, the study could require observing people's everyday life, at home, at work and in all circumstances in which it might be possible for organisational activities to occur. Going along (see Kusenbach 2003) might lead to some informant within an immigrant organisation giving information on where and when she or he (and her or his co-members) meet. Studying their organisational careers through documents (and media perhaps), and then doing interviews for gaining a more biographical, long term perspective is also a viable solution. In all this, naturally occurring data from the "go alongs" and observations of meetings, assemblies, demonstrations etc. may very well become available.

Most studies of immigrant organisations make use of "manufactured data" in their analyses. The use of semi-structured interviews with alternative questions and open-ended questions to give both a quantitative and a qualitative dimension to the study is common. This is the case especially when doing a mapping of the organisational field (Jenkins & Sauber 1988, Dahlstedt 2003). In studies focusing more on immigrant organisations' interactions with their institutional environment, such as political opportunity structures, we also find the use of interviews, often combined with "naturally occurring data" in the form of documents produced in the interaction as official documents, meeting protocols and observations of different situations (Emami 2003, Aytar 2007).

In this delimited setting you might actually be able to get access to "naturally occurring data" both by observing the interaction but also through text produced by actors involved in the structure.

In fact, some of the opportunity structures with which immigrant organisations interact also include several meetings that might be observed. This opens for the possibility to adopt a "more recent ethnographic" approach and decide to observe a more specific setting "...specific activities, limited to arenas and milieus where such activities take place" (Sjöberg 2011:54). Forums for consultations between organisations are examples of such opportunity structures that might be open for a more ethnographic approach. But the fact that some of these opportunity structures develop over a long range of time still makes it difficult to merely rely on observations. Moreover, if we look at the texts that are produced in the interaction between the organisations and the political opportunity structures, we see that most of the texts are produced by strong actors that interact in the setting and seldom by the immigrant organisations. Relying on this kind of data could have the result of the researcher not being able to access the weaker organisations' perspective.

Manufactured data and interviews

The reasons for using so-called manufactured data (i.e. interviews but also questionnaires with open ended questions) in the study of civil society organisations' interaction with political opportunity structures could in this sense be formulated in the following way: First of all the interest in the immigrant organisations' own perspective on their strategies towards political opportunity structures might motivate the use of such data. By strategy I mean a focus on the organisational actions and the actor's own accounts of these, including the way of looking at different organisational roles.

The second reason is that, as mentioned before, the organisational life of many immigrant organisations leaves few records behind that might be observed in order to gain an understanding of the interaction with an opportunity structure in a more continuative sense. Furthermore, the immigrant organisation might often play a less central role in, for instance, the public-private partnership that is being studied, which might lead the researcher to miss many important details concerning their participation in the partnership.

A third reason is that the interview gives the researcher the possibility to explore the boundaries of, and the roles in, the political opportunity structure towards the representation of the participant, without having to change settings. The researcher can get information about three types of interaction, inside the organisation, between organisations and outside organisations (Ahrne 1999) at the same time. One of the limits is of course that the interview data is "inherently interactional" and "locally and collaboratively produced" (Rapley 2004: 16). This means that the content of the interview must be analysed while taking into account the circumstances in which it has been produced and that the framing of the interview is also part of its construction.

It is also important to acknowledge that, when asking the interviewee about their experiences, we do not get in touch with reality but rather with a representation of it by someone who has experienced it (Silverman 2007). This is not a problem if we are interested in the persons' experiences and evaluation of them, especially if we assume that an organisation cannot speak for itself. In this case the interview becomes a good way of collecting data, especially when, because of the organisational structure and resources, the chance of being able to observe its representatives expressing these views in a more natural situation are very small.

Furthermore, data about an organisation, whether they are naturally occurring or manufactured, are always "fabricated" in the sense that they are produced for a specific purpose that might be more or less in conflict with the researchers' objectives. Organisational activity plans might for example be produced to impress public authorities and gain legitimacy. Also many observable activities might be a response to pressures from the outside and in this sense be "decoupled" from the actual organisational activities. Thus the data need always to be contextualised and one useful method is to gather as much information as possible from other sources when approaching the field.

How can we approach the field?

The choice of interviews as an important source of data does not exclude the combination of other methods, some of which might include "naturally occurring data". The choices of which political opportunity structures to study and of which organisations to contact for the interview or to send the questionnaire to are processes that require that the researcher gets in touch with the field. To be able to study immigrant organisations in a local context there is a need to both circumscribe the field and get access to it. To do so there might be a need of gathering as much naturally occurring data as possible. In the case of immigrant organisations this means mostly to search the internet and the urban space for information about organisations in the local context that has been chosen. Another important way of getting a picture of the field is to rely on some key informants. For example, contacts with government officials and representatives for the organised civil society could be useful both as sources of information and as facilitators for access to the field. A problem with key informants is that the role the researcher acquires in the field is very dependent on the role of the key informant (Anderson 2006).

Also the single interview is often preceded by a phase in which the researcher formulates the questions or the sub-topics that he or she intends to discuss during the interview (Rapley 2004: 18). In the case of the study of immigrant organisations' interactions with political opportunity structures, this phase includes actually trying to gather as much information as possible about the structure itself, such as documents, reports, evaluations or other key documents that might shed some light on the topics the researcher will be asking about. The study of the website of the organisations is also a good way of getting to know some relevant information in advance.

This approach to the field, even if aiming at gathering information that might help to perform an interview study, might be seen as being at least inspired by "ethnographic" studies. The aim is not only the delimitation of, and the access to, the field but also to be able to focus the interview on the topic of interest (Rapley 2004). Furthermore, it can be crucial when it comes to asking good follow-up questions. In this way I mean the researcher becomes part of the collaborative production of the interview. Being able to use what might be assumed to be common knowledge between the interviewer and the interviewee the researcher can use this information and together with the interviewee reflect over central issues in the topic that are being discussed.

Conclusions

This chapter has discussed two main methodological challenges in studying civil society organisations: the delimitation of the research field and the access to and collection of data. The arguments have been based on the methodological insight that organisational properties affect the research method. The focus on civil society organisations is based on the fact that among these organisations we usually find a larger variety of organisational

forms than in the corporate and public sector. In particular we find more diffuse forms of organisation where the boundaries between an organisational level of action and an individual one are much more blurred. This does not mean that we do not find more structured, formal and professionalized forms of organisation in civil societies. However, the study of such organisations is more similar from a methodological point of view to the study of organisations from the other two social spheres, public and private corporate. It is also these organisations that are usually focused upon in textbooks of organisation theory and organisational research methodology. This chapter has focused on the methodological challenges in studying civil society organisations and some of these challenges are related to the concept "organisation". There are of course other theoretical perspectives that study collective actions that are not bound to such a concept. For example, applying a network perspective on immigrant organising makes it possible to distinguish both networks between organisations and interpersonal networks between people (Brass et al. 2004). Also by following networks of people and organisations through "snow-ball" sampling some of the challenges described in this chapter might be avoided. A focus on organising processes instead of organisations (cf. Czarniawska 2005) might be yet another alternative to the approaches described in this chapter.

Social movement research on the other hand has focused more on civil society organisations since these organisations are products of social movements. Social movement organisations as a product of the mobilisation of social groups have been highlighted by social movement research as has the interplay between organising and organisation (e.g. McCarthy & Zald 1977). The political opportunity structure theory that has been used in this chapter as an example of a research approach has also been developed in the study of social movements (e.g. McAdam et al. 1996). However, it has been argued (e.g. Bengtsson 2010) that many immigrant organisations are in fact not necessarily connected to social movements and are often multifunctional in the sense that, as many other interest-organisations, they are less political and ideological than social movements and the organisation they give birth to. The functions of immigrant organisations, which have been described and categorised in different ways in previous research (Bäck 1983, Rex & Josephides 1987, Emami 2003), often include functions that are more connected to the social or cultural sphere than to the political one. This is an important reason why theoretical approaches that have been developed in social movement theory should at least be adapted to the context of interest organisations before being applied to this research field (Bengtson 2007). In a similar way I would argue that the same applies to some of the methodological issues that are treated in organisation research and third sector research when focusing on organisational phenomena that are different from many smaller "we for us" organisations. The study of this kind of organisations requires, as I have tried to show in this chapter, a certain amount of flexibility in the methodological approach that resembles the way in which Buchanan and Bryman (2009:6) describe the study of organisational fields which are in constant change:

> Depending on the research topic, the flux and patterns of change become substantive data observations, and the role of the researcher may be to "catch reality in flight". In these kinds of rapidly changing organizational settings research methods must be regularly reviewed and adjusted in a flexible manner, as initial plans become inappropriate and as fresh lines of inquiry become apparent.

Being civil society, as such, not regulated by law, we will always find realities in this sector that are on the verge of being an organisation. Such realities, even if included under the same category (e.g. immigrant organisation) might be difficult to study using a standardised a priori defined method. The study of such organisational fields requires, as the quote above mentions, a flexible and reactive approach and, I would like to add, an eclectic approach towards the data collection. One of the constants in this kind of organisation is the involvement of one or more individuals who might be interviewed and/or observed. Thus the lack of formal organisational structure makes the interpretation of the data a sensitive issue. As one of my interviewees responded when asked about how anchored his opinions were in the organisation: "The organisations are not hierarchical in this way, non-profit organisations, so that there is always a range of views so you can ask a representative and you get some opinions and you ask another and you get some nuances, independently of background and a lot of other stuff so it is not the same as asking an executive director at a company."

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Ethnography on the Internet – an overview

Gabriella Scaramuzzino

Abstract

The aim of this chapter is twofold. Firstly, to review relevant online research and method literature and to present an overview of some of the methodological and ethical concerns which, for the purpose of this chapter, have been categorised in three themes: defining and interpreting empirical data on the Internet, creating a research field on the Internet and dilemmas and prospects when observing without being seen. Secondly, to contribute to the methodological and ethical discussions by using the author's research about a Swedish virtual red-light district as an example. The chapter shows that the Internet is an artificial arena with both textual and visual environments, which is in a constant process of change following cultural trends and technological innovation. This means that we can always expect the emergence of new methodological and ethical challenges and it is very important to continuously develop and discuss research on the Internet from a methodological and an ethical point of view. The knowledge we have about the Internet becomes, in fact, easily outdated compared to other arenas. The chapter's conclusions are that ethnographers online have only made limited use of the potentials that the Internet offers, that it is important to take account of which alternative techniques ethnographers online do not make use of and most importantly, why they are not using them.

Introduction

Internet usage continues to grow and the Internet is now an integral part of many people's daily lives. As a result of this expansion of the Net and the digitalisation of contemporary society ethnographers have started to use the Internet in order to study online communities and online cultures. Even though the Internet is quite a new research area for ethnographers the methodological development has come a long way. More and more researchers conduct ethnographic studies on the Internet and the amount of online research and

method literature to be published is continuously increasing. In fact, this is one of the fastest growing research fields (Silver 2003, Kozinets 2010). Going through what has been written I sense an air of anxiety because the Internet has been an untested research area, not having common standards and not attaining legitimacy. However, I also sense an air of desire to innovate the method, to create guidelines and to coin new terms. The number of terms used for describing ethnography on the Internet, and the fact that several different methodological approaches have been developed and used makes the method difficult to grasp. It is not easy to get a general view of the methodological and ethical concerns.

This chapter provides an overview of some of the most important methodological and ethical concerns discussed in ethnography on the Internet. The aim of this chapter is twofold. Firstly, to present an overview of relevant online research and method literature which I have categorised according to three themes: *defining and interpreting empirical data on the Internet*, *creating a research field on the Internet* and *dilemmas and prospects when observing without being seen*. Secondly, I wish to contribute to the discussions about methodological and ethical concerns by using my own research of a Swedish virtual red-light district as an example. Before moving on, I will present a brief background to ethnography on the Internet.

A brief background

Ethnography on the Internet has mainly been used in sociology and in the field of consumer and marketing research. Frontier-breaking studies have paid special attention to sex, deviance and consumer behaviour (Catterall & Maclaran 2001, Murthy 2008, Kozinets 2010). As discussed in the introduction, several different terms have been coined and used for describing ethnography on the Internet. Thus, to some extent, the terms represent diverse methodological approaches even if they are occasionally used as synonyms. To simplify, you can say that *online ethnography*, *cyber ethnography* (see e.g. Correll 1995, Fox & Roberts 1999, Catterall & Maclaran 2001) and *virtual ethnography* (see e.g. Hine 2003, Hine ed. 2005, Williams 2007) regard ethnography as an adaptable and sometimes also responsive method that does not have any fixed techniques. The studies are conducted in various ways and tend to follow, to a certain extent, the established tradition. *Digital ethnography* (see e.g. Murthy 2008) deals with the use of all kinds of new technologies, not necessarily on the Internet including, for instance, digital video.

Netnography (see e.g. Kozinets 1998, 2010,) however, is different. Netnography has been developed in marketing and consumer research but is now also widely used in other research fields. It is also a multi-method approach but developed as a distinct method with specific steps and procedural guidelines, inspired by cultural studies, anthropology and sociology. Common language and a citation base have been developed to make netnographic research more comparable, as well as to obtain consistency and greater recognition and legitimacy for scholars who conduct netnographic studies. Netnography is sometimes described as "promiscuous", because several methods can be used as a complement to this approach (Kozinets 2010).

Ethnographers on the Internet are generally especially interested in online communities and online cultures. These online communities often gather around a shared interest or identity such as cross-cultural weddings (see e.g. Nelson & Otnes 2005), TV-series like Buffy the Vampire Slayer (see e.g. Gatson & Zweerink 2004) and Star Trek (see e.g. Kozinets 2001), the purchasing of sexual services (see e.g. Blevins & Holt 2009, Sanders 2008, Di Nicola et al. eds. 2009), online games like Whyville (see e.g. Fields & Kafai 2010) and Second Life (see e.g. Bardzell & Odom 2008), skinheads (see e.g. Campbell 2006), lesbians (see e.g. Correll 1995) and gay men (see e.g. McLelland 2002). The focus is often on how identities are created and expressed, on social interaction, sexuality and learning processes.

The following section will deal with some of the challenges and concerns that many of these previous studies highlight. This discussion will be based, as mentioned above, on three themes: defining and interpreting empirical data on the Internet, creating a research field on the Internet and dilemmas and prospects when observing without being seen. I have found these themes to be constantly present in a more or less explicit way in the literature I have reviewed, despite the changing online environment and the different disciplines and research focuses which characterise this literature.

Defining and interpreting empirical data on the Internet

Discussions about what can be regarded as empirical data on the Internet and attempts at defining this data can be found in several empirical studies, articles and method books (see e.g. Kozinets 1998, Ward 1999, Fox & Roberts 1999, Catterall & Maclaran 2001, Hine 2003, Williams 2007, Murthy 2008, Mann & Stewart 2009, Munar 2010). As a point of departure, I will use my own empirical data from my thesis as an example and contribute to this on-going debate. In my Ph.D. project I observe, take "pictures" and perform content analyses of the interaction between the users on three different websites where people share an interest in prostitution and in purchasing sexual services. I study these websites as part of a Swedish "virtual red-light district". I use the term "virtual red-light district" as a metaphor. I see red-light districts as a social institution with historical roots in most western urban areas. In this sense the term can be used to understand if and how processes are transferred from an offline to an online setting. I will use this metaphor to exemplify through empirical data some of the methodological and ethical discussions in this chapter.

The purchasing and selling of sexual services occurs in various locations. Some of these transactions take place in so-called red-light districts. The term red-light district is used to describe physical areas that are linked to the sex industry (Hubbard & Whowell 2008). In these districts visitors can usually find sex clubs, strip clubs, sex shops and legal and/or illegal prostitution concentrated within a limited area. The districts are also known as places where different kinds of drugs are often available. Neon lights light up the districts at dawn. Often there are also trendy restaurants, nightclubs and designer shops within

the areas in order to attract people who are out to enjoy themselves. This means that red-light districts are not only meeting places for people who purchase or sell sexual services; they are also places where social workers hand out condoms and tests for HIV and other sexually transmitted diseases as well as provide welfare services and information. Various representatives of the authorities carry out social control over (primarily) female "sex workers".

At a first glance there are many similarities between a physical red-light district and a virtual red-light district, but the latter manifests itself in a slightly different way. Online there are no neon lights, instead there are so-called banners flashing from online stores that sell pornography, sex toys, potency pills, webcam services and sometimes drugs that have not yet been classified as illegal. In the virtual red-light district there are also traces of advertising from social workers who offer advice, support and prevention packages with condoms and information so escorts can increase their safety and reduce risks when selling sexual services. As a visitor, a 'flaneur', you can also see that there are advertisements for sexual services. The advertisements include photos, descriptions, information on body size, price, the sexual services offered and sometimes even video clips. This means that ethnographers online can study and gather a range of visual material like photographs, video clips, avatars[4], banners and other visual aspects of websites and use this material as a source of empirical data (see e.g. Kozinets 1998, Williams 2007, Murthy 2008, Munar 2010). It is also possible to use special computer programs like "Snagit", where a camera takes "pictures" of what is on the computer screen. By using this program the researcher can take pictures of the virtual red-light district, fully or in parts, edit the pictures and erase all personal and sensitive information before they are downloaded and stored.

Even if online ethnographers can use a range of visual data, most online communities and cultures exist in primarily textual online environments and ethnographers are particularly interested in the parts of websites in which users of the Internet interact and present themselves (Williams 2007, Munar 2010). The main empirical data therefore often consists of conversation threads and posts written by the members in an online community, in news groups, networking sites, online games, virtual pubs, fora or blogs but also on personal websites. In these threads users share information, give advice and support, flirt, joke, discuss, argue, etc. The ethnographer's own experiences of being a user on the Internet can also be used as ethnographic material (Hine 2003).

Despite the fact that new technology has been introduced and a variety of new types of visual data are available, text is still the main source of empirical data used by ethnographers online (e.g. see Kozinets 1998, 2010, Ward 1999, Fox & Roberts 1999, Williams 2007, Catterall & Maclaran 2001, Hine 2003, Mann & Stewart 2009, Munar 2010). The overview of the literature shows that the use of one's own experiences as ethnographic material is rare. We might expect a change in the future as visual data becomes more

4 Avatars are graphical presentations of the users online. They are often three-dimensional and are common in, for example, virtual worlds and online games. Avatars are sometimes also two-dimensional, like icons, and used by contributors in, for example, online communities or fora. They are also used as a form of presentation of the users, together with their usernames.

important. Hopefully, ethnographers online will use the various empirical data that are accessible and to greater extent continue to break new ground.

Accessibility of empirical data

One of the major possibilities with the Internet is what Robert Kozinets (1998) refers to as its accessibility. Ethnographers online can fairly quickly and easily copy, paste and download the interaction and communication in threads and posts like it was already exactly transcribed and use it in field notes (Kozinets 1998). Ethnographers online can do their research anywhere, anytime, and they can also follow interaction from the past because quite often these interactions and conversations are saved in archives on websites (Munar 2010). Thus, ethnographers online are not bound by time or space. The field is always there and it is possible to go back to the original situations during the research process. This accessibility of empirical data is discussed in several method books and articles (see e.g. Kozinets 1998, Catterall & Maclaran 2001, Hine 2003, Sveningsson et al. 2003, Daneback & Månsson 2008, Rokka 2010, Munar 2010).

This implies that online ethnographers can, to a relatively low cost and in a short time, gather a lot of empirical data, which can also impose a challenge since there can be too much data available and it can be difficult to handle. Because of the accessibility of data there are also many poorly conducted online studies (Kozinets 1998, Catterall & Maclaran 2001). This has opened up for discussions about what is and what is not ethnographic research online (see e.g. Hammersley 2006). Some point out that it is important for ethnographers online to create a context for the empirical data and to contextualize the text (see e.g. Rybas & Gajjala 2007). This problem, with many poorly conducted studies, is one of the reasons why Netnography has been developed and finally presented as a methodological approach with techniques and procedures upon which there is a consensual agreement.

3.3 Interactional clues as a help for interpreting text

A common notion is that the Internet is an arena where interaction and communication is disembodied and the focus in the literature is often on the lack of embodiment in primarily textual environments (Ward 1999, Whitty 2003). As already mentioned, online ethnographers mainly read text and communicate through text. Observers on the Internet do not get much contextual information in their empirical data and the text can be difficult to interpret. This implies that they cannot use interpersonal skills to the same extent and need more skills in dealing with text (Garcia et al. 2009).

Even though the physical body is not present on the Internet the body is nonetheless important and can be constructed in textual exchanges. Monica Whitty (2003), who discusses cyber-flirting and cyber-sex, argues that people tend to describe themselves and that the body can be translated through text. Users on the Net can for example describe how they look and in this way create attractive bodies. “Emoticons” like smileys are often used in textual exchanges as a substitute for body language and to show emotions and feelings in text (see e.g. Correll 1995, Whitty 2003, Campbell 2006). Sometimes the emoticons also have sounds, text or moves. Users also tend to use acronyms like “LOL”

(laughing out loud) and "hehehe" (laughter). When someone writes with CAPITALS it can often be interpreted as someone who is shouting out loud (Whitty 2003). Below is an example where two contributors interact through text and use emoticons:

> **A contributor in a couple who sells sexual services**: After this summer and our summer jobs we thought that we would slowly start with our sexual activities and business again tomorrow with a threesome ooh how I long for it…
> **A contributor who purchases sexual services**: If I only had that to look forward to  .

Ethnographers online who mainly observe interaction through text and conduct their studies in textual online environments need to consider and include in their analysis the use of descriptions, emoticons, acronyms, capitals and dots to visualize the "presence" and the field (see e.g. Correll 1995, Whitty 2003, Williams 2007). How, for example, are we going to interpret the interaction above? I interpret emoticons, acronyms, capitals and dots as "interactional clues" which are essential for ethnographers online because emotions and the body are not expressed in the same way through text. Irony and what is to be read between the lines can also be difficult to grasp. As an ethnographer online it is essential to develop skills for the interpretation of these interactional clues. Consequently, they can be used as "interpretive puzzles" (Garcia et al. 2009:61) in the research.

However, the Internet is constantly changing and with new technology graphical online environments, in 3D, with avatars like in the games Cyberworlds and Second Life (Williams 2007, Bardzell & Odom 2008) are becoming more and more common. The users use visual representations and avatars which offer a new possibility to communicate and interact and which provide the ethnographer online with more interactional clues. The users can make gestures while writing: they can walk, sleep, hang out and have fetish sex (Bardzell & Odom 2008) with other avatars. Ethnographers online are thus not only restricted to text, even if this is the main way in which users communicate and interact in this type of environment too. In graphical online environments the users recognize each other, not only by what their avatar looks like, but also by how they write and use interactional clues in textual online environments. New dimensions are added and the ethnographers online and contributors do not have to visualise and interpret as much. It is all there: streets, buildings, bedrooms and so on (Williams 2007).

With new technology, webcams have been introduced. It is now possible to actually see bodies and body language online. Hybrid spaces are offered that resemble neither purely online nor offline settings (Ward 1999). To a greater extent the Internet is starting to look like and be like an offline environment (see also Garcia et al. 2009) or something in-between, a hybrid, not bounded by time or space. This demonstrates how the Internet as an arena is changing and how new possibilities of interaction and communication are continuously being set up.

Creating a research field on the Internet

One important discussion found in the literature is how the research field is created on the Internet and some of the challenges to be faced when doing this (see e.g. Flick 2006, Hine 2003, Williams 2007, Murthy 2008). A research field is never something just "out there" and neutral. It is always created through deciding and constructing its boundaries.

Deciding its boundaries

Researchers (not only ethnographers) always choose what to include and exclude in the field setting, both through its boundaries and through the definition of the study object and context. These boundaries also affect the interactional patterns that the researchers are able to see (Murthy 2008, Sjöberg 2011).

This means that I have also constructed my research field in my thesis and its boundaries. There are processes of, communication, socialization and interaction that are not visible in my study. I examine websites as part of a Swedish "virtual red-light district" but I only observe the interaction that is visible to me and the parts of the virtual red-light district that do not require any registration. I have not studied all websites that can be linked together as a Swedish virtual red-light district. To define such a district is not only difficult, but impossible. This is because it is a concept and an abstraction that I use and an "illusion" created by me. I could choose to include other websites and exclude the one I have in the "virtual red-light district" and might for example have seen other interactional patterns. On the other hand, if the study would have been in a physical setting the ethnographer would still have to choose which streets to observe and would still have to face the same dilemma. The point is that the definition of a research field through its boundaries needs to be reflected upon in general but when we are addressing a non-physical environment we face some specific challenges. What do "boundaries" mean on the Internet and should we focus on specific websites or follow connections?

Visiting specific websites or following connections

There is a general critique that ethnographers online, like those in the established tradition, have a tendency to make the research field seem very homogeneous and they tend to choose to study bounded settings like, for example, one or several specific websites. Christine Hine (2003) has contributed the most to this discussion and she criticises this tendency because it may lead to missing out on other ways of understanding people and cultures. She suggests that "…the field site of ethnography could become a field flow, which is organized around tracing connections rather than about location in a singular bounded site" (Hine 2003:61). Instead of using one single website, Hine (2003) suggests that online ethnographers choose a more connective approach which is based on connections, differences and heterogeneity. Instead of focusing on "places" like websites, online ethnographers can, for example, see their research field as a field of relations.

The online ethnographers might still have to start from a particular website but could then follow connections like different links and explore their contents and see how they are organized and these connections can be seen as an invitation to carry on the study. Consequently they can decide to follow people, things, links, narratives, cultures etc. (Hine 2003). Matthew Williams (2007), for example, discusses how it is possible to "travel" with avatars both within and outside Cyberworlds. Following avatars can be useful if the research interest is that of online experiences. However, following individual avatars as a way of creating the research field can be both difficult and problematic if the research interest is the Internet as an arena. Following individual avatars only gives a narrow and partial picture of what is going on in the online terrain and can also make it tricky to know and decide where the research field begins and where it ends. Therefore Williams (2007) preferred to define his boundaries by choosing a website as his research field.

Hine (2003) argues that the Internet has opened up for several opportunities to show heterogeneity and to create the research field in new ways. She argues further that the whole concept of the field site becomes challenged once online because ethnographers can choose to create their research field in numerous innovating ways. I find the argument that such a concept should be especially challenged online questionable. The Internet might create new opportunities, but as the overview of the ethnographic studies shows, many ethnographers online prefer to create their research field by choosing one or more websites. Thus, as will be discussed below, this is probably changing, following the present social trend that the digital and the physical realities, online and offline, become more and more intertwined especially for the newer generations.

Online and offline

Ethnographers do not have to choose between creating their research field either online or offline. Recent literature has started to discuss that even if some social phenomena only exist on the Internet it is getting more problematic to justify that ethnographers online limit their research field to online settings as I have done in my thesis (Garcia et al. 2009, Kozinets 2010). The notion of a separation between online and offline is being questioned. The digital and the physical realities are both important parts of people's everyday lives. These realities cannot easily be separated if we want to reach a greater understanding of social worlds and cultures in contemporary society (Miller & Slater 2000, Williams 2007, Munar 2010, Kozinets 2010). The literature (see e.g. Garcia et al. 2009, Kozinets 2010) argues that we can expect some changes to be relevant also for ethnographers who solely study offline settings as they will face more difficulties in not including the Internet in their research fields. One significant question is what the consequences are that these shifts and trends will have for ethnography in the future. If the notion of a separation in social life between online and offline is being questioned then the division between online and offline ethnography can also be discussed.

Some of the methodological dilemmas in ethnography online are connected to ethical principles and especially the role of the researcher vis-à-vis the other users in a setting where the body often stays offline while the researcher goes online.

To be a lurker on the Internet

On the Internet it is possible to be a so-called "lurker" (to observe without being seen and without participating or contributing within, for example, the online community). All ethnographies online involve some form of observation (see e.g. Ward 1999). Many ethnographers online, at least initially lurk when they conduct their studies in order to do a so-called "cultural entrée" (Kozinets 1998, 2010). By lurking before participating it is possible to gain knowledge about the online community and its culture and norms and learn more about its specific language. Online ethnographers can develop a strategy for their entry by gathering information about which contributors are best to contact and which are the most frequent contributors etc. (see e.g. Catterall & Maclaran 2001, Gatson & Zweerink 2004). Thus, some ethnographers online never move from lurking to participating as it is possible to gather all empirical data while remaining invisible.

The Internet makes it possible to make detailed observations and to be "a fly on the wall" without an invitation into the communities and without help from key informants and gatekeepers (see e.g. Daneback & Månsson 2008). In fact, many websites are created so visitors can lurk. This is not possible in the same way in graphical online environments like Cyberworlds where every user must use an avatar. If they observe without interacting through text they will still be visible and present in the same way as offline. In textual online environments, as those considered in my study, ethnographers are instead limited to observing the contributors' communications, interactions and self- representations and cannot observe users who are exclusively watching without interacting through text (Williams 2007). Further they have no, or only little, knowledge of the interaction that is occurring in "places" that demand some kind of access.

In my study, I only observe the interaction that is visible to me and the parts of the virtual red-light district that do not require registration and that I consider public. I am not dependent on informants giving me access to the field. One possibility that I see, and that is not given so much attention in the reviewed literature, is that on the Internet ethnographers can study and maybe even observe people and groups who can be difficult to find offline, like people who purchase sexual services. Ethnographers online can also explore sex and deviance in new ways because of the opportunity to observe without being seen and without engaging themselves (see e.g. Ashford 2009). The possibility of being anonymous in the interaction contributes to the formation of communities that only exist online (Kozinets 1998), especially deviant subcultures (Blevins & Holt 2009).

This is one of the points where ethnography on the Internet is clearly divided into two positions. Netnography is a participative approach, not recognizing unseen observation as a viable alternative because interpretations based on such techniques are considered to be questionable. Furthermore, observers who are not participating are not considered capable of obtaining embedded understanding of the online community and online culture. However, some researchers from the netnographic tradition have created a "passive" form of Netnography that includes the possibility of "lurking" in the method (Kozinets 2010). To observe without being seen is a technique that is sometimes, in my opinion, too easily dismissed. It can provide important knowledge about people, groups and phenomena that

we know little about although there are, nonetheless, some ethical dilemmas that need to be addressed.

The methodological and ethical dilemmas of unseen observation

There is no consensus if and when it is acceptable to observe users without being seen and to use texts and visual data without asking for informed consent. Much has been written about the ethical dilemmas in lurking (see e.g. King 1996, Fox & Roberts 1999, Sveningsson et al. 2003, Sanders 2005, Daneback & Månsson 2008, Mann & Stewart 2009, Ashford 2009, Rokka 2010, Munar 2010) and we find arguments both in favour of, and against, the choice of unseen observation and consequently observation without asking for informed consent.

The overview of the literature shows that questions about ethics on the Internet are complicated. For instance, is the Internet a public or a private arena? The interactions that online ethnographers observe can be considered as occurring in both a public and a private space and the line between these spaces is often blurred. The contributors might feel that they are interacting in privacy and that their anonymity can be presumed. People on the Net tend also to write about private matters publicly (see e.g. Sandred & Engström 1999, Fox & Roberts 1999, Catterall & Maclaran 2001, Elgesem 2002, Gatson & Zweerink 2004, Flicker et al. 2004, Daneback & Månsson 2008, Murthy 2008, Williams 2007, Garcia et al. 2009, Munar 2010). Thus, in the reviewed literature, correspondences and private messages are often considered private whereas message boards and blogs are considered public. However, there is no easy way to take a stand in this dilemma but all agree to the importance of protecting groups that are marginalized and vulnerable (Murthy 2008).

Most scholars also seem to agree on the fact that doing research on the Internet is not the same as doing research offline. Therefore ethical guidelines used offline cannot fully be used on the Internet. The Internet as an artificial world is constantly changing and there is a need to evaluate ethical guidelines constantly. Guidelines and knowledge produced about the Internet easily becomes outdated compared to other arenas. New possibilities of interaction, like for example, the expansion of online graphical environments and the use of avatars, raise new ethical considerations (Williams 2007). Nonetheless, unseen observation also offers some possibilities for studying situations in a more "natural setting".

Observing naturally occurring situations

At the beginning of my ethnographic studies on the Internet my plan was to study exclusively the interaction between users who purchase sexual services and afterwards to do a cultural entrée and then interview them. My study ended up quite differently from what I had planned. I started to be curious about the interaction and communication between all users who contributed publicly in the virtual red-light district. After a while I decided to stay on the Internet because the interaction, as it occurred on the websites was interesting in itself.

As already mentioned, there is a strong expectation that the researcher participates in the culture or online community being studied, especially in Netnography (see e.g. Miller

& Slater 2000, Kozinets 2010). Existing ethical guidelines recommend the researcher to be visible rather than invisible (Kozinets 1998, Murthy 2008). However, as David Silverman (2007) suggests, using the opportunity to observe without being seen makes it possible to observe actual conversations and situations as they occur naturally. Ethnographers online who observe without being seen have the prospect of gathering what Silverman refers to as "untouched data" and he emphasizes the importance of observing naturally occurring situations instead of automatically starting to conduct interviews and using manufactured data in the way that I had initially planned. Blevins and Holt (2009) argue that not interacting with the contributors reduces the risk of bias and contamination (see also Fox & Roberts 1999, Sanders 2005). Nonetheless, it is the researcher who chooses what to include and exclude and how to interpret and analyse the situations (cf. Silverman 2007).

In this section I have shown how both prospects and dilemmas of unseen observation have been debated in ethnography on the Internet. I argue that instead of automatically dismissing unseen observation as a technique, the purpose of the research should be considered and should guide the ethnographer. The potential harm should also be weighed against the relevance of the knowledge gained.

Conclusions

In this chapter I have reviewed method literature and relevant empirical studies and presented an overview of some important methodological and ethical concerns that online ethnographers may face. I have categorized these concerns in three themes: defining and interpreting empirical data on the Internet, creating a research field on the Internet and dilemmas and prospects when observing without being seen. I have also used my own experiences from my thesis as examples in this chapter.

The overview of the literature shows that the common notion of the Internet as an arena with disembodied interaction has, in recent studies, been questioned. With graphical environments, avatars and new technology such as webcams, interaction and communication online is starting to resemble that of the offline environment but without limits of time and space. The question is, what will this mean for ethnography? So far, most online ethnographers only use a small part of the empirical data accessible on the Internet and have not been eager to incorporate visual data and their own experiences of being a user in their studies. Textual environments, and interaction and communication through text are still the preferred empirical data. This overview suggests that one of the major concerns in online ethnography is how to interpret text. Ethnographic studies reviewed show that advanced techniques have been developed to make use of "interactional clues" like emoticons and acronyms and are used as pieces in an interpretive puzzle. We might expect a change, a greater variety of empirical data being used and a continued development of the methodological approaches used in ethnography on the Internet.

Furthermore, the overview of the literature shows that the common notion of a distinct separation between the digital and the physical realities has in recent studies also been questioned. Offline and online are becoming more and more intertwined, particularly for

the newer generations. As a result, online ethnographers have started to question whether or not it is reasonable to create a research field solely online or offline. It can further be discussed whether or not the upholding of a distinction between offline and online ethnography will be useful in the near future. Online ethnographers have not really started to create their research field in new ways by, for example, following connections or relations even if it is possible to see some changes in practice. Instead, most studies reviewed have created their research field by choosing one or several websites.

Ethnography on the Internet is characterised by being a new research area and a method on the rise. The overview of the literature shows that there is a struggle for gaining recognition and legitimacy for the different approaches. This is most evident in Netnography. The unseen observation of online communities has been raised as a controversial issue. I interpret this as part of the struggle for recognition and legitimacy where the "air of anxiety" that I sensed in the literature comes to the surface. As a major concern for ethnographers this dilemma and its prospects have been heavily debated. My interpretation is that this fairly strong striving for recognition causes insecurity in the use, not only of the "lurker" position, but of a whole range of available options. Another hypothesis is that we just do not know how to engage and approach some of the more recent developments in the online environment.

The methodological and ethical concerns that are discussed in ethnography on the Internet are about what ethnographers online do, and issues that they face when conducting their studies. Because of the nature of the Internet some of these methodological and ethical discussions also have a tendency to quickly be outdated. This chapter shows that ethnographers online have not fully used the potentials that the Internet offers; on the contrary they have only used a small portion of the possibilities. I suggest that it is important to add to the methodological and ethical concerns, the question of which alternative techniques ethnographers online do not use, and most importantly, why they are not using them.

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Doing qualitative interviews over the telephone

Lotta Jägervi

Abstract

The purpose of this chapter is to investigate the function telephone interviews could fill in qualitative research. Based on previous research on the subject and on the author's experience, the common notion that telephone interviewing is an inferior method is investigated and challenged. The most common methodological objections to telephone interviewing and the scientific ideals behind them are discussed. The specific conditions of telephone interviewing are argued to involve not only potential problems but also potential gains, depending on which measures of quality are given priority.

Introduction

Most researchers writing about qualitative telephone interviewing relate to the common notion that telephone interviewing has obvious disadvantages compared to face-to-face interviewing, and is a procedure more suitable for quantitative or highly structured interviews (cf. Holt, 2010; Novick, 2008; Stephens, 2007; Sturges & Hanrahan, 2004). In this chapter, I will discuss the advantages and disadvantages of using telephone interviews in qualitative research with my own experience as a frame of reference. Three questions will be investigated: 1) Which are the arguments against using telephone interviews in qualitative research that are made in the literature? 2) Which are the scientific ideals behind the critique? 3) What function(s) could telephone interviews fill in qualitative research? I will go through the main points of critique against telephone interviewing in the literature and discuss them in relation to my own experience. As a background, I will first give a short description of my research project and of how the literature review was conducted.

The role of telephone interviewing in my research

I am in the process of conducting telephone interviews with people who have been in contact with two Swedish victim support organizations in order to ask them about their experiences with Victim Support. I have two main areas of interest: on the one hand, the needs of crime victims and how well these needs are met and, on the other hand, how the crime victims perceive the actual and expected role of the helper. Most of my interviewees have had a fairly short telephone contact with Victim Support; between one and three telephone calls. This was expected since most Victim Support contacts are of this length (Svensson, 2006) and I have tried to get a sample that mirrors the variety of contacts with Victim Support as well as possible, the only selection criterion being that all interviewees should be over 18 years of age. The interviews were thus expected to be rather short, and since I wanted to get an overview of the variety of experiences with Victim Support, I have strived to do 30–40 interviews. Thus far, I have done 28 interviews that lasted 5–20 minutes each. In addition to providing a broad material, the telephone interviews function as a screening to find informants for longer face-to-face interviews, avoiding the potential bias of finding informants on recommendation from Victim Support.

Conducting the literature overview

The literature on telephone interviewing in qualitative research is rather scarce. In my literature review, I have used and combined the terms "telephone", "interviewing", "qualitative interviews", "qualitative research" and "research note" in LibHub (the search engine at Lund University Library) and the databases SocIndex and PsycInfo. Many of the articles found were written by researchers in nursing studies and because of that, I also searched the medical databases PubMed and Medline. Finally, I used the references in the articles that were found in the databases. In this chapter, I will use all articles on telephone interviewing in qualitative research that I have been able to access in full text. I will also use some articles on doing telephone surveys that address questions that could be relevant for qualitative research.

Critique against telephone interviewing

Novick (2008) argues that there is a bias against telephone interviewing in qualitative research, while it is frequently used and researched in quantitative research. Since my own literature search yielded an extensive amount of research on quantitative surveys and less material on telephone interviewing in qualitative research, I am inclined to agree with her at least on the last point. Although many contest them, two major objections towards material collected through telephone interviewing reoccur in the literature: Data derived from telephone interviewing is less rich and data derived from telephone interviewing is less representative of reality. There are also a number of practical issues that can be regarded as possible reasons for the lack of richness and/or representativeness.

"Data derived from telephone interviewing is less rich"

That data derived from telephone interviewing is less rich than data from face-to-face interviews seems to be the main objection against the method. Richness is a rather broad concept but in this context at least it concerns both the actual quantity and the variety and depth (an equally difficult concept that I will return to in later sections) of the data. Richness is a rather uncontested measure of quality in the literature, and there is some discussion as to whether or not sufficiently rich interview data can be collected over the telephone. Novick (2008) shows that although some researchers have found that richness of data is reduced in telephone interviewing, others have found few differences. She argues that the most reasonable interpretation is that telephone interviewing requires certain skills, rather than that the method is suitable only for short and structured interviews; the latter is claimed in most textbooks she cites. Smith (2005) argues that the time span for efficient interviewing is shorter over the telephone than face-to-face and thus recommends telephone interviewing only for interviews with a clear and rather narrow focus. However, many are fairly optimistic about the possibilities of collecting rich verbal data. Struges and Hanrahan (2004) compared both quantity and content between data from face-to-face and telephone interviews using the same interview guide and found few systematic differences. However, McCoyd and Kerson (2006) found some differences favouring face-to-face interviewing in both depth and length when making a similar comparison. Holt (2010) and Stephens (2007) do not compare material from face-to-face and telephone interviews systematically, but both argue that the richness of the material they have collected by telephone interviewing is more than adequate. In addition, Holt argues that telephone interviewing actually provides the researcher with a richer transcribed material as the limitations in non-verbal communication are often compensated verbally.

"Data derived from telephone interviews is less representative of reality"

The objection that data collected from telephone interviewing is less representative of reality is most often found in the literature on telephone surveys in quantitative research. Here, correspondence between answers and outer reality is most often the main object of methodological discussions (Bexelius et al., 2009; Baron-Epel, Haviv-Messika, Green & Nitzan Kalutzki, 2004; Wilson, Roe & Wright, 1998). Since most questions concern behaviour, validity issues mainly concern whether or not the interviewees are telling the truth about their behaviour. Although this is not directly transferable to qualitative methodology, similar issues are mentioned in writings on telephone interviewing in qualitative research, mainly in nursing studies. One objection towards telephone interviewing that reoccurs in Novick's (2008) overview is the risk of distortion of data. Distortion could be caused by misunderstandings or deception but the result in both cases is that the researcher cannot access the true opinions or feelings of the interviewee. This problem is often connected to the ambition to affect the answers as little as possible when interviewing. In an overview of the literature on the use of semi-structured telephone interviews in health care research, Smith (2005) discusses whether small but systematic differences between the results in

telephone and face-to-face interviews could mean that the validity is compromised in one or both methods. She does not define validity, but interviewer bias is the most discussed threat. Burke and Miller (2001) also argue that the risk of biasing the interviewee could be increased when interviewing over the telephone since more verbal encouragement is needed. While interviewer bias is a problem closely connected to representativeness as a measure of quality, there are a number of practical issues that are not clearly connected to specific ideals. Before going further into the discussion about quality measures, I will present the most common practical objections towards using telephone interviews in qualitative research.

"Contextual data is lost in telephone interviews"

The richness and/or representativeness of data is argued to be compromised in telephone interviewing for a number of reasons. A common objection is that contextual data is inevitably lost. Even the most passionate advocates of telephone interviewing seem to agree on this. When the term contextual data is used, it mainly refers to information about the physical environment of the interviewee during the interview. Some argue that the inability to assess and/or control the environment of the interviewee could cause some methodological problems, mainly distractions that could affect the interviewee but may not be noticed by the researcher (Opdenakker, 2006; Stephens, 2007; McCoyd & Kerson, 2006). Novick (2008) addresses this objection by arguing that most researchers doing face-to-face interviews work primarily with recordings and transcripts and thus do not use this kind of data anyway. Genovese (2004) chooses a different angle and argues that a sensitive interviewer can access background information without vision. She gives many vivid examples of how the contextual and non-verbal information comes through by background sounds and tone of voice. I would like to add that telephone interviewing can provide not only some of the background information that can be found when interviewing face-to-face, but also some additional forms of contextual data that would more easily have been missed in a personal meeting. In my own research, telephone interviewing means that the data is collected in a way that is similar to the situation I am interviewing about, since most contacts with Victim Support are phone calls. I could thus get some extra information about how my interviewees might react to the context of the encounter with Victim Support from their reaction to the context of the interview. Although there is of course a need to be careful with over-interpreting, an interviewee having difficulties finding a private space to talk and saying that that is usually the case, or telling me that I need to speak clearer as she has difficulties hearing over the phone, is probably providing me with some information about the conditions of the talk with Victim Support that I would have missed if I had talked to her face-to-face.

"It is difficult to establish contact over the telephone"

Another common objection is that it is more difficult to establish contact over the telephone. Stephens (2007) describes the matter of contact as the main difficulty in telephone

interviewing and argues that the loss of non-verbal data sometimes makes the conversation less smooth, with more unintentional interruptions and a need for all communication to be very clear and explicit. Holt (2010) asked her interviewees to evaluate the telephone interviews and though most comments were positive, some stated that more cues to keep on talking would have been nice. I recognize this from my own interviewing, especially in the first interviews when I sometimes needed a few seconds to understand why my energetic nodding did not seem to have much effect. In comparison to Holt (2010) and Stephens (2007), I have taken on yet another difficulty in making the conversation run smoothly, since I do not record my interviews but take notes during them. This sometimes makes me slow in responding and though most interviewees seem to be content after an initial explanation, some seem uncomfortable with the lack of affirmation. Balancing the quality of the interview and the quality of the notes is then a rather difficult issue and I have prioritized contact. I take more comprehensive notes directly after all interviews and since the interviews are rather short, I believe that I get fairly good accounts in this way. Recording would probably have been preferable though, since some types of analysis are difficult to perform on the basis of notes. This implies a loss of data that might be just as severe as the risk of interviewees excluding information, and relates to the issue of creating trust, which will be discussed next.

"It is difficult to create a trusting atmosphere when telephone interviewing"

In Novick's (2008) literature review, as well as in Struges and Hanrahan's (2004), two contesting ideas are present: Telephone interviews have the disadvantage of creating a less personal and trusting context and telephone interviews have the advantage of making interviewees less visible, making it easier for them to open up. McCoyd and Kerson (2006) and Stephens (2007) argue that one advantage of telephone interviews is that the interviewees can control their own context, which might make them feel more comfortable. Musselwhite, Cuff, McGregor and King (2006) and Opdenakker (2006) argue that the increased anonymity can have the same effect. Holt (2010) also points out that many of her interviewees, often being single mothers with previous contacts with social services, were more comfortable with telephone interviews since home visits were associated with being assessed and judged.

In spite of this, I believe that my own research has suffered from difficulties in establishing trust early enough. I asked my fist potential interviewees for permission to record, but all declined and some withdrew or were very close to withdrawing from the interview. I thus stopped asking to avoid losing some informants completely. Burnard (1994) argues, contrary to my experience, that most people who agree to being interviewed by telephone also agree to recording. He recommends making small talk before starting the interview and though I have no objections in theory, I have found it difficult to perform in practice. When I conducted face-to-face focus groups, small talk came naturally, but when calling a stranger on the telephone, it seems to be a less obvious thing to do. The only time small talk has come naturally is when we have had difficulties finding a good time to do

the interview. With the few interviewees with whom appointment booking has become a problem, I have been transformed during the process from a fairly anonymous Ph.D. student to a woman who is married to a sailor and has a three-year-old who most often goes to sleep at about eight o'clock. Trying to make contact, I have found that most people seem comfortable chatting with the latter but not with the former. My interviews usually take about ten minutes and are fairly small undertakings. Even though it is a semi-structured interview with few and open-ended questions, it is probably more similar to a survey than to an interview that takes several hours, at least from the interviewee's perspective. The informal talk concerning appointment booking and other practical issues would probably be less of an exception when conducting longer interviews.

"Interviewees are less committed to telephone interviews"

In establishing contact, I face issues that might be more typical for quantitative research. This seems to reoccur through the interview, leading us to the last objection: Interviewees are less committed to telephone interviews. A problem that I have encountered is that interviewees choose times to do the interview that may not be optimal from my perspective, for example when appointments after the interview or too many disturbances seem to make them want to rush through the questions. Holt (2010), Stephens (2007) and Sturges and Hanrahan (2004) all mention and dismiss this problem. In their experience, there is little difference between booking telephone and face-to-face interviews. Holt (2010) even argues that she has experienced less stress and disturbances when telephone interviewing, since it is easier to re-schedule if the time turns out to be less appropriate than expected. However, our interviews are somewhat different, not only in length. Holt (2010) interviewed parents about their children's involvement in the youth justice system whereas Sturges and Hanrahan (2004) and Stephens (2007) interviewed people about aspects of their work; both are topics that could be expected to be relevant for the interviewees, though probably differing in emotional importance. In contrast, many of my interviewees are initially sceptical, not so much to me or my research but to their own ability to contribute to it. They tell me that they have only talked to Victim Support once or a few times and do not really have anything to say about it. Telling them that this is the case for most of my informants and that the interviews are over the telephone and fairly short usually changes their minds. However, it may also influence their expectations of the interview so that they become even less inclined to give me an extensive account.

Burnard (1994) addresses the problem of commitment and appointment booking and recommends doing telephone interviews in the evening, since interviewing in the workplace often makes interviewees want to rush though the questions. Although this sounds reasonable enough, once again I have had difficulties implementing his recommendations in practice. When doing the first set of interviews I usually called my interviewees during working hours for practical reasons and though I suggested that we book an appointment, some preferred to do the interview right away. Many also suggested a time during working hours. Some were pensioners or worked irregular hours but I have also experienced the very problem Burnard points to, having people rushing through the questions during a coffee-

break. Although I started giving a slightly exaggerated account of how long the interviews usually are after experiencing this, I was reluctant to suggest a time myself since letting the interviewee decide seemed more polite. At the moment, I am doing my second set of interviews with the help of a different victim support organization and since I need to adapt to who is willing to help me with the contacts and in what way, I am now dependent on having the interviewees contact me. They often call for the first time on an occasion when they find it convenient to be interviewed and I try to meet this wish if possible. Notably, I have experienced much fewer disturbances from my informants' surroundings during these interviews. As mentioned above, Holt (2010) argues that an advantage of telephone interviewing is that interviewees are more able to control the time of the interview. This could also be an advantage with having my interviewees calling me; they can take control over the time completely, and the risk of unexpected disturbances or shortage of time is thus much lower.

What are the scientific ideals behind the critique?

In the literature on telephone interviewing in qualitative research, the main focus is on practical issues. Two parallel discussions are held, one about whether or not the potential problems presented above occur, and one about whether or not they are relevant for the quality of the interview material. The question of whether practical problems are likely to occur when telephone interviews are used is an empirical one. The research in the area is disparate and the question can probably not be given a simple yes or no answer but rather depends on a number of circumstances. The question of whether the practical problems actually are problems in the sense that they compromise the quality of the research is more complicated, as it requires that we define quality. As we have seen, richness is the predominant criterion in the literature, but representativeness is also discussed. Although these criteria are sometimes combined, they represent quite different ways of thinking about the research process.

What is a good interview?

Most of the articles I have found are focused on practical issues and do not go into epistemological questions at any great length. Loss of data and problems with making the interviewee feel comfortable and engaged in the interview could of course affect both richness and representativeness, and the problems discussed do not always give any clues to the authors' scientific ideals. However, some do account for their views on representation of reality as a criterion of quality, briefly or indirectly. Many researchers, especially within nursing studies, mention the ambition to access data that are as uncontaminated as possible (Burke & Miller, 2001; Wilson & Wright, 1994; Smith, 2005). Interviewer bias is discussed as a potential problem and interviewing is often referred to as obtaining or collecting information. In my literature review, this seems to be the most common orientation, although it is often addressed more indirectly than in the articles referred to above. For

example, similarity in the content of the material collected in different interview modes can be discussed as a sign that the quality is not compromised by the choice of methods (Struges & Hanrahan, 2004). Methods can also be discussed in terms of how well they produce honest responses (Carr, 1999) or avoid misleading information (Novick, 2008). However, some authors stand out from the crowd. Stephens (2007) takes the stand that all interviewing is by nature interpersonal and contextual, thus discarding the ambition to interview without affecting or biasing the interviewee. Holt (2010), doing a narrative analysis, takes a similar stance in that she focuses on identity-making both outside and inside the interview context. Neither Stephens nor Holt discusses the material in terms of how well it corresponds to any firm reality. As I will return to later, their methodological discussion also stands out somewhat since they do not focus on comparing the material derived from telephone and face-to-face interviewing as a product, but rather discuss all parts of the interviewing process as equally important.

While representation of reality is a controversial criterion of quality, richness seems to be generally accepted. Richness is, however, quite difficult to measure and less extensively discussed in articles focused on measuring the quality of the material as a final product. Many only mention a less rich material as a possible consequence of experienced practical problems. Nevertheless, there are two studies that take on the challenge of comparing richness between transcripts from face-to-face interviews and telephone interviews using the same interview guide. Struges and Hanrahan (2004) used two measures of richness: the length of the answer to each question and the depth of the answer to each question. Although they give examples of very similar-sounding answers they do not define depth, nor do they discuss how it is assessed. McCoyd and Kerson (2006) compared material from three modes of data collection, face-to-face, telephone and e-mail interviews, with regards to richness, among other things. In their study, richness is operationalized as the length and depth of the entire interview. Depth, in turn, is operationalized as the frequency of emotional cues. Holt (2010) discusses the quality of her material in somewhat different terms. She argues that the lack of visual cues in telephone interviews makes the verbal communication, and thus the transcripts, more rich. As I understand it, richness here means quantity, and possibly variation, as some things that are usually not expressed verbally now find their way into the transcripts. However, Holt distances herself somewhat from the notion that more material is always better material. Much of her discussion concerns the experience of the interviewees rather than the quality of the material, and when the material is discussed, taking the perspective of the interviewee is stressed as important. From this point of view, Holt argues that telephone interviewing makes the researcher more dependent on the interpretations of the interviewee and that this is actually a good thing. While interviewing face-to-face allows the researcher to look at and interpret the interviewee's appearance and sometimes everyday surroundings, telephone interviewing gives the interviewees more space to present themselves from their own point of view.

To sum up, I see two conflicting ideals for interview data appearing in the literature. On the one hand, we have the view that good material is a good representation of reality. This can be true for accounts of behaviour or events or, more often, for the more subjective reality of the interviewee's true feelings and/or opinions. From this point of view, richness

is a secondary but important measure of quality and stands for quantity, detail and the somewhat evasive concept of depth. New information adds to the picture and the more the researcher knows, the closer s/he gets to the truth. Consequently, misleading or biased information is considered a threat, no matter how rich it may be. On the other hand, we have the view that interviewing is a process where the material collected cannot easily be assessed on its own but is seen as context bound. Since the interview is viewed as inevitably co-constructed between interviewer and interviewee, correspondence with reality loses its relevance as a measure of quality. Richness can still be important but is discussed somewhat differently. More and/or different data can lead to a different and more complex picture but not necessarily a truer one, as all information is viewed as subjective.

Practical consequences

In the beginning of this chapter, I presented a number of common practical objections made against telephone interviewing. None of them relate in a simple manner to one view of how quality of data should be assessed, but both their relevance and the reasons for considering them could differ depending on what perspective is taken. Starting with the objection that contextual data is lost, how big the problem is depends on if and how contextual data is used. Opdenakker (2006) and McCoyd and Kerson (2006) both point to the problem that the environment of the interviewee cannot be standardized in a telephone interview. The ambition to standardize is closely related to the ambition to avoid bias, thus mainly relevant if interview data is viewed as being representative of reality rather than constructed. On a similar note, Novick (2008) discusses whether or not the loss of contextual data makes it more difficult to detect misleading information. Most commonly, however, the loss of contextual data is viewed as problematic mainly because it makes smooth communication more difficult, leading us to the next two objections, that it is more difficult to establish contact and create a trusting atmosphere over the telephone.

Holt (2010) and Stephens (2007) are the only authors in the literature I have found who discuss contact to any great length. Although contact could be argued to be important from any perspective, a view on the interview as interpersonal and contextual may make the process of interviewing a more important object of discussion. In contrast, trust is discussed by most authors. Holt and Stephens both use contact and trust almost synonymously, as signs of a positive relationship that is discussed both in its own right and in relationship to the quality of the material in terms of richness. However, trust can also be used as an indicator of the interviewee's inclination to reveal sensitive information in a truthful way (McCoyd & Kerson 2006; Musselwhite et al. 2006; Opdenakker, 2006). Here, trust is discussed in relation to both richness and correspondence to reality as measures of high quality data, but the reflections on the relationship between interviewer and interviewee end there. The issue of commitment falls rather close to those of contact and trust but it is discussed in a much more homogeneous way in the literature; it is mentioned briefly and as a matter of practical circumstances when setting the scene for the interview.

To sum up, in spite of different views on what a good interview is, much of the same practical considerations are discussed in the literature. However, they are discussed

in different ways and sometimes for different reasons. When priority is given to the correspondence with reality as a measure of quality, the methodological discussion is held with eyes fixed on good interview data as the final goal. When the interview is viewed as contextual by nature, the relationship between interviewer and interviewee is described as more dynamic and can also be discussed in its own right.

Conclusions

Qualitative telephone interviewing is a method that is not much researched, and the research that does exist points in somewhat different directions and also focuses on different aspects. However, the conclusion that telephone interviewing is a method that is always inferior to face-to-face interviewing is premature. There are two sides to most choices and what is seen as a problem from one perspective could be an asset from another. Not being able to see the interviewee gives the interviewer less control, which could be viewed as a problem of standardization, or as a way of giving the interviewee the right of interpretation. Some contextual information is lost but other information may be gained. The distance between interviewer and interviewee could be interpreted as cold and impersonal or as a protective shield. A smaller commitment could provide less rich interview material but may also be a requirement for some interviewees. Some assets are clearly context-bound and seldom discussed in the literature. In my case, telephone interviewing makes the interview situation similar to the situation that is the topic of the interview, making some dimensions of the contact with Victim Support easier to apprehend. In Holt's (2010) case, telephone interviews were argued to be preferable with some interviewees since home visits had negative connotations for them. Although telephone interviews seem to have both general and context specific advantages as well as disadvantages, most of the literature is focused on the critique, either providing it or answering to it.

There is no area of qualitative research where telephone interviewing is viewed as the standard method. I decided to use telephone interviews in my own thesis work for practical reasons mainly, which seems to be rather common. As an example, Holt (2010), Stephens (2007) and Sturges and Hanrahan (2004) all start their notes on telephone interviewing by describing how they came to interview over the telephone by chance when face-to-face interviewing became unpractical. I planned my interviews to be conducted over the telephone, and making the interviews a fairly small investment for the interviewee was the main reason. As we have seen, the main objection towards telephone interviewing is the loss of richness. Since I interview about a contact with Victim Support that could be expected to be rather short in most cases, I did expect that not everyone would have much to tell me, but that the short comments could still be of interest when taken together. Finding an easy and convenient way to collect them, both for my informants and for me, thus seemed reasonable. Though this may have affected the material negatively in some ways, I have reason to believe that many of my interviewees would have turned down a face-to-face interview. Some were initially reluctant to share their experiences even over the telephone and though most agreed to be contacted again, many of those I have asked for a

face-to-face interview have been unable or unwilling to participate.

An advantage that most researchers seem to agree upon is that access to groups that are difficult to reach is facilitated by using telephone rather than face-to-face interviews. Although the smaller investment is probably one reason, other reasons, such as physical distance (Musselwhite et.al., 2006; Opendakker, 2006; Stephens, 2007), disadvantages with available meeting places (Holt, 2010; Sturges & Hanrahan, 2004) and increased anonymity (Musselwhite et.al., 2006; Opdenakker, 2006) are mentioned. Eliasson-Lappalainen (2000) argues that shedding light on previously untold stories and listening to voices that are usually not in a position to make themselves heard is an important mission for researchers, and a practice that is important both for the quality and for the ethics of research. In line with this, finding a way to reach groups that would normally decline participation in research projects for whatever reason should be a priority in the methodological literature. To challenge the notion that face-to-face interviewing is always preferable in qualitative research may be a step in the right direction. As we have seen, telephone interviews could facilitate the participation of some groups of interviewees and may also facilitate some areas of communication. Using newer forms of technology may have similar advantages. Although meeting research participants personally has its clear assets, it seems reasonable to believe that a larger variety of modes of communication could provide a larger variety of participants and research results.

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Relations in the field

The politics of methodology: Examining inclusive research for people with learning difficulties

Niklas Altermark

Abstract

The purpose of this text is to discuss one prominent feature in disability studies, namely the ambition to include disabled individuals in research. The focus is on the inclusion of people with learning difficulties, a group commonly understood to lack many capabilities necessary for engaging in scientific inquiries. This chapter offers a critical literature overview of articles discussing or using inclusive research methods for the study of and with people with learning difficulties, published in three major journals within this particular research field. In the analysis, three re-occurring features are discussed: (1) the role of the researchers and the disabled participants respectively, (2) the relation between getting the inclusive research process right and getting interesting results and (3) the idea that participants with learning difficulties represent a wider body of similarly disabled individuals. The analysis shows that there is a tendency in inclusive research projects to cling to the overriding normative ideals on inclusion, whilst being less inclined to show how these ideals are transformed into practice. Among the problems discussed within the three themes are: the tendency to refer to a "real" or "genuine" way of doing inclusive research whilst being less clear about what this "real" way entails, the inclination to highlight the inclusive process rather than the empirical results and the idea about disabled activist groups as representing the constituency of people with learning difficulties. To fulfil the promises of a more democratic and inclusive research practice, I argue that these issues need to be critically discussed.

Introduction

The last twenty years have seen the emergence of disability studies as a distinct research field within the social sciences. This field, I would argue, is not only distinguished by its

empirical focus, but also by a commitment to the emancipation of people with disabilities and the improvement of public services targeting this group. Close links between the research community and the disability movement have nurtured discussions and debates about the proper place of disabled people in research processes (Hodge, 2008; Priestley et al, 2010), focusing primarily on the relationship between researchers and the people being researched (Atkinson & Wamsley, 2010). Research on disability has a history of excluding people with disabilities and therefore critical voices have argued that professional researchers are benefitting from, or even exploiting, the life stories of disabled people (Barnes, 1996). In opposition to what is being called "rejecting research" an inclusive research agenda has emerged, advocating participation and control for people with disabilities in all steps of the research process (Barnes, 2003).

This chapter examines the methodological ideals of inclusive research, especially focusing on people with learning difficulties. Research is commonly understood as an intellectual pursuit, requiring capabilities that people with learning difficulties are often perceived to lack. Whilst a physical impairment does not affect the ability to formulate research questions or conduct analysis, the same cannot always be said about, for example, Down's syndrome. Hence, learning difficulties pose some special challenges to the ideals of inclusion. The last decade has seen a host of projects involving professional researchers and individuals diagnosed with cognitive impairments. The purpose of this text is to pinpoint some insights brought to the fore by these and to discuss some problems that need to be dealt with in order to take the research agenda forward. In essence this chapter provides an examination of articles drawing on or using inclusive methods. The analysis will revolve around three themes that re-occur in the publications I have looked at and that I believe are of vital importance in order to meet the inclusive ambitions. These themes are: (1) the roles of professional researchers and disabled participants respectively, (2) how to weigh the importance of getting the inclusive research process right versus getting interesting results and, lastly, (3) the idea that participants with learning difficulties represent a wider body of similarly disabled individuals.

The articles on the basis of which I build my argument are collected from the issues of three academic journals published in the last ten years; *The Scandinavian Journal of Disability Research*, which is the journal with most frequent contributions by Swedish and Scandinavian scholars; *The British Journal of Learning Disabilities*, which is one of the prime journals publishing research on people with learning difficulties and *Disability & Society*, which is one of the most influential disability journals in general. This provides me with 35 articles that discuss or apply inclusive methods involving individuals with learning difficulties. Obviously, this is not enough in order to be able to offer a complete account on inclusive research, but will suffice to hint at some important tendencies. Before turning to the three themes and empirical analyses of the articles I will provide a background of the emergence and roots of inclusive disability research.

The basics of inclusive research

It would be a mistake to suggest that only *one* inclusive research agenda exists. Rather, inclusive research could be seen as a set of different methods and ideas, stemming, however, from the same overriding principles, developed and advocated by the disability movement (Barnes, 1996; Priestley et al, 2010). To understand the methodological ideas of inclusive research it is therefore important to understand the ethos of disability activism.

Since the late seventies, two ideas have been foundational for the political aspirations of disability organizations in Western Europe. The first is the so-called *social model* of disability, developed in Britain, but exported to and adopted by activists and scholars all over the world. The central feature of this model is a distinction between *impairment*, which refers to the medical condition of the individual, and *disability*, which is understood as the result of social structures (Shakespeare 2006:11). The social model core claim is that there is no causal relationship between the two, that is, the living conditions of disabled people are the results of social organization rather than inherent impairments of the individual. This implies that focus needs to be shifted from individual characteristics to social structures (Shakespeare 2006:11-15). One of the prime targets for disability studies has been to identify such structures. Simultaneously, the exclusion of impaired people from research itself has been interpreted as a disabling barrier that needs to be torn down (Barnes, 1996).

The second idea is expressed in the slogan "nothing about us without us", implying that processes concerning disabled people should involve, and take as a starting point, the lived experiences of this group. In terms of research, this points to an inclusive approach – rather than being research *about* disability, an ideal of research *by* disabled people has attracted strong recognition (Priestley et al, 2010). In order to produce valid knowledge on the living conditions of disabled people, these must be involved and heard in the process: they are the experts and their life stories are the key to gaining knowledge.

The social model and "nothing about us without us" sparked a debate in the mid-nineties about how inclusive methodologies could be incorporated into disability studies (see for example Disability & Society, no. 1, 1996). In this debate a re-occurring distinction was made between *emancipatory* and *participatory* research practices (Chappell 2000). Within the emancipatory programme research is understood as a tool for improving the lives of disabled people. Ideally the researchers are themselves disabled and all scholars should be held accountable to the organizations of disabled people (Chappell 2000:38). The participatory model could be seen as a milder version grounded in the same spirit; research questions should be identified, or at least brought to the attention of the constituency of disabled people, disabled individuals are to be actively involved in the analysis and alliances are to be formed between the research community and self-advocacy organizations (Chappell 2000:38-39). There are also numerous other ways to label similar methodological propositions (like "participatory research", "action research" and so on). Considering the purposes of this text, the broader term "inclusive research" will be used and methodological differences will be spelled out and addressed rather than tagged with labels picked up in the terminological jungle.

The political standpoints of the disability movement, leading up to the call for inclusive research, was initially not developed for people with learning difficulties, but for people with sensory and physical impairments (Shakespeare, 2006:1). It has also been suggested that people with learning difficulties are overlooked in inclusive research as well as in the disability activist discourse in general (Goodley, 2004, Chappell 2001:45). The facts that many individuals with learning disabilities have problems reading and writing academic texts, that some communicate in ways that are different from those of the non-disabled and that some struggle to understand abstract concepts, all prompt the question: Can *they* really do research? People using wheelchairs are helped to engage in science by accessible buildings and offices, as proclaimed by the social model – but what would be the equivalent way of letting people with learning disabilities in? The growing number of inclusive research projects on learning difficulties can serve as a basis for hinting at some problems that need to be overcome in order to reach an answer.

Including people with learning difficulties

Turning now to the analysis, the three themes presented below all represent re-occurring features in the articles analysed. Each of them could be read as an argument for what I believe to be an important lesson for those interested in engaging in inclusive research with people with learning difficulties.

Classifying and clarifying researcher roles

As noted above, people with learning disabilities are often perceived to lack the capabilities necessary to undertake research on their own. According to a social model understanding, this is a consequence of discriminatory structures intertwined with the scientific infrastructure and practices. The role of the professional researcher should thus be to help people with learning disabilities to tear those structures down. However, one might argue that reading and writing, theorizing and concluding, are inherent to what research is, without these abilities research would be something else. No matter how many discriminatory structures are removed, some individuals with learning difficulties would still struggle with these things. This suggests that the purpose of professional researchers might be something more than just getting structural hindrances out of the way and, similarly, the role of participating individuals with learning disabilities might be something other than doing research in the ordinary sense of the word.

There are numerous ways of labelling professionals and disabled participants in inclusive research projects. Some articles avoid making a distinction between the two (see for example Abell et al, 2007), whilst there are also a wide set of labels ("participants", "co-researchers", "real researchers") used to denote the individuals with learning disabilities. Despite the numerous labels, Walmsley (2004) has argued that inclusive research suffers from a lack of role clarification regarding the contributions to the research process of the professional

researcher and the participants respectively. There is a strong tendency, Walmsley claimed, to overlook the fact that people with learning difficulties and professional researchers are different in important respects. This could be seen in light of the dominance of the social model, according to which differences are a result of social structures rather than of individual characteristics. Hence, from that perspective disparities that seem to stem from impairment characteristics are deemed irrelevant. However, to neglect differences is to risk that unequally distributed control over projects gets masked behind the rhetoric of inclusion (Walmsley, 2004). My overview shows that there are still tendencies to be opaque about the roles of professionals and participants, but that there are also a number of more recent projects that have answered to Walmsley's call for role clarification (see for example Williams & Simons, 2005, Lunn & Munford, 2007).

Despite the numerous ways of setting up "participant-professional" relations in the analysed material, frequent references are made to what, among other things, is called, "real inclusion", "truly participatory research", "inclusion in reality" and so on (see for example Goodley, 2004, Williams & Simons, 2005:8, Hodge, 2008:29, Garbutt et al, 2009:39). Such allusions to a right way of achieving inclusion are made in a majority of the articles analysed. In turn, this figure of thought implies that there exists an opposite fake participation or hoax inclusion. However, the idea about a correct way in which to fulfil the ideals of inclusive research is at odds with what is actually stressed in most of the articles. On the other hand, my material shows that the role of the professional researcher varies greatly between different projects, from a solely supportive and facilitating role, to a traditional analytic role where people with learning difficulties seem more like interviewees than participants (for example Brooks & Davis, 2007). Hence, it seems that "inclusion" can mean many things – ranging from interviewing to letting people with learning difficulties make the analysis. Furthermore, even within the same research projects, the material shows several examples of how roles are changing and evolving over time and thus flexibility is often put forth as a key to success (see Abell et al 2007, McClimens, 2007); in order to make an inclusive research process function, one has to adapt as the process evolves. For example, participants are learning and can therefore gradually take on more analytic responsibility whilst the professional researcher steps back (Williams & Simons, 2005). A key characteristic, it seems, is for the professional researcher to be able to shift between supporting, educating, handling technical supporting-devices and listening to the opinions of the people involved (Williams & Simons, 2005, Abell et al, 2005).

The references to a real way of doing inclusive research instil the impression that a best practice exists. The articles analysed, on the other hand, suggest that inclusive research is rather about being able to switch between different roles and models, adjusting to contextual factors and designing inclusive research in relation to given circumstances and in cooperation with participants. The references to the "best" or "real" way of doing inclusive research are never defined; throughout the articles the real/genuine-inclusion remains an abstraction, permeating the statements of purpose, but never filled with substantial methodological recommendations.

This lead me to the conclusion that it is important to design the research process to enhance flexibility so that changes can be made, learning and evolvement can be facilitated

and all participants, professional or not, can grow into their respective roles. Inclusive research is therefore time-consuming and implies a long-term engagement (see Cameron & Murphy, 2006). Inclusive researchers should be hesitant to the inclination of labelling researcher-roles or trying to brand projects as "real" or "genuinely inclusive". Inclusive research varies greatly and the main ambition should not be to live up to some undefined and pre-set standard, but to account for flexibility, change and variety in all steps of the research process. This means that inclusion will be manifested in different ways depending on the nature of the research projects.

Getting the process right and getting interesting results

The second feature I will address deals with *process* and *content* – how research is *being done* and the *subject matter* of research projects. The relation between these two dimensions manifests itself in two different, but nonetheless related, ways depending on the *purpose* of the inclusion and the *focus* of the articles.

Doing inclusive research involves two parallel processes: to ensure that participant influence lives up to the standards set and to ensure that the research gets done (Williams, 2005:12). Looking at the first aspect of balancing these concerns, the *why* of inclusive research, the following question is posed: Is it to get a more democratic disability research community and promote stakeholder influence or is it to reach better and more insightful conclusions? Of course, the answer need not be either/or; inclusion can promote a normatively more desirable process as well as contributing to better results at the same time. Nevertheless, quite a few authors address this question and emphasise that there could be a potential conflict. In his article about doing inclusive research with a self-advocacy group Val Williams (Williams & Simons, 2005) accounts for his concern on whether participants can manage the subtle communication that is required to do good interviews. He quotes his own field-note asking: "what is more important – to get the information we want, and to get interviewees talking, or to get the process right?" (Williams & Simon, 2005:12) Similar concerns are expressed in a number of articles.

Alex McClimens (2007) argues that there is a tendency to overlook all content-related problems in doing inclusive research. My overview reinforces this judgement on how process and content are emphasised; whilst only a quarter of the articles elaborate on how inclusion improves or imposes problems in generating research findings, more or less all articles stress the value of an inclusive process for its own sake. Often, this process-prevalence is expressed in terms of "empowerment". In the material a number of different meanings are attached to this concept; such as the opportunity to learn, getting to tell one's own story or being in control (see for example Atkinson & Walmsley, 2010, Lesseliers et al, 2009), but most often the concept is not elaborated upon (see Williams & Simons, 2005, Petersen, 2011).

The overall impression from the articles analysed is that the competence of people with learning disabilities is relatively neglected in comparison to the emphasis on the needs for this group to be included for more idealistic reasons. The superiority of process as the dominating reason for undertaking inclusive research may result in an impression

contradictory to the intended: reading inclusive learning disability research may instil the impression that this is a group that needs empowerment, but that good research does not necessarily require the inclusion of people with learning difficulties (a good example of such an emphasis is found in Cameron and Murphy, 2006). Therefore, a shift towards highlighting the contribution to research outcomes that could be made by the disabled would be most welcome.

The second dimension of process-content regards what the articles are focused upon and to what extent. It is notable that the articles in the material embody considerable differences regarding the type of research project: some projects are initiated by disability organizations, some by researchers, some are collaborative in nature and contain many members and some consist solely of a professional researcher and a participant. What is striking, however, in terms of focus, is that despite the differences, it is often the inclusive method itself that is given the most attention in the written report. It seems that process-prevalence is not only about motives for undertaking inclusive research, but also about actually writing more about the process than about the empirical findings. Here there are roughly three typical cases on how articles are written. The first is a reflective account on an inclusive project, focused on methodology. This kind of article provides reflections and advice although rarely pointing out the pitfalls (see McClimens, 2005). The second typical case takes the form of an evaluation of some special method or technical device (see Germain, 2004). Most articles written in this way conclude that the technical device works fine as a tool for inclusion. The third type is the sneakiest: it states that the project addresses some empirical problem, although it, often implicitly, shifts focus to evaluating the inclusive methodology itself (see for example Brooks & Davis, 2007, Garbutt et al 2009). The heavy focus on methodology infuses the impression that the scientific community is more interested in the method itself, and the normative underpinnings, than in the empirical matters the research projects deal with. This might of course be a result of articles on inclusive methods being easier to get published than empirical investigations, which would mean that the responsibility lies with journal editors as well as with the authors of the articles. It might also be due to the studies focused on empirical findings being published elsewhere. Nevertheless, the lack of empirically focused articles in the material is both surprising and worrisome for the same reason as stated above: the risk of re-constituting individuals with learning difficulties as being in need of empowerment but not so much as contributing to research findings. Hence, I would suggest that inclusive research projects to a large extent have justified their methodological choices in terms of empirical gains and that writing on inclusive research should be more about empirical findings and less about the purposefulness of the process.

Representing people with learning disabilities

The last theme to be discussed is the idea of representation, that is, the notion that individuals with learning disabilities are entitled to being represented in research. The slogan "nothing about us without us" contains two "us", the first denoting people with disabilities in general and the second disabled people functioning as representatives in

research processes. The idea being that the inclusion of some individuals with learning difficulties in research is equal to the wider community of people with learning difficulties being represented. Hence, underpinning this argument is the notion that the individuals in the second "us", to a sufficient extent, are able to represent those of the first.

In the inclusive research literature frequent references are made to the disabled as "experts" providing perspectives professional researchers are presumed to lack (see for example Brooks & Davies, 2007:128). What are these unique perspectives that participants with learning difficulties bring to the table? Although some publications point out how participants have come up with angles and ideas, often stemming from their everyday life, the participant perspective is often treated as self-evident. This, I believe, is quite a serious misconception.

Considerable doubt could be cast on whether this, often unexplained, "expertise" of individuals with learning difficulties really can be said to capture the interests and perspectives of the wider constituency of people with learning difficulties. The formulations that there is a "perspective", in singular, that needs to be present ignores that people with learning difficulties constitute a very heterogeneous group with regards to needs for support, political inclinations, favoured forms of service, ways of looking at the world and so on. The notion that it is possible to be an "expert" on living with a learning difficulty implies that the experience of disablement is much more homogenous than it can reasonably be. Representation talk is also at odds with the often-mentioned flexibility stressed above, which emphasises that the inclusive research process must be adapted to the different capacities and circumstances of the participants. At the same time, claiming that these individuals, with their different capacities and circumstances, represent a common ground of the wider population of individuals with learning difficulties thus appears as contradictory.

The projects I have analysed have often sprung from cooperation between researchers and activist groups (for example Garbutt et al, 2009, Stalker & Lerpiniere, 2009, McClimens, 2007). Like all political organizations, these activist groups hold ideological worldviews, political aspirations and practical demands for change – that is the essence of being political. It is not problematic to involve groups with ideological standpoints *per se*, since all social science, in some sense, is built on theoretical and normative underpinnings. However, it becomes troubling when a specific activist group, with a specific ideological perspective, is (implicitly or explicitly) understood as representing general interests or ideas among people with learning difficulties. None of the articles about cooperative work between activist groups and researchers acknowledge the ideological nature of these or discusses what these groups are representing in ideological terms. An equivalent example of, let us say, including a social democratic youth organization to represent young people in general would obviously not meet scientific standards of validity.

Similarly, it is probably fair to say that most participants in inclusive research have relatively mild learning difficulties. Reading the articles, it appears that professionals and participants are able to communicate, often about relatively complicated and intricate matters concerning the research projects (for example McClimens, 2007, Atkinson & Walmsley, 2010). Although some participants cannot read complicated texts the

researcher can nurture understanding through explaining or producing easily-read material (Walmsley, 2004). However, this may not be possible to do when working with more severely impaired individuals. In terms of representation, the focus on mildly impaired individuals is problematic, since people with different kinds of learning difficulties and different service needs, might also have different interests.

These arguments cast considerable doubt on whether participants in inclusive research could be properly understood as representatives for the wider population of people with learning difficulties. To not acknowledge this and to treat the ideological and functional characteristics of participants as being more general than they really are, implies excluding and neglecting those who do not share these ideological standpoints or impairment characteristics. This is not an argument against inclusive research as such, but an argument against the idea of inclusive research practices as representative. Therefore I argue that researchers and participants in inclusive research projects should be careful as to how they frame and describe "representation" as a motivation or tool in research processes.

Conclusions

Inclusive research on learning difficulties illustrates the ubiquitous link between norms and methodology; how we do research is not neutral but built on ideals about who gets a say, and in what way, in the production of scientific accounts. The inclusive research agenda is only one of many methodologies seeking to bring the relation between normative values and research practice to the fore: within a variety of empirical fields researchers have sought to emancipate the groups being researched by involving them in the process. In order to take advantage on the ideals of inclusive research dealing with the particular group in focus here, more focus on the specific challenges in researching together with people with learning difficulties would have been desirable. The actual differences between professionals and individuals with learning difficulties are rarely mentioned and potential problems in involving this group generally not spoken about. More or less all articles I have analysed are written as "success stories", which in turn may prevent cumulative learning from the mistakes of others.

The analysis presented bears witness to how the boundaries between the social movements, the research community and the state are often blurred by interactions and co-operations. Many disability scholars are themselves activists, the research agenda, at least in Sweden, is continuously influenced by steering ambitions of the state and the ideas produced influence the implementation of disability legislation. The debate on the purpose that is served by disability research is entangled with the goals of various political actors. The strong linkages found between normative ideals and the ways in which inclusive research is presented illustrate this. I would argue, however, that the strong ideological nature of inclusive research may also result in a risk of problematic aspects and potential pitfalls being overlooked. The idea of inclusive research as representing a stigmatized group, the references to the "real" way of achieving an inclusive process and the preoccupation with process rather than content, may all be interpreted as indicative of an inclination to cling

too strongly to the values of emancipation and participation. However desirable these ideals might be, my impression is that they sometimes overshadow distinct methodological problems. A shift of emphasis from norms to the practice of inclusion may serve as a starting point to handle the problems pointed out by the analysis in this chapter.

Inclusive disability research emerged as a reaction against previous discriminatory research paradigms. It is of vital importance that the inclusive agenda is not turned into a new dogma, out of reach for scrutiny and counter-attacks. One way of avoiding this may be to start to debate and critically assess the inclusive ideals themselves. The polemic nature of many of the articles I have read is puzzling considering that no visible enemy is within sight; everybody writing about inclusive research seems to agree on the inclusive ideals. Critical examinations on problematic aspects, practical problems and adverse effects are nowhere to be seen. This article has been an attempt to bring a few such aspects to the fore. Inclusive research on learning difficulties contributes to disability studies and social science methodologies in a general way, as the articles analysed here testify but, just as in any other field, it would gain from debate and critical examination.

Overall, I would wish for inclusive research to be more confident about bringing important insights and to be a little less fond of the normative heritage from the disability movement. We have to allow for inclusive research to be a diverse and fluent activity that perhaps cannot always be placed under one single set of ideological taglines. In so doing, including people with learning difficulties should not be understood as representing people with learning disabilities in general in order to live up to pre-set standards, but gaining the perspectives and experiences of some individuals that really want to participate in order to find out really interesting things.

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Fieldwork with children in school: Positioning oneself as an out-of-the-ordinary-adult

Maria Heintz

Abstract

Researchers have emphasized the child's perspective within social science since the 1990's which has resulted in a shift in the view of children within research towards the recognition that children are active, creative social agents in their own right. The main aim of this paper was to present methodological and ethical aspects regarding research with children on the basis of two propositions, the first being that research with children should emanate from the child's perspective, and that the methods should be modified accordingly. The second proposition stresses the importance of continuously reflecting upon the ethical issues inherent in research with children. In relation to my propositions I discuss the implications of these ideas both on the role of the researcher and also from an ethical standpoint. The researcher's role as an adult doing research with children is complex and I argue for a sensitive and reflective approach both with regard to the choice of method and when it comes to ethical considerations. I also stress the importance of acknowledging that the children are the experts in their own field. A main conclusion is to use an out-of-the-ordinary-adult approach in order to reduce power structures embedded in the relationship between adults and children. This implies that the researcher positions himself/herself as an uninformed adult or as someone who is unaware of the actual meaning and essence of being a child today and actually openly shares this unawareness with the children.

Introduction

Since the United Nations Convention on the Rights of the Child in 1989 there has been a paradigm-shift regarding children in research and in society at large (James and

Prout, 1990; Woodhead and Faulkner, 2008). The emergence of the paradigm reflects in part a movement away from seeing children as objects of research, often neglecting the children's own experiences, towards recognizing that children are active, creative social agents in their own right (O'Kane, 2008). In this tradition the researcher "reflects a direct concern to capture children's voices, perspectives, interests, and rights as citizens" (Corsaro, 2005:45). The main aim in this paper is to explore two distinct propositions regarding research methodology which I have developed after reviewing the literature on studying children from a child's perspective. Firstly, the methodological framework should enable the researcher to engage with the child in a way that allows for the child to participate on his/her own terms. Secondly, the ethical considerations inherent within all science have to be even more thought through during the entire research process when children are the focus of the study. This is because of the children's position in society which is bound by power structures that have to be reflected upon and dealt with.

The focus of my thesis is the perception of friendship and peer group formation at school among 10-11 year-old schoolchildren. Throughout the paper I will use my experiences in this particular field to explore and discuss these two propositions which I see as central to the study with children.

In the next section I will present the child's perspective and in the following two sections I will discuss the two propositions and their methodological implications on the role of the researcher.

The child's perspective

To understand the following arguments in this chapter a presentation of "the child's perspective" is in place. What does the concept "the child's perspective" signify? Arnér and Tellgren (2006) discuss the concept child perspective related to the concept of the child's perspective. They argue that the concept child perspective is complex and hard to define but could be explained as the adult trying to view a situation through the child's point of view. The child's perspective, on the other hand, is the child's own conception of his/her life (Arnér and Tellgren, 2006). The struggle to be seen as an individual within a social category is not something unique to children; the uniqueness lies rather within the research society being at all interested in children's own interpretations of their experiences and thoughts as social actors and analyzing these experiences through a societal perspective.

The child's perspective within social science has to a greater extent been emphasized by researchers since the 1990's and has been named the new childhood sociology (see Corsaro, 2005; Bliding, 2004; Ihrskog, 2006; James and James, 2004; James and Prout, 1990; Rasmusson, 1998; Qvarsell, 2004). Particularly emphasized in the sociology of childhood is the view of childhood as a social construction (Halldén, 2007; James and Prout, 1990; James, Jenks and Prout, 1998; Thorne, 1993). This means that it is impossible to consider childhood as a mere biological phenomenon. Halldén (2007) makes a distinction between children and childhood by pointing out that "children" is the description of an age group

while "childhood" denotes cultural and social significance. Furthermore, the biological traits will mean different things in various groups, societies and cultures; thus, childhood will be given a meaning through social arrangements and cultural rules. In my research on children's friendship formation in school this implies that friendship and peer groups could be constructed differently within and between different peer groups, in different school classes and in different schools. James and Prout (1997) argue that childhood is both the same for all children but also very different depending on the diversity of children's different life situations.

Within the paradigm of childhood sociology, children are seen as being constructed in different ways (James and Prout, 1997; Morss, 2002). One approach is to view childhood as a "tribe", that produces its own unique "children's culture" with its own beliefs, practices and institutions (Corsaro, 2005). Another approach is to view childhood as a part of the structure of society. Children are then seen as constituting a social category, just like other social categories within society, such as social class, gender and ethnicity (Christensen and James, 2008a; James and James, 2004). An extension of the social structure approach is the "minority group" approach which treats childhood as an oppressed minority group (Mayall, 2000; 2008; Qvortrup, 2008). O'Kane (2008), Qvortrup (2008) and Mayall (2000; 2002) have in their writings conceptualized children as a minority group in society with reference to the child-adult power imbalance and the generationing processes (e.g. the legal designation by age). At the same time, researchers within the new childhood sociology stress the importance of acknowledging the diversities between children within their childhood (Christensen and James, 2008b; Kehily, 2005).

The aim of my study is to show the child's own experience and interpretation of friendship formation in a fourth grade school class. The child in this paper is viewed as both belonging to a social category (children) which is primarily defined by adults and appointed by the society, and belonging to friendship groups defined by the children themselves. Thus, the focus is on both the child in society, in the group, and on the individual child's unique understanding of these experiences.

How to go about studying children

Both Christensen (2000) and Christensen and James (2008a) stress the point that children are good informants and that they can participate in interviews and observations and answer questionnaires to the same extent as adults. However, there are also methods that can be used when studying children in order to include them in the research on their own terms. The methods I use include an ethnographic approach and different participatory techniques. These will be discussed in this section together with the role I play as a researcher using these methods.

Even if the research question and the researcher's background are crucial for the choice of methods, an important idea within the child's perspective is that the methods should be constructed for, and even with, the child. The trend in the paradigm of the study of childhood in social studies has been that of focusing on the use of ethnography as an

appropriate method since "it allows children a more direct voice and participation in the production of sociological data than is usually possible through experimental or survey styles of research" (James and Prout, 1990:7-8).

Ethnographic studies are characterized by closeness to the field and a wish to engage in the everyday life of the people of interest (e.g. Hammersley and Atkinson, 1995; Sjöberg, 2011). Ethnographic methods are used in the anthropological tradition when exploring foreign and exotic cultures. Using the concept of "friendship cultures" (Corsaro, 2005) for children's interactions and labelling children as a "tribe" or a social category might be a reason why these methods seem especially useful for the understanding of children. The following quotation demonstrates a general stand on how to engage in the lives of children:

> [I]f one really wants to capture the rich social world of children's lives and peercultures it is necessary to do extended fieldwork. Literally this means that one has to enter children's play and be willing to get pants dirty and shoes muddy. (Evaldsson and Corsaro, 1998:381)

Thus, in order to learn about the knowledge the children have of their friendship formation and of being school children I have to study the children's actions in their social environment. In my case the school is one such environment where social interaction takes place. My study focuses on participant observations of the children's everyday activities regarding friendship formation and social interactions in school. Observation, which is a central part of ethnographic studies, is not dependent on what people say they are doing or what they are thinking of but is more of a direct approach as compared to, for example, interviews (Einarsson and Hammar Chiriac, 2002). Since some children (as well as some adults) might have difficulties expressing themselves in interviews, the observation method contributes to a deeper understanding of what actually happens. At the same time the role of the researcher becomes of fundamental importance to the outcome of the study since relationships are formed in the field. To get into the children's world as described by Evaldsson and Corsaro (1998) above, I really have to participate in their play, getting to know them. What implications does this stance of trying to get into the children's world of play have on me as an adult researcher?

The role of the researcher

When it comes to the role of the researcher studying children my position as an adult is of concern for the design of the study. There is an unbalanced power structure embedded in the relationship between adults and children, which I have to relate to. Researchers interested in childhood and the social life of children have, as a result of this unbalance, emphasized the value of taking on a research role as an atypical and less power-oriented adult (Corsaro, 2005; James and James, 2004; Mandell, 1991; Mayall, 2008). This "least-adult-role" requires the researcher to try to blend in with the social world of the children, not siding with the adults but operating on the children's level in their social worlds (Mandell, 1991). This approach has been questioned for its practical feasibility (Mayall, 2008). The critique which I present includes whether or not it is practically possible to

"become" a child as an adult (e.g. Christensen and James, 2008a; Mayall, 2008; Connolly, 2008). I would argue that it is not. Even if I sometimes recollect childhood memories and remember what it was like being a child the memories are tainted by time and my experiences as an adult and a parent. Also, the very structure of childhood and what that constitutes has changed considerably. And, above all, children are clever enough to realize that something strange is going on.

I have reviewed several studies using the children's perspective which show how the appearance of the researcher as an adult both in manner, size and language has had an impact (Connolly, 2008; Corsaro and Molinari, 2008; Bliding, 2004). The children have commented on them, changed vocabulary around them or talked about what they thought the researcher wanted to hear; they have also reflected on the very issues taken up by the researcher, making them their own. Furthermore, the power structure mentioned above is a part of the relationship between adult and child and might not be sufficiently taken into consideration by the researcher trying to "become a child". The reflections upon the researcher's influence on the study, which are crucial within ethnographic studies, are then diminished.

The out-of-the-ordinary-adult approach

I argue that a position as an uninformed adult could better lead the way towards acquiring a child's perspective. Uninformed or unaware of the actual meaning and essence of being a child today and actually *openly sharing this unawareness* with the children is one way of making them understand that they have information and knowledge that I as an adult do not have. In a school context it is important to be aware of the role the other adults' play, which is often of an authoritarian nature. When I presented myself to the children for the first time, they asked me if I was going to be a schoolyard keeper when being out on the playground with them during breaks. When I explained that I would not be a schoolyard keeper and intervene in small disagreements and disputes they were puzzled. They seemed to be used to adults intervening in their play when there were disputes. The children reacted to me as an adult in different ways and also reacted if I did not live up to their expectations of what they were used to an adult role being. In trying to understand children's peer groups in school and their perceptions of friendship groups it is crucial to let the children themselves reveal how they make friends and to let them talk about what is important for *them*.

My experience in a school setting is that adults interpret the children's intentions and feelings without actively asking what the child means by, for example, "friend" or if they are displaying feelings of exclusion. The teacher seems to have a ready concept of friend and exclusion and acts on those presumptions. In my study I have tried to thoroughly go through my own thoughts and preconceived ideas about friendship, children and children's peer groups in order to be aware of them, not trying to pose my ideas on the children. I have two paroles which guide me in my study; "I don't know" and "I want to understand" in my interaction with the children. This view is supported by Mayall (2008) and Thornberg (2007) who, in their research with children, have presented themselves as a person who, precisely *because* s/he is an adult, does not have the knowledge the children have of their

social worlds. I would also argue that this is a stance taken by many researchers studying adults. It is the people in the study who have the experience of what it means to be them and if it is their perspective I wish to understand then I have to be humble, to listen and to be genuinely interested in them. This out-of-the-ordinary-adult approach is not only fruitful in the collection of data but it also makes the children want to talk to and share their thoughts with me.

The importance of language

This leads me to the informal conversations and interviews that take place in ethnographic studies (Anderson, 2006; Hammersley and Atkinson, 1995; Sjöberg, 2011), in which there is an emphasis on both the language used by the children and that used by the researcher. Coming from different discourses the researcher has to be aware and sensitive towards the ways in which the children communicate and relate to their world. This means getting familiarized with the language that is used, the social actions and meanings that are connected to concepts and words as well as being able to make an understanding of the social interactions and the relationships that the children are part of. Two empirical questions that are of importance in my study are which are the terms that children use when talking about friends and peers and do they vary among the school classes and in different friendship groups? It is essential to let the children themselves define the terms they use and, in so doing try to reach some understanding of how they construct friendship.

Silverman (2007), as well as Rapley (2004), argues that informants will turn to collective representations to explain their behaviour and present themselves differently depending on the audience. Hence, there must be an awareness of the children's representations of themselves in relation to me as an adult and as a researcher in a school setting where they are used to adults telling them what to do and answering accordingly. This applies to diverse settings within school, where the children present themselves differently in the classroom compared to in the schoolyard when no adult is present. To respond to the differences in communication between adults and children the emphasis is on allowing them to participate on their own terms. To get to the more "natural" talk in an interview with children one way is to conduct the interview in a, for the child, known and familiar context (Corsaro, 2005; Mayall, 2008). Furthermore, when letting the children talk in smaller groups around a topic the discussions and conversations that take place could become more similar to that of their own natural conversations instead of answering the researcher's questions (Mayall, 2008).

Participatory techniques

The research process is inevitably a product of the relationships between the researcher and the research participants. This has been recognized by those who argue for the involvement of the children in all stages of the research process; from the design of the study as a co-researcher, to helping with interpreting the data and results (Alderson, 2008). With their reflectivity the children are especially suitable to become co-researchers, as they are already

examining the world around them. They are seen as sparring partners in the research process which puts an emphasis on their active participation as well as the interaction between the researcher and the children. O´Kane (2008) among others (see Christensen and James, 2008b; Ihrskog, 2006; Rasmusson, 1998; Staunæs, 2000) argues for the use of participatory techniques when conducting research with children. Participatory techniques were originally developed to help people with limited literacy or verbal skills to express their views about different topics and are generally regarded as empowering. These techniques can be seen as mediators in the communication between the researcher and the children as informants (Christensen and James, 2008b). Compared to the discussion above on the different language used by adults and children, it could be argued for the use of participatory techniques to help the researcher understand the children rather than stating that they have limited abilities.

Examples of participatory techniques are painting pictures, taking photographs and bringing into play other visual images which have been used to enroll children in research projects (see e.g. Christensen and James, 2008b; Ihrskog, 2006; O'Kane, 2008; Rasmusson, 1998). Studies have shown that participatory techniques are powerful tools enabling the children to express their needs and interests as well as take some control over the researcher's agenda (ibid.). The use of such techniques also affords the concretization of abstract ideas and concepts which helps the children to understand the researcher's aim. A variety of different techniques are used in the daily teaching situations in school that might be an advantage to my research. These techniques used together with the researcher within the study might make the children more comfortable in participating. There might be a problem if the child experiences school as something rather unpleasant and could thereby also be reluctant to participate in something connected to that environment. The key is to find a technique suitable for the individual child.

Ethnographic studies with children are hence characterized by the use of a variety of techniques once one is in the field (Anderson, 2006; Hammersley and Atkinson, 1995; Sjöberg, 2011). Children's participation on their own terms varies in different kinds of methods, and the researcher's role is to adapt the situations according to where the child is. An important remark here is the assumption stated that children are creative actors in their own environment, which means that their ability might not be any lower than the researchers but put forth in another way. The role of the researcher in interaction with children is thus complex and some of the ethical aspects on it will be examined in the next section.

Ethical considerations when studying children

Above I have briefly touched upon ethical issues connected to research with children. In this section I will further discuss the power imbalance and some central ethical implications connected to my research with children.

The ethical aspects of researching with children not only include the research ethical demands such as informed consent, confidentiality and the protection of the rights of the participants, but also the interpersonal aspects of ethics and morals (Jensen, 2000). This means that one needs always to be aware of what it is one is doing and to reflect upon and act according to the child's perspective. It is also important to ask the child whether or not he/she wants to participate in the study; it is not only the parents who always need to be informed and asked. Together with Christensen (2000), I view ethics as a continuing social practice which means that the researcher has to trust his/her own judgments regarding ethical decisions throughout the research process. A number of situations emerge in the field which require the researcher to contemplate on ethical issues. In ethnographic studies, as well as in other qualitative research, there are issues concerning access to the children, how to handle the relationships with the children and with significant adults once in the field and also how to best exit from the field. Below I will in short highlight some of these ethical situations.

Field entry

Field entry is crucial in ethnographic studies since one of its central goals is the establishment of membership status and striving for an insider's perspective (Hammersley and Atkinson, 1995; Sjöberg, 2011). In research with children these goals depend on dealing with and developing the trust of a range of adult gatekeepers such as teachers and parents and gaining the acceptance from them as well as from the children (Corsaro and Molinari, 2008). For my study the first gatekeeper was the headmaster of the school whose approval I needed in order to be at the school at all. To get access on this level required me to take on the official role of researcher and gain trust through this status. The next gatekeepers were the teachers and parents. A formal consent from the children's parents is required in research with children, thus the researcher has to thoroughly explain the research agenda and the aim of the study not only to the children but also to the adults. It is important to engage the children from the start and I have chosen to inform them personally before I met the parents. The parent had been given written information before my visit to the classroom but the first verbal explanation and visit was with the children. This is to emphasize their unique contribution to the formation and realization of the study. At this stage I also engaged in the role of the out-of-the-ordinary-adult where the children got to freely ask questions or talk about what they thought of me being there. Hence, I had to shift roles within my study to get access to the different arenas concerning the worlds of the children that continued throughout the field study (e.g. the official school arena, the relationship to the parents and, most importantly, the relationship with the children).

Children's social position in society

The argument that children constitute a social category themselves implies that they interact with other social categories in society such as gender, ethnicity or social class. Children's daily lives, and thus childhood as an institution, are structured by adult views of how

those lives should be lived and of what childhood is (Qvortrup, 2008). This view entails that the researcher who wishes to study with children needs to confront issues concerning generation, such as the imbalanced power structure. As stated above one way of narrowing the power misalignment is to consciously take on the role of an interested adult convinced of the child's autonomy and expertise as an individual, not as a social category or minority group. To get an understanding of these implicit power relations the researcher also has to be able to work both ethically and practically with these issues. A practical way is that of involving the child in the entire research process using methods that are appropriate for the child in that particular study as well as for that particular child. An ethical and moral way is that of being reflective throughout the research process. At the same time, the researcher needs to be aware of his/her immediate connection to other adults in his/her position as an adult researcher.

The relationships with other adults in the child's surroundings form a major part of the ethical discussion in research with children. Since children are under the care of others (parents and teachers) the caretakers frequently want to have an influence on the research process as well, presenting demands of monitoring the research process and the children's statements. This collides with the research ethics and the duty to protect the rights of the children in the study as well as their privacy and sensitivity. I have been in situations where I have had to meet the adult demands in order to gain access, at the same time as having to be true to the research itself, as well as to the child's own wishes. A typical example in school is the teacher's wish to be updated on how things are in the classroom; what I as a researcher see. The same applies, for example, to the parent who wishes to be updated on her child's experience in school since the parent believes his/her child is being bullied. In this case I had no such indications and could therefore not see that the advantage of telling the parent was greater than not telling the parent from the child's perspective, which led me not to talk to the parent. These are truly complex ethical situations for which there is no exact answer as to how to respond. However, taking a child's perspective, the research should always be conducted with the child's interest in mind.

Inclusion and trust

Another ethical issue lies within different ethical codes in different discourses (Jensen, 2000). Jensen (ibid.) describes how children have an inclusive approach to others as opposed to the adult's exclusive approach. This implies that when a researcher comes close, as in ethnographic studies, the children involve the researcher in their interactions in a natural way. They show an intimacy and trust in the interaction with the researcher. One such example was when the children in my study instantly wanted to be friends with me on social forums on the internet. This was something which led to my having to consider the implications of accepting or declining the offer. In research with children this is a good example of situations when basic textbook knowledge about not getting too attached does not suffice. If I accept their invitation I will gain access to information which goes outside my study at the same time as they gain access to my private life, both of which I consider as belonging outside the research relationship. The children are used to being on the internet

and friendship online is evidence of acceptance in the friendship group. If I place myself outside this social forum I state my position as not belonging to them, which reinforces my role as a researcher and an adult thus proving the incompatibility of being an adult trying to become a child.

I do want to establish trust in order to be able to conduct the study and it is of importance to create this trust if I am to get close to the children. At the same time it is my job and I have to leave. My experience is that children are prone to trust a researcher who listens to them and truly hears them. From a researcher's point of view this is fantastic but from the child's point of view it might pose a risk of opening up, only to see the researcher leave. I have been very clear with the reason and length of my stay, as well as with the times when I will be at the school. Even so, the exit from the children is not easy but has to be taken into account from the very beginning so that both they and I know the circumstances. I also have to be very careful to balance the child's trust in relation to sensitive information that might harm the child (e.g. bullying) whether or not to talk to teachers or parents if information comes to my attention that I believe needs to be acted upon. In my study I have arranged for the school's student health team to be able to be a back-up if sensitive situations or information makes a child uneasy or if s/he has thoughts during and after my stay. Again, this is not a dilemma specific to children, but is of the utmost significance since children are in a dependent position which is crucial in a school setting. As I have mentioned earlier I always have my two paroles in mind, "I don't know" and "I want to understand" which in a humble manner puts me back in focus emphasizing that it is the children's playground I am visiting as an adult researcher.

Conclusions

I have, in this paper, explored some methodological and ethical aspects of research with children from the point of departure of two propositions, the first being that research with children should be conducted from the child's viewpoint and the methods should be modified accordingly. The second proposition stresses the importance of continuous reflection on the ethical issues concerning research with children throughout the whole research process. In relation to the propositions stated above I have also discussed the implications these ideas have on the researcher's role and ethical standpoints. The core viewpoint on children as socially constructed subjects in this paper has been shown to have an impact on how studies are carried out as well as on the way in which relationships are seen in the field. Furthermore, the advocates for this perspective stress the importance of viewing children as their own makers, constituting their own social category and culture which has to be explored to give voice to the children's experiences through the individual child's perceptions. I would argue that placing them within a specific peer culture and studying them with specific methods does not, per se, give them their agency. It is the approach of involving the children as participants in the research that in a broader perspective can disclose children's lives through a child's perspective. Still they are children

who reside within a larger society where there is a generational power imbalance and the adults make up the rules. I have in some places put forth critical remarks on the uniqueness of these issues connected to children arguing that these are methodological and ethical implications that are applicable when studying adults as well. What might be ethically dubious is to regard children as completely different from adults, not subjects in their own rights. One of the basic ideas of ethnographic studies is to really get close to a group and acquire an insight into their lives and cultures. The way to truly get an insight into the children's lives and friendship formation is not to become a child myself or to impose an adult perspective on them, but to be a curious "out-of-the-ordinary-adult".

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Research in a development setting: Ethical concerns during fieldwork in Tanzania

Karin Lindsjö

Abstract

This chapter discusses ethical concerns when conducting fieldwork on a sensitive topic in a development setting. Research on AIDS has been conducted in various settings and within various fields of science since its outbreak and has raised ethical dilemmas for the researchers. In this chapter the issues of fieldwork, vulnerable groups, power relations and sensitive research are discussed. The practice of using informed consent and securing anonymity for the participants is considered necessary within all qualitative research in the social sciences and therefore more of a practical part of the fieldwork. It is concluded that all research ought to have a high ethical standard no matter what the geographical location is, but perhaps this results in a greater responsibility for the researcher conducting research in a foreign setting in which he/she needs to be sensitive to the local context and be aware of the purposes and possible consequences of the study. A high level of awareness of one's own interests in the field and of the potential outcomes for the participants could make a difference in avoiding doing harm to the researched. However, it is nonetheless questionable whether we as researchers can guarantee that the research will actually '*do good*' to the respondents.

Introduction

Within my PhD research I am interested in how AIDS affects human capital formation in the Iringa region of Tanzania, which is one of the poorest countries in the world. Out of 182 listed countries on UNDP's Human Development Index (HDI) ranking list, Tanzania is listed as number 151 (UNDP, 2009). The current HIV prevalence level is estimated at 6.2% nationwide (UNAIDS, 2008:4), and 15.7% for the Iringa region, which is one of

the most affected regions within the country (Lawi, 2011). The study will address how the formal education system is affected both in supply and demand as well as how informal education is handled. Therefore, I am interested in how families are coping with the formal education of their children when affected by HIV and AIDS in comparison to non-affected families, as well as how the school system is handling the supply of education due to loss of personnel. In addition, I am interested in how informal education between generations is dealt with as many parents die before being able to pass on their knowledge to their children. Traditionally, the extended family has had the responsibility of taking care of the orphans but due to the extent of orphans in many African societies this coping strategy is breaking down (Isaksen et al. 2002:17, Kalipeni et al. 2004:313).

This chapter aims at considering ethical concerns that might be of importance when conducting fieldwork on a sensitive topic in a development setting. The discussion is limited to including only the fieldwork itself and not the preparing stages, data compilation or the analysis. Even though the themes presented in this chapter are related to one another, the discussion is divided into topics of fieldwork in development settings, vulnerable groups, power relations and sensitive research. The chapter ends with a concluding discussion.

Fieldwork in development settings

Ethical concerns are based on the idea of what we as researchers ought to do (or not to do) while doing research or writing. Top-down models, and the idea of modernization of the field being studied, initially dominated development research. The view at the time was that the researchers knew best and that they were in the field so as to develop the less developed world. The '*subjects*' in the field did not have a voice of their own and what was thought to be appropriate by the researcher was also considered ethical. Since then concerns in ethics in development research have grown in importance, as has the involvement of the field in collaboration, participation, formulation of questions and research design. The '*subjects*' in the field are no longer only present in data collection (Desai and Potter, 2009:25-26).

Ethical issues and decisions are concerned with what is right or fair in relation not only to the participants in the research, but also to the project itself and its workers and sponsors. These decisions depend on the values of the researcher and his or her community as well as the amount of control the researcher has over the project (May, 2008:58-59). As researchers we never enter the field as a blank piece of paper; our own social, political and religious backgrounds may influence the choice of research, terminology, method etc. Additionally, as individuals we have different positions, for example as a woman, parent or religious believer, and this influences the way in which we interpret the world around us (Bergström and Boréus, 2005:319). To what extent are ethics culturally or contextually relative and do ethical issues change over time? These two questions are raised by Ringheim (1995:1691).

One line of argument is that ethical rules are contextualized since they are built on religious and philosophical beliefs and therefore the ethical expectations may vary between researchers and those studied when research is conducted in a foreign setting (Christakis,

1992:1080). Values are culturally constructed which probably influences the researcher's data collection, adapting it to local values and sensibilities (Graffigna et al., 2010:342-343). Sayer (2008) points to the spatial difference of ethical principles and that what may be appropriate within one social organization or relationship such as markets, states, voluntary organizations or friendships, might not be so within another. Therefore, different social relations require different ethical principles, and social organizations are influenced by history and geography, for example through economic, cultural and political influences on hierarchies and boundaries. To a varying degree people are physically and geographically constrained to a particular space. Furthermore, Sayer highlights that individuals may sometimes play different roles, one '*outside*' identity in representing a particular group when being in the field and one private identity (Sayer, 2008:180-185).

From 'do no harm' to 'do good'

One way of justifying a piece of research is by applying a risk versus benefit calculation in which the potential benefits to the participants in the research must always justify any possible risks. One definition of the concept of '*do no harm*' to participants in research is that the research "*must not jeopardize the psychological well-being of the individual who participates, or put the individual at risk of social ostracism or repercussions from family or community*" (Ringheim, 1995:1692). Some researchers argue that ethics within social research includes more than not doing any harm to the individuals. It includes social responsibility and the possibility of bringing about improvements (Schoepf, 1991:749). Fieldwork in a foreign setting, in order to be ethical, "*is about building mutually beneficial relationships with the people you meet in the field and about acting in a sensitive and respectful manner*" (Scheyvens and Storey, 2003:139). It involves the potential of empowerment and to '*do good*' towards the field and maximizing the merit (*ibid*. and Peled, 2010:22). By this statement the researchers' ethical obligations are no longer only to '*do no harm*' to the people in the field context but actually to '*do good*'. Do western universities and their representatives have special obligations when conducting field research in a developing country? And if so, is this possibly related to the vulnerability linked to today's relative poverty and the historical world hierarchies? Is today's need to '*do good*' when conducting research in a development setting a way of returning something or reimbursing previous generations' encroachment and search for economic profit?

Informed consent

When ethical concerns are discussed in relation to fieldwork, the attention of researchers is mainly drawn to the aspects of informed consent and anonymity for the participants (see for example Corbin and Morse, 2003:341, Nyambedha, 2008:773 and May, 2008:60 regarding informed consent and DiCicco-Bloom and Crabtree, 2006:319 and Ringheim, 1995:1693 regarding anonymity). Informed consent means that only after we have explained to the people involved what it is that we are doing and what we intend the outcomes to be, may we begin to carry out our research. When the research involves sensitive

topics the informed consent will most likely include a part about anonymity (Desai and Potter, 2009:26). If confidentiality is not guaranteed individuals may refuse to participate in the first place or adjust their answers (Ringheim, 1995:1693). As a PhD-candidate not being familiar with the traditional way of carrying out development research, the issues of informed consent and securing the participants' anonymity seems to me so obvious and as a natural part of the process that I might not even consider it as an ethical issue. Having said this, I do not want to diminish the practice of informed consent and anonymity, it is certainly important and necessary and it is something that obviously has to be done before starting interviews and focus groups and therefore I consider it more of a practical matter. In my opinion, these two issues do not raise any questions to the researcher as to whether or not they should be carried out – they should. However, some researchers raise concerns in relation to the informed consent. Mabunda (2001) suggests that an informed consent may be adapted to a local culture and literacy level before being used or that the community leader might be used as a mediator. On the other hand, by entering a research project through the village leader, someone might feel pressured to participate in the research against their will (Mabunda, 2001:113). Schoepf (1991) and Ringheim (1995) agree that the community leader might give the informed consent in development settings since individuals with low education are unlikely to make an informed decision and fully understand the possible risks and benefits with participating in a specific study. The lack of the concept of '*the individual*' in many settings also needs to be taken into consideration. It is, however, assumed that the community leader acts on behalf of the community members (Schoepf, 1991:758, Ringheim, 1995:1693-1694).

Sources of funding

Research in development settings involves high costs and the researcher is dependent on funding. The possible ethical dilemma then might be that research results do not correspond to the beliefs of the sponsors, with the possible risk of the researcher not being able to obtain further research funds from the same source. The issue is then whether we want to publish our results knowing the possible future problems of funding or if we manage to formulate the result in a way that avoids revealing the data that might be problematic. Fortunately, since most sponsors of development research are research councils or governments and not multinational companies or local government-controlled agencies this is unlikely to happen to the individual researcher. Instead it may have long-term consequences for the people in the research field and therefore any unexpected results ought to be resolved in the context itself (Desai and Potter, 2009:27). As I understand Desai and Potter, certain findings are not likely to influence possible future research funds for the individual researcher; they may instead influence possible future research and/or international aid that is related to the researched and/or the geographical context itself. Is this not more of an ethical concern? To be precise, possible future research and/or international aid related to the researched subjects or the setting itself might be in danger while the researcher faces limited risks in obtaining future research funds.

Vulnerable groups

The researcher ought to be sensitive when carrying out research that involves marginalized and vulnerable groups so as not to be oppressive and for the study not to be a self-serving exercise. Vulnerable groups may include women, children and ethnic minorities, as well as poor and physically or mentally disabled individuals. Members of these groups are more often found in less powerful positions even if they are not universally considered to be oppressed. This is not to say that research should avoid these groups but rather that the researcher ought to reflect on his/her own motivation for the research, conduct it with a sensitive approach and also, according to Scheyvens and Storey, (2003:167-168) ensure that the research itself is more than an exercise in the interest of the researcher. Even the concept of vulnerability is complex. Usually it is associated with dependency, fragility, victimization, insecurity and risk. However, Nyambedha (2008) argues that these concepts themselves are not only difficult to define but also depend on their social context. The vulnerability due to HIV and AIDS is often increased by poverty and the inability of the extended family to care for their relatives (Nyambedha, 2008:721). The main group to be interviewed and observed in this project is comprised of, not only relatively poor, but also vulnerable people due to their illness; thus, the project certainly needs to be carried out in a sensitive manner.

Should participants from development settings be considered vulnerable merely because they are found within this context? Appeals such as the following tend to give that impression: "*If you are embarking on research in the developing world it is particularly important to be aware of your privileged position in terms of wealth, education and so on, in relation to those you will be working with, and to recognize that your research is embedded in the context of colonialism*." (Flowerdew and Martin, 1997:114). To me it seems important to make a distinction between, on the one hand, the awareness of differences and that we are privileged but, on the other hand, not to turn the participants or the people we meet in development settings into victims and treat them as such.

Perhaps this discussion on vulnerable groups should be altered – are we as researchers not supposed to do research just because a target group is more vulnerable than others? A research project has the possibilities to make the voices of these people heard in a larger context, something that might not happen otherwise and could benefit the researched themselves as well as the area and other comparable groups elsewhere. Besides, who is to decide who is vulnerable and who is not? However, is our obligation then completed when their voices have been heard? Not according to Peled who argues that it is an ethical obligation for us as researchers to act upon research results on behalf of vulnerable groups (Peled, 2010:23-24).

Power relations

All research includes dimensions of power in various relations; between the researcher and the researched, the rich and the poor, men and women, the educated and the illiterate and so forth. In order to be able to understand the local power relations the researcher needs to become familiar with the studied setting in the research context. In addition, as a foreign researcher one often depends on local assistants such as translators and gatekeepers and it is important to remember their power relations within the research context. Furthermore, all researchers have an agenda of their own, ranging from an actual interest in the research context to a wish to demonstrate a specific phenomenon (Desai and Potter, 2009:27-28).

Power relations between researcher and researched

A primary power relation is the one between the researcher and the interviewee and/or the observed group, harbouring different aspects. One aspect is the power relation that comes from me being a young educated female from one of the wealthiest countries in the world, interacting and carrying out a study with people from one of the poorest countries in the world. Having these aspects in mind, the power relation is to my advantage. On the other hand, for me as a PhD candidate interested in learning about the families' ways of handling formal as well as informal education for their children I depend on families and their time to let me interview them and in that sense I may feel that they are in power since they, of course, have the right to say no to, or terminate an interview. In relation to this aspect there is also the issue of my asking for the families to take time from their everyday life for an interview without me being able to give anything in return at the time of the interview. If I did pay the families more than compensation for travel costs or lost wages to agree for an interview, I might question their reason for agreeing to participate as well as their answers.

According to Desai and Potter (2009), with regards to power relations, the researcher needs to be aware of the concepts of validity and reliability, to be context-sensitive and to be honest about the research interests as well as about how these interests affect not only the research itself but also the relations towards the people of the research context (Desai and Potter, 2009:28). The researcher "*should be aware of any social, political, legal, and economic complications that might result from having participated in research and take measures to safeguard against these*" (Corbin and Morse, 2003:349). When conducting in-depth interviews it is important to rapidly develop rapport, which includes the establishing of a safe and comfortable environment, as well as trust and respect for the interviewee (DiCicco-Bloom and Crabtree, 2006:316). To some extent a focus group interview may alter the power relation since the individuals, when being present in a group, might feel more safe and comfortable and since the facilitator does not have the same measure of control as in an individual interview (Owen, 2001:653).

Corbin and Morse (2003) draw attention to the power imbalance between the researcher and the researched in favour of the latter, in the sense that, at least with regards to unstructured interviews (and to some extent semi-structured interviews), it is the interviewee who decides what to include in the conversation and what to reveal, and the

researcher is not the central actor (Corbin and Morse, 2003:339). A similar argument is put forward by Cassell (1980) who claims that the power relations that exist while conducting fieldwork are in favour of those who are studied since they are the ones who are in control of the research setting and it is not unusual that the researcher is dependent upon the community for shelter and food. Furthermore, those who are studied can influence the interaction, they may leave or refuse to enter the interaction and they have the power to disturb the research. Interaction flows in both directions and the power relation is on a more equal basis than for example in biomedical research (Cassell, 1980:29-31). This contradicts Rapley's argument that it is the interviewer who is the one who always guides the conversation, deciding what to ask and what to follow up upon (Rapley, 2004:17-21). Yet, even though it is the interviewer who decides on the questions it is still the interviewee who decides on what to reveal.

Apart from the above mentioned arguments, I have not been able to find any discussion on the reversed power relation that is in favour of the researched. Actually, I think in some way that the interviewees do possess a certain degree of power, perhaps a different kind of power but, nonetheless we, as researchers, depend on them and the time they take to share their stories and knowledge; they can decline to participate and, at any time during the research, they can decide not to participate anymore. Moreover, they can adjust the truth or give untruthful answers.

Is it possible that the dimensions of power have been too narrowly discussed? There will always exist power relations, even in your everyday life you are surrounded by various power relations so why then should power relations between the researcher and the researched be of such a specific nature? I am no longer sure this should actually be an ethical issue as long as all participants are treated with respect and informed about the research and their possibilities to not participate as well as the possibility of withdrawing during the research. The most important thing is that you are aware of your own interests in the research and the possible consequences for the participants in the field context and, furthermore, do not use the results in any way that could be of a disadvantage to the respondents. This might sound like a simple task but is almost certainly one of the most difficult issues to handle as it means trying to foresee possible outcomes and consequences of the research and your presence in a foreign context.

Expectations

Research in Development Studies is always associated with expectations of future interventions, either good or bad. Within social research, paying participants is always considered to be bad practice as it may bias the interviewee and his or her answers (Mikkelsen, 2005:343-344). If a gift should be considered, a gift to the community is to be preferred rather than to individuals (Desai and Potter, 2009:30, Mikkelsen, 2005:344-345). Usually the participants want something in return; it may be validation, information or being able to help others. By "*a sense of presence or of being with the participant in the story*" the researcher can give something in return to the interviewee (Corbin and Morse, 2003:342). Ringheim (1995:1695) questions whether an impoverished individual really

has an option not to participate in a study when monetary compensation, which is more than reimbursement for lost wages and travel cost, is received.

The gain of fieldwork is believed to be in favour of the researcher unless the researcher is focusing on policy-oriented issues or is previously linked to institutions that will act upon the results. Published research may to some extent contribute to policy change over time. However, it is argued that just because the researcher gains more than the participants this does not make the research itself unethical. Instead, the gained knowledge and understanding of the field may be used later on when the researchers have the possibilities to influence development policy and practice or influence others working with development issues. Apart from this it is being argued that the fieldwork itself and the presence of the researcher may contribute positively by showing the participants that their information is valuable, it can raise people's self-esteem; poor people who are used to being ignored are visited and, in-depth interviews can sometimes have a therapeutic value. Furthermore, when conducting research in a development setting you as a researcher may serve as a distraction from the boredom of everyday life and even as a source of amusement (Scheyvens and Storey, 2003:155-157, 175 and 191).

Sensitive research

Research on health-related issues intrudes on participants' privacy and the possible disclosure of participants' health status may lead to social stigmatisation and even legal sanctions for engaging in risky behaviour. Therefore, the interest in participating in a study on health-related issues might be limited (Melton and Gray, 1988:60). Worldwide 33.4 million adults and children are currently living with HIV, half of them are women and 2.1 million are children under 15 years of age. A majority of the total number of HIV infected, 67%, live in Sub Saharan Africa (UNAIDS, 2009:6, 21). In many African settings there is a social stigma surrounding HIV and AIDS (Kalipeni et al., 2004: 293, 311, Evans and Thomas, 2009:111) and therefore the cause of death might not always be openly spoken about in some families and communities. The death might instead be referred to as depending on some other disease. On a household level the impact of a single AIDS-related death can have the same consequences as a fatality of any other disease but it is the great number of people affected by HIV and AIDS that causes disruption for both families and communities (White and Robinson, 2000:15, 40). Furthermore, the social stigma surrounding the illness in combination with geographical disparities in treatment and care service mean that the majority of HIV affected families in Tanzania do not receive support. This is especially notable in rural areas. Lack of social protection, high hospital fees and overburdened health facilities often result in home-based care for the ill person, work that is mainly carried out by women and girls. The stigma and the lack of external support may result in emotional distress for the ill person and their caretaker as well as in isolation (Evans and Thomas, 2009:111-113, 118).

What might then be characterised as sensitive research? According to Owen's definition, sensitive research is "*when the people being studied are powerless or disadvantaged, where there*

is an opportunity for people to feel exploited or degraded, or where the subject matter relates to personal experience" (Owen, 2001:656). This broad definition on sensitive research raises questions like: Powerless in relation to whom? Disadvantaged in relation to whom? Is there not always a risk that people included in research feel exploited or degraded? One might wonder if the relations in the first two questions are between wealthy and poor settings – then *all* western research in poor settings would be sensitive just because of their location. Is that really the case?

When it comes to research on sensitive topics such as spousal abuse, major illness or loss, or research that is incriminating or embarrassing, review boards are concerned about the possible psychological harm or distress. Corbin and Morse (2003) argue that there is a potential risk of psychological distress and that it is impossible to say that it never happens, even though it is rare. Most likely this is due to the fact that participants themselves decide what information they want to reveal, how much information they want to reveal and when to give the information. Furthermore, the authors point to the fact that those who do not want to talk about the topic or are emotionally fragile will presumably not agree to participate in the study in the first place. One risk in relation to sensitive topics is the issue of anonymity; the risk of any harmful consequences for the participants must be avoided. This risk is being minimized by paying attention to the handling of records as well as to the concealing of information that might identify individuals. In addition, Corbin and Morse argue that talking to a researcher on the topic ought not to be more distressing than talking to family members or friends. On the contrary, the researcher may be more interested in the story of the interviewee and therefore pay more attention (Corbin and Morse, 2003:336-338, 344, and 351).

The risks of research on sensitive topics depend on the skills of the interviewer in recognizing signs of distress and responding to them, changing the topic or adjusting the interview according to the situation, which could mean terminating and/or postponing the interview (Corbin and Morse, 2003:343, 351).

Considering my own project I believe the issue of stigma surrounding HIV and AIDS is my main ethical (and practical and methodological) concern. Not only because I may have doubts as to which families are affected and which are not if they are trying to hide this fact from the surrounding community, but this will be more of a methodological issue in relation to finding interviewees. Instead, I am more concerned by the fact that my interviewing certain families will point to which families within a village or community are actually affected by the illness just by me visiting them and this might have consequences for these families after my departure. And, of course, I cannot ask the families with sick family members to travel elsewhere to conduct the interviews and thus be pointed out. I consider a way of overcoming this situation by conducting a short questionnaire with *all* households within a village and to stay somewhat longer to conduct interviews with certain families. In this way, families would not be singled out and further stigmatised.

Conclusions

In this chapter the issues of fieldwork in development settings, vulnerable groups, power relations and sensitive research were discussed. Somehow I find myself overly concerned about ethics in relation to research and I have started to reflect more on what the truly ethical issues are and what could be referred to as more of practical and methodological matters. We ought to be able to determine which the truly ethical concerns are, and which issues are of a more methodological nature. Likewise, we must be open to the fact that an initially practical or methodological problem might turn into an ethical problem. There is no point in creating these problems for ourselves but I believe that the awareness in advance of possible concerns could make us more sensitive and humble to the context and the participants. The reason for emphasizing the awareness during fieldwork is that a higher awareness of consequences in the field and for the participants will hopefully make us act accordingly and try to avoid these negative outcomes, which could make all the difference between doing harm in the field and '*do no harm*'. The possibility to '*do good*' is certainly more difficult to predict even though some researchers have highlighted the therapeutic value when the researched are given time to talk and be listened to.

In the literature it is argued that research in a development context should not only '*do no harm*' but it should actually '*do good*' and have beneficial outcomes. This argument struck me with surprise. First of all, how am I (or anyone) to guarantee possible beneficial outcomes not knowing what the result itself will show, and not actually knowing who might use it for developing purposes. Secondly, why is it that just because it is in a development setting there have to be beneficial outcomes included?

One does not need to travel far to come across various aspects of these topics so why then pay extra attention to research located in a development area? All research, no matter the geographical location, ought to keep a high ethical standard which goes beyond the practice of informed consent and securing anonymity for its participants. It needs to be sensitive of what the research may or may not do to the researched. When it comes to developing countries, where the context is so different from ours, it is the responsibility of the researcher to be sensitive to the local context and to be aware of the purposes and consequences of his/her study. To a degree, current research in these countries may be viewed upon as a new form of colonialism; the western army of researchers go to the developing world digging for information for a few months and then leave to go back and carry on with their careers. And what is left in the field? On the other hand, I believe nearly everyone doing fieldwork in development settings aims at providing more knowledge about certain phenomena and possibly influence the policy planning, structures or flow of aid. Furthermore, I am not sure why we need to be extra careful while doing fieldwork in a foreign setting than if we were to conduct it in our own settings. Should we not always keep a certain ethical standard and awareness throughout our research – no matter what the geographical context is? Particular to the foreign context therefore is for the researcher to try to gain awareness of the local culture and customs and to understand that ethical principles and power relations may differ, and furthermore be aware of possible future consequences for the participants.

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Theory as methodology

The studying of cultures – measuring or immersing?

Susanna Magnusson

Abstract

The aim of this chapter is to discuss two different approaches to the studying of cultures – the etic and the emic approaches. The two different approaches reflect not only different views on how cultures ought to be studied, but also to some extent different views on the nature of cultures. The etic approach suggests that it is possible to study cultures from "the outside" and that it is feasible to compare different cultures along standardized, universal and objective dimensions. This perspective unfolds most clearly when using a quantitative methodological approach, such as in the works of the Dutch anthropologist Geert Hofstede. Hofstede is one of the most influential researchers on cultures within certain academic areas, such as managerial cross-cultural studies. The emic approach, on the other hand, claims that each culture is unique, and should not be compared to other cultures. According to this approach, cultures can only be properly understood "from within" and to increase the understanding of a certain culture, qualitative methods such as "thick description" as proposed by the American anthropologist Clifford Geertz, are appropriate. In the chapter, I give an account of theoretical and methodological perspectives implied by the two different approaches and point out strengths and weaknesses. I finally argue that both approaches are justified in cultural research and also that they are possible to combine. However, as a researcher, it is necessary to thoroughly reflect on the basic theoretical and methodological assumptions embedded in the different views on culture.

Introduction

The discipline of cultural anthropology is based on the concept of culture (Geertz, 2001). The concept stands at the very core of several academic fields, not just anthropology, but also ethnology and ethno-geography. Since the 1980s, "culture" has been given great significance in other academic areas such as business and organizational studies (as seen

in, for example, research on "corporate culture" and "business culture"). The concept of culture is complex, full of nuances and is applied in many different ways, often without displaying the specific meaning. Because of this, the concept sometimes contributes to obscurity rather than elucidation. Søderberg and Holden (2002), for example, argue after having reviewed business and organizational literature, that the concept of culture is often used without any closer attempt to reflect on the different ontological and epistemological standpoints embedded in the concept.

In this chapter I focus on the elusive concept of culture and contrast two different main approaches, the emic and the etic approaches, to the studying of cultures. My main claim is this: different ontological assumptions about the nature of cultures tend to give rise to different methodological positions, and research methods. These methodological positions and research methods in turn have an impact on the research questions and consequently, on the results that the researcher achieves. It is important to be aware of how different ontological standpoints on what culture "is" play out in different epistemological positions and research approaches. I hope that this chapter may contribute to a heightened awareness of ontological and epistemological issues, and consequently inspire to greater clarity and consistency amongst students aiming at studying cultures in different contexts. In this chapter I approach the concept of culture in a general sense; however, the distinction between the two approaches is also applicable to the studying of cultures in more narrowly demarcated senses, such as "organizational cultures" or "youth cultures".

I start by giving a brief introduction to the wide range of interpretations of the concept of culture. Then I contrast two different methodological approaches to the studying of cultures; the etic and the emic approaches. Further, I illustrate the differences between the two approaches by comparing the cultural theories and research of the Dutch anthropologist and organizational sociologist Geert Hofstede and the American anthropologist Clifford Geertz. In the next section I present and discuss criticism towards Hofstede and Geertz and I also discuss general strengths and weaknesses with the two approaches. In the conclusion I elaborate on the question of whether the researcher has to choose one approach exclusively, or if the two approaches are possible to combine.

Definitions of culture

Claiming that the concept of culture is complex and bearing many different, sometimes contradictive, meanings is rather like stating the obvious. As early as in 1952 the American anthropologists Kluckhohn and Kroeber reported the existence of 168 different definitions of the concept of culture and there is no sign of decrease since then (Søderberg & Holden, 2002). To start with, culture has at least two different distinctive meanings: "high" culture such as literature, art and music, and culture in the "soft", anthropological sense (Hofstede, 2001): this paper deals primarily with "culture" in the latter sense. The term culture in its anthropological sense is a generic term to describe, amongst other things, different ways of life, central norms, ideals and values, specific traditions, ways to solve problems, ways in which reality is perceived or how something is shaped and expressed. It can be used to

describe secluded social categories, specific action or the result of these actions (Öhlander, 2005a). Sometimes it is used as a synonym for civilization, nation, people or society (León Rosales, 2005). There seems to be a tendency to use the concept in relation to larger groups, on a societal level (Gudykunst & Kim, 1997). This tendency to equate culture with nation (one culture – one nation) is heavily criticized in modern anthropology and parts of more recent studies on intercultural communication (Brumann, 1999; Jensen, 2008; León Rosales, 2005; Saint-Jacques, 2009). The whole concept of culture, as well as its modes of application, has been heavily debated in different areas of academia, especially within anthropology. It has been criticized for suggesting "an inordinate degree of boundedness, homogeneity, coherence and stability" (Brumann, 1999, p.1). It has been argued that it is used too deterministically, as some kind of an all-embracing force and of being linked with ethnicity and race in an erroneous way (Appiah, 2006; Brumann, 1999; Kuper, 1994; Sen, 2007; Öhlander, 2005b). Obviously, the concept of culture has the potential of working in two diametrically opposed ways, both as a liberator (from biology, from what is considered "normal" and "natural") and as a mental prison (when deterministically equated with essential characteristics, "race" or "ethnicity") (León Rosales, 2005; Öhlander, 2005a). As a reaction to the deterministic and essentialist version of the concept of culture, more complex perspectives on the nature of cultures have evolved in the last decades. These are postmodern and critical perspectives that emphasize that a culture does not necessarily have to imply stable and shared values and norms that are constants in people's minds, but rather that culture is seen as shared knowledge and meaning that is constantly being negotiated between people (Jensen, 2005). Some critical scholars would even say that culture is primarily a tool of domination and "a site of struggle and contestation" (Moon, 2010, p. 38).

The etic and emic approaches to the studying of cultures

Looking back at the history of anthropology, there are two broad approaches as to how researchers should tackle the studying of cultures: in an emic or an etic way. These concepts have been called "…perhaps the two most crucial constructs in the study of culture" (Aneas & Sandín, 2009, p. 5). The terms originally derive from the linguist Kenneth Pike, who argued that phonetics stands for the classification of bits of sound according to their acoustic properties while phonemics stands for the classification of bits of sound with regards to their specific meaning and function within a certain language (Emerson, 2001; Hall, 2002; Pike, 1954). While an etic approach lends itself to comparisons between different languages, an emic approach concentrates on the unique aspects and meanings in a certain language (Morris, Leung, Ames, & Lickel, 1999). Despite its linguistic origin, the terms emic and etic are used most frequently to describe two different types of research positions in the studying of cultures. Not only do emic and etic researchers tend to approach the subject of culture from different methodological positions, but they also tend to express different notions about the nature of cultures (Morris et al., 1999).

The emic approach in intercultural research

The emic approach stresses the unique and specific aspects of every different culture. The underlying assumption is that it is impossible to compare different cultures. Each culture is seen as distinct, in characteristics and evolutions, each culture has its "particular configurations of values, to be grasped intuitively" (Kuper, 1994, p. 539). The emic approach emphasizes "the insider perspective", using concepts and categorizations that are employed naturally by the members of the individual culture being studied. There is often no ambition to compare different cultures to one another, but rather to gain a deep understanding of the local knowledge and internal characteristics of a certain culture (Berry, 1999; Emerson, 2001; Young, 2005). This in-depth understanding is ideally reached through immersing in the local culture and by making interpretations. An emic analysis focuses on all the different ways in which people in a single culture make sense of and organize their lives. The local context and the meaning of constructs, without the researcher imposing pre-defined constructs, is seen as crucial (Aneas & Sandín, 2009; Niblo & Jackson, 2004).

The etic approach in intercultural research

The etic approach, on the other hand, stresses the comparable and general aspects of cultures. The underlying assumption is that it *is* possible to compare different cultures in a multitude of aspects; that they do, in fact, have some fundamental characteristics in common which make comparisons valuable. The etic approach studies behaviour from a position outside of the system, the criteria are often considered universal, and often (but not always) several cultures are involved in the comparison. The etic view assumes that there is a universal standard, that all cultures are, in a way, only variants of a single type "one in origin and in destiny" (Kuper, 1994, p. 539) and that we can, in fact, understand single cultures a great deal better if we make comparisons. The etic approach aims at constructing larger overarching systems of comparisons, using concepts constructed in advance that do not necessarily derive naturally from the culture studied (Emerson, 2001; Lee, 2002; Silverman, 2006).

Comparing the emic and the etic approaches

To some extent, the emic/etic division represents a distinction between different philosophical and scientific paradigms and strategies (Harris, 1976). The choice of one approach or the other implies following in the footsteps of a certain research tradition. While the emic, "inside"-perspective has its roots in the tradition of psychological studies of folk beliefs, the etic, "outside"- perspective follows in the tradition of behaviourist psychology and certain anthropological approaches (Morris et al., 1999). These approaches tend to link cultures to external factors, such as economy, regulations, climate etc.; factors that might not always be distinct and tangible to the cultural insiders. The divide between the two approaches still exists in contemporary anthropology, between interpretivists and comparativists – a divide that I will return to later, clarifying and illustrating the differences

by pointing at two influential researchers representing each perspective – Geert Hofstede and Clifford Geertz.

Depending on the perspective, there tend to be differing views on how to understand the concept of culture. Emic researchers tend to regard cultures as an integrated whole, an interconnected system that cannot be understood by studying parts of it separately, by isolating different components (Morris et al., 1999). Researchers taking an etic perspective, on the other hand, make the exact opposite assumption, that it is, in fact, possible to distinguish separate dimensions of a culture and to state hypotheses about their origins and consequences. These partially differing views on the nature of cultures also affect the kind of research questions posed by the researchers, as well as the research methods employed. The most important issue for an emic researcher is the search for meaning, for understanding the significance of a certain phenomenon within a certain culture. The perspective of the insiders of the culture and their self-understanding is an essential part of this research. This means that long-standing and wide-ranging observations, immersing in the local culture to understand the variety of ways in which the "insiders" perceive their daily lives, become important for researchers (Morris et al., 1999). In other words, emic researchers normally have an inductive approach and employ qualitative methods such as participant observation along with interviews in one or only a few settings. Another possible method is doing content analysis of texts, movies, music, etc. For the etic researcher in contrast, the primary question is not what a phenomenon means, but rather how many of these phenomena there are, how often they occur, what causes them, and what are their consequences. Etic research is more likely to involve multi-setting surveys, either by means of brief observations and interviews, or, more likely, by questionnaires[5]. While emic researchers hold an interpretivist stand, aiming at understanding sense-making of the actors and the construction of meaning in a certain culture, an etic researcher tends to hold functionalistic assumptions, looking for causal relations, in a deductive way determining the impact of cultural variables on certain forms of behaviour across different cultures (Morris et al., 1999; Samovar & Porter, 2003). In this way, the two perspectives tend to work in a self-reinforcing way: the emic perspective inherently leads to results focusing on causes of phenomena that are internal and local to the cultures being studied, while the etic perspective captures relationships between external structural variables and observable behaviour. So the circle is closed.

It is important to point out that the emic/etic division in practice is a continuum. However, as Morris et al. point out, the two concepts tend to be regarded as a dichotomy that "has played a central role in the metatheory debates in many social science disciplines" (1999, p. 782). In this metatheoretical discussion, the concept of etic could be linked to an ontology viewing social entities as physical and as the natural unit of analysis – the ontology of *being* (Chia, 1995). The emic approach, on the other hand relates closer to a postmodern line of thought regarding the relationships between agents as the proper level

5 However, as Morris et al. (1999) point out, the connection between perspective and methods is not absolute; quantitative techniques may and have been used in emic research, and qualitative methods are sometimes used to support findings from an etic perspective.

of analysis – the ontology of *becoming*. The most significant differences between an etic and an emic approach are summarized in the table below.

Figure: comparing the etic and the emic perspectives

	Etic	**Emic**
Scientific paradigm	Modern/functionalistic	Postmodern/interpretivist
View on culture	Cultures are possible to compare, comparisons are valuable for the better understanding of each culture	Each culture is distinct and impossible to compare to other cultures
Perspective	Outsider perspective, using predetermined categories	Insider perspective, using insider categories and concepts
Analysis level	Looking for causal relations determining the impact of cultural variables	Interpretations of local actors' sense-making and interpretations
Typical research questions	How many? How often? What causes it? What are the consequences?	What does this mean to the "insiders"? How do they make sense of their experiences?
Preferred methods	Quantitative: questionnaires analysed statistically	Qualitative: observations, interviews and fieldwork

How the emic and etic perspectives play out in actual research – Hofstede vs. Geertz

I wish to illustrate the consequences of the emic and the etic perspectives respectively by contrasting two very different researchers: the Dutch organizational sociologist Geert Hofstede and the American anthropologist Clifford Geertz. They both have a profound interest in the studying of cultures; however they have contributed to the field in completely different ways. Geert Hofstede is a strong advocate for a distinct version of the etic perspective, having performed large-scale cross-cultural surveys. He adopts realist and deterministic assumptions (Williamson, 2002), focusing on universal dimensions of culture, on culture as a directive force and on the effect of culture on behaviour (Singh, 2004). Clifford Geertz, on the other hand, is without doubt one of the most influential modern anthropologists, and represents with his studies an interpretive, emic approach, using qualitative methods. Below I present their views on culture and cultural research followed by an account of some of the criticism they have received.

Geert Hofstede – culture as "collective programming"

Geert Hofstede (1928 -) has had an almost canonical impact on the studying of cultures in some academic areas, such as cross-cultural organizational and managerial studies

(Søderberg & Holden, 2002). He is joined in the functionalistic paradigm by authors and consultants such as Fons Trompenaars (Trompenaars, 1993). However, Hofstede is by no means the more influential of the two. At one point he was one of the most cited sources in the Social Science Citation Index (Hofstede, 2001). Even if he is almost unmentioned in standard works of anthropology, he has made an influential contribution to the etic strand of studying cultures. He defines the concept of culture in the following way:

> [Culture is]… the collective programming of the mind that distinguishes the members of one group or category of people from another. (Hofstede, 2001, p. 9)

Hofstede (2001, p. 9) argues that the core of this "collective programming" is values – "a broad tendency to prefer certain states of affairs over others". Values, the inner core of each culture, are seen as entities that can be classified, measured and correlated in a quantitative way. Culture, Hofstede argues, is something fairly stable and homogeneous, being transmitted from generation to generation and something that clearly demarcates different groups. Furthermore, Hofstede argues that nation states are valid empirical units, hence the use of the concept "national culture" (Hofstede, 2001; Hofstede & Hofstede, 2005).

In the 1960s and 1970s, Hofstede conducted a large study in cooperation with IBM worldwide. The aim of the study was to detect work-related values in different countries. IBM conducted an international employee attitude survey programme between 1967 and 1973. Two survey rounds produced answers to more than 116,000 questionnaires from 72 countries. The analysis focused on country differences in answers to questions about employee values. Additional data was collected and through theoretical reasoning and statistical analysis four different dimensions of culture were revealed, dimensions along which dominant value systems in 50 countries could be ordered, according to Hofstede. The dimensions were: *power distance* (ways of dealing with the distribution of power), *individualism* versus *collectivism* (ways of dealing with the relation between the individual and the collective), *uncertainty avoidance* (ways of dealing with uncertainty and ambiguity) and finally *masculinity* versus *femininity* (ways of dealing with gender roles). Later a fifth dimension, *long-term* versus *short-term orientation* (attitudes towards time) was added (Hofstede, 2009). According to Hofstede, these dimensions reflect basic problems that any society has to cope with but for which the solutions differ. In that way, they are more or less universal and affect human thinking, organizations and institutions in certain predictable ways.

Criticism against Hofstede

As previously mentioned, Hofstede has had a great impact on the studying of culture within some academic areas. Søderberg and Holden (2002) even claim that his view, in some areas, has become the principle point of departure from which all other theories on cultural differences are being compared and validated. The easy accessibility and supposed applicability of his dimensions has made his work very influential among practitioners and teachers in culture and communication. An enormous market for so-called "interculturalists"

(Dahlén, 1997) has evolved, in many aspects building on the theories of Hofstede. The interculturalists are consultants that, in this era of globalization and multiculturalism, are teaching people and organizations how to communicate with people from different cultures by achieving "cultural sensitivity" and learning how to overcome "culture shock" when confronting a "new culture" (Dahlén, 1997).

However, Hofstede by no means stands unchallenged. He has been strongly criticized by researchers within interpretive and social constructionist traditions. Criticism has been raised regarding ontology as well as epistemology. According to some critics, Hofstede is basically mistaken about the nature of culture; the essentialist point of departure, claiming that culture consists of basic assumptions, beliefs and values that are possible to reveal, is deeply erroneous (Søderberg & Holden, 2002). Also, he is claimed to severely exaggerate the stability, national homogeneity and determinism of culture, while neglecting the dynamics, diversity and complexity in social reality. Ailon (2008) argues in regards to Hofstede: "…this concept of culture promotes an image of the individual as merely a passive carrier of a predetermined cultural template" (p. 894). In other words, his distinctive etic and functionalistic position, using positivist epistemology, searching for universal regularities and systematic, causal relationships, faces increasing resistance within the research community. The loudest criticism comes from researchers with a more interpretive or social constructionist bias, researchers that view culture in a more Geertz-inspired – emic – way.

Clifford Geertz – culture as "webs of significance"

Clifford Geertz (1926 – 2006) is considered the founder of interpretive, or symbolic, anthropology. Among other things, he is known for his extensive fieldwork in Indonesia and Bali. The concept of culture, according to Geertz, is "essentially a semiotic one" (2001, p. 56). One of his most famous and quoted statements is: "Believing with Max Weber that man is an animal suspended in webs of significance he himself has spun, I take culture to be those webs and the analysis of it to be therefore not an experimental science in search of law but an interpretive one in search of meaning" (Geertz, 2001, p. 56). To Geertz, culture is not a force, something to which forms of behaviour or processes can be causally attributed. It is not something that one "has" in the form of stable values and beliefs that can be measured; rather collective cultural identity is seen as a process that is dynamic, relational and situational in theory and practice (Geertz, 2001). The studying of culture, according to this view, should not aim at making cross-cultural comparisons based on universals, but rather to find, through interpretations, what is unique, significant and meaningful in each culture. This suggests that Geertz, unlike Hofstede who rests safely within the functionalistic paradigm (Williamson, 2002), is inspired rather by the symbolic interactionist perspective. According to that tradition, cultures are shaped and reshaped through the using of symbols in social interaction. Cultures contain everything that man has created, material and immaterial, and the focus of study is the meanings attributed to symbols, the sense-making process (Stier, 2009). Since human behaviour, according to this view, is shaped from the way people perceive, classify and make sense of the world around

them, the only reasonable strategy is the insider (emic) approach to cultural description. The way of doing this, Geertz proposes, is through "thick description" (Emerson, 2001, p. 33)

Thick description according to Geertz

The foundational task if you want to understand a culture is, following Geertz, to study what he calls "meaningful structures", where subtleties and complexities are not reduced, but seen as important (Emerson, 2001, p. 33). While a "thin description" only describes what someone is doing, a "thick description" is one where you attempt to describe the meanings of behaviour to the involved actors. This is done through different qualitative ethnographic methods, such as participant observation and interviews. To achieve thick descriptions, the ethnographer needs to be in long and close contact with the actors, immersing in the local culture. Geertz demonstrates the difference between thin and thick descriptions with reference to the many different things that a person rapidly opening and closing an eye can signify. It can be an involuntarily twitch, a wink made conspiratorially, or even the mocking of such a wink. A "thin description" only establishes that the eye opened and closed; that is hardly interesting and will lead us nowhere in cultural understanding, according to Geertz. Instead, what we need is a "thick description" that separates twitches from winks and one sort of wink from another (Geertz, 2001).

Geertz contends that ethnographic descriptions are not objective, reproducing reality in an undistorted, literal way. Rather, a thick description is a "complex interpretation of local meanings – the ethnographer's effortful, imaginative rendering of these meanings" (Emerson, 2001, p. 33). The ethnographer realizes that descriptions are soaked in values, theoretical commitments and cultural categories used to observe, classify and describe events. The ethnographer should, however, as much as possible, try to describe the actors by using their terms and concepts, being "actor-oriented". A methodology such as Hofstede's, where you try to isolate different elements of culture, determine their internal relationships and then characterize the system in a general way, is not the way forward (Geertz, 2001). The aim is not to ignore or reduce contextual meanings to get "data" that are "standardized" or "comparable".

The fact that objective, literal descriptions are impossible to attain, does not mean that you cannot tell a good description from a bad one: a good one is one that separates twitches from winks (Emerson, 2001). A "good" description manages to see what is extraordinary in an everyday life, and what is normal and mundane in the exceptional (Silverman, 2007).

Ethnographic thick description is by nature interested in the small, the microscopic. It does not pretend to do studies on a larger scale (Geertz, 2001). Is it then possible to move from a local truth to some kind of general wisdom? My interpretation of Geertz's view on this matter is that he finds it difficult to make generalizations since emic research is so bound to a specific context. And that is the way it has to be, according to him; the more theory constructed, the further away from peoples' lives, from reality, the researcher is brought. At the same time he argues that one has to be able to see that "small facts speak to large issues (2001, p. 70), hence suggesting that some kind of horizontal generalization

is possible, after all. However, according to Geertz, this is not the primary goal of cultural research.

Criticism against Geertz

Geertz is considered one of the greatest authorities in anthropology, and he was extremely influential in anthropology and other social sciences during the '70s. This, of course, does not secure him from criticism. The most common criticism implies that he has failed to take into account "the bigger picture", the social structures of power and inequalities. Keesing, for example, has argued that Geertz ignores the fact that people do not have equal possibilities to create and define meaning (Roger et al., 1987). Those "webs of significance" might not be spun by everyone to the same degree; some are simply caught in other people's webs. Aspects of history, economy and politics are all important parts of a context that tend to be neglected in Geertz's frame of theory (Valerio & Roger, 1987).

On a more generic level, objections and doubts have been raised against the emic approach. One of them concerns the issue of ethnocentricity. Ethnographic, emic research, has the ambition to emanate from the interpretations of the actors and their own sense-making. However, this ambition has often failed and involuntarily research has expressed ethnocentrism. Said, for example, has written extensively about how anthropological research has contributed to exotism and estrangement of other cultures (Emerson, 2001). This kind of criticism, however, has also been directed at etic research, which implies that this has less to do with methodology and more with the fact that ethnocentricity seems to be one of the fundamental difficulties with cross-cultural research.

Another important aspect – quite the opposite of the one of ethnocentrism – in relation to the emic approach is the issue of relativism. One of the departure points for emic research is that a researcher should not judge other people's culture by the standards of the researcher or any other outsider. This relativism may support the idea that an outsider is never allowed to value or criticize what goes on in other cultures. In fact, if you take the issue of "studying from the inside" seriously, this might suggest that a culture can only be properly studied by someone with the "right" cultural framework, someone who has some kind of intuitive knowledge about that culture, perhaps who is a part of the studied culture. These ideas, that are a current trend in anthropology (discussed, for example, by Kuper, 1994), would, taken to an extreme, imply that no-one is allowed to say anything about anyone else – that the only reliable knowledge is self-knowledge.

Conclusions

In this chapter, my aim was to highlight that there are two basic and fundamentally different approaches to the studying of cultures. I have tried to show how these different approaches tend to be related to different conceptions about the nature of cultures. I wanted to show how these play out in actual research, exemplifying by contrasting the etic approach by Geert Hofstede with the emic approach by Clifford Geertz.

It is obvious that each approach leads to different kinds of knowledge with regards to the culture(s) studied. It is also evident that both perspectives have advantages and disadvantages. The etic approach has the advantage of providing a broad perspective about differences and similarities across different cultures. However, it can only provide a crude map that indicates which areas that might be of interest for further investigation. The details, complexities, contradictions and nuances tend to go missing in etic studies (Morris et al., 1999; Williamson, 2002). The emic approach, on the other hand, gives a deeper understanding of how a culture is "a working whole" and not just something constituted of certain static "dimensions", "variables" or "parts". The weakness of emic studies is the distinctive subjectivity of the researcher, where the researcher's choice of what to generalize may distort the picture; also, the insider position might make it more difficult to distinguish key principles (Morris et al., 1999). Furthermore, the presupposition that cultures are "a working whole" can obviously be questioned (Alvesson & Sköldberg, 2008).

The question then is whether a researcher has to choose one of the two approaches. Some researchers argue (although it is unusual) that it is possible to combine the two concepts (Morris et al., 1999; Niblo & Jackson, 2004). According to this view the emic and the etic approaches respectively work equally well, but at different stages of the research programme. It has been suggested that an emic approach is best in the initial stages of research, followed by an etic approach testing hypotheses (Morris et al., 1999). The exact opposite has also been suggested, that an etic approach is best in the beginning of the research process, mapping out the terrain and pointing at interesting areas for further emic investigations (Williamson, 2002).

There is no ground for considering the concepts emic and etic *in themselves* to be irreconcilable. Berry (1999) argues, referring to the works of Kenneth Pike, that Pike considered there to be a tension between the concepts, but not a dilemma. Furthermore, Pike considered emic and etic to be equally important. Pike writes: "Both etic and emic approaches are of great value for special phases of behavioural analysis" (Pike, 1954, p. 11). He argues, as previously suggested, that the etic approach is valuable for students to get a "broad training as to the kinds of behaviour occurring around the world" (1954, p. 11), but that the emic approach helps the more advanced researcher to understand the "life drama" (1954, p. 11); the actors' emotions, interests and attitudes. Looking at it from this perspective, emic and etic perspectives are parts of a symbiotic relationship rather than opposites.

Furthermore, there is no such thing as a purely emic or a purely etic perspective. Pike argues that all descriptions are in a sense etic since there is "no other way to begin an analysis than by starting with a rough, tentative (and inaccurate) etic description" (1954, p.11). In line with this, Geertz also argues that emic research is not purely emic. Since the researcher is an interpreter, even the constructs of the actors become filtered through the concepts of the researcher; hence they are not truly emic. Geertz, in fact, suggests that the emic and etic dichotomy should be replaced by a continuum between "experience-near" and "experience-distant" concepts where the former are emic-like concepts that are used naturally and effortlessly by the actors, and the latter are etic-like concepts employed by the researcher (Emerson, 2001, p. 35). Hofstede, for his part, seems to acknowledge that

an etic approach needs to be supplemented by an emic approach. He writes: "Validating the dimensions is of course not only and not even mainly a quantitative issue. Equally important is the qualitative interpretation of what differences on the dimensions mean for each of the societies studied, which calls for an emic approach to each society" (Hofstede, 2009, p.24).

In the light of this, is it then safe to assume that the two concepts marry well? I believe that the answer to this question depends on the theoretical rigour required in research. Methodology, so it says in the methodological literature, should be consistent with assumptions about epistemology, ontology and human nature (Alvesson & Sköldberg, 2008; Williamson, 2002). The emic and the etic perspectives have connections to different research traditions, and have, at least to some extent, come to represent different views on what culture *is*, how it should be understood. If "culture" according to emic research is not the same as "culture" according to etic research, this means that there are differences in what emic and etic researchers try to get a grip on (or "measure" or "understand"). This may give rise to difficulties in regarding them as complementary rather than competing.

I believe, however, that it is obvious that no one has yet revealed the "truth" about cultures and the studying of cultures. Therefore we must accept and welcome different perspectives on the studying of cultures, both emic and etic, and further be open for a combination of the approaches even if this means accepting a certain theoretical ambiguity. I believe, inspired by Alvesson (2008), that theoretical and methodological rigour, is not the only success factor in research. Important aspects are also those of trying to gain rich empirical material and interpreting it on different levels, micro- as well as macro, and trying to see the material from different perspectives. Furthermore, the empirical material should be used to support arguments in a debate rather than as an absolute truth.

In my opinion, the primary problem with the research made by Hofstede is not the research in itself (even if I inevitably find his deterministic and all-embracing view on culture more than dubious). Rather, I find the fact that his research has been used completely uncritically and in the wrong context by students and researchers all over the world to be more problematic. Perhaps the same could be said about some of Geertz' conceptions, such as the one considering cultures to be "a working whole". I believe both etic and emic research to be important. Students interested in the studying of cultures (such as the cultures of certain social categories or organizations) should first and foremost adopt the research perspective that is fitting for their research questions. However, I believe that it is equally important to have an awareness of, and an account of the ontological and epistemological assumptions that are embedded in the chosen perspective. Even if theoretical rigour may not be the key aspect in cultural research, a student should, for example, think twice before applying etic concepts á la Hofstede to studies with an emic starting point. Regardless of which perspective a student or researcher chooses in the study of culture(s), I believe that it is most important to have a critical mind that does not adopt a certain framework without reflecting.

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Where discourse ends: The discursive and the non-discursive in Laclau and Mouffe's discourse theory and in Fairclough's critical discourse analysis

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Abstract

In this paper, I present and analyse the ways in which two different versions of discourse analysis – Laclau and Mouffe's discourse theory and Fairclough's critical discourse analysis – deal with the distinction between the discursive and the non-discursive. In particular, I focus on the consequences that the two approaches to this distinction have for research methodology. I illustrate my theoretical and methodological discussion with an example of how research on the advance of the Sweden Democrats in the 2010 parliamentary elections in Sweden can be formulated with the help of these two methodological approaches. In a nutshell, I argue that both ways of dealing with the distinction between the discursive and the non-discursive present some problems: in the case of Laclau and Mouffe there is a risk of reductionism, while in the case of Fairclough there is a risk of methodological eclecticism and incoherence.

Introduction

It is often stated that discourse analysis is a method where language is central (e.g. Bergström and Boréus 2005: 305). This, however, does not necessarily entail a linguistic analysis nor does it mean that texts constitute the main object of the study or the primary empirical material. Although in some cases this would be true, it is not the common denominator of

different types of discourse analysis. Rather the centrality of language should be understood on the philosophical – ontological and epistemological – level. That language is central means that the method of discourse analysis is based on a particular view of the role of meaning in the social construction of the studied reality. Thus opting for discourse analysis often means that a researcher not only chooses a method, but also subscribes to a certain ontology and epistemology that recognizes the key importance of language in reaching an understanding of the social world. In this sense, discourse analysis is "a complete package" – to use Jørgensen and Phillip's expression (Winther Jørgensen and Phillips 2002:3) – in which theory and methodology are interlinked.

By resting on the ontological assumption that the discursive plays a crucial role in the construction of the social world, discourse analysis is always in a dialogue with theories that search for explanations of the social in the material sphere. And this dialogue is about where the line between the discursive and the non-discursive should be drawn. In this paper, I will take a closer look at how two different versions of discourse analysis define and deal with the distinction between the discursive and the non-discursive. I will focus on the discourse theory presented by Ernesto Laclau and Chantal Mouffe, on the one hand, and on the critical discourse analysis formulated by Norman Fairclough, on the other. I will present the solutions proposed by these scholars and try to point to advantages and disadvantages thereof. I will pay special attention to what these solutions mean in methodological terms. I am particularly interested in the consequences that different ways of dealing with the non-discursive in the discourse analysis have in practice for research methods.

It is important to stress that the discourse theory and the critical discourse analysis mentioned here are but two approaches on a whole spectrum of discourse analyses, other schools being rhetorical political analysis, interpretative policy analysis, discourse historical approach, discursive psychology and Q methodology, to name but a few. Different versions of discourse analysis build on different ontological and epistemological assumptions and apply different methods; it is thus hard to talk about discourse analysis as one homogenous method. So, even though all the types of discourse analysis have a common ground which is "an overriding concern with questions of meaning and the centrality attributed to subjects on the construction and apprehension of meaning" (Glynos et al. 2009:6), not all of them work on ontological and epistemological assumptions defined in such a categorical way as is the case in Laclau and Mouffe's and Fairclough's theories. Other versions of discourse analysis can thus often be treated more like techniques than as whole theoretical-methodological packages, which means that their application does not lead to any ontological commitments.

My choice of the discourse theory formulated by Laclau and Mouffe, on the one hand, and the critical discourse analysis by Fairclough, on the other, is motivated by the central place given to the relationship between methodology, ontology and epistemology. Opting for one of these approaches entails some important theoretical and philosophical choices. At the same time, the two versions of discourse analysis stand for different approaches to the distinction between the discursive and the non-discursive, which promises an interesting comparison.

The paper is structured in the following way: In the first part, I briefly discuss the roots of discourse analysis. Here, I present the interdisciplinary character of discourse analysis and its relation to the linguistic turn in the humanities and social sciences. I also explain the position of discourse analysis in relation to Marxism. In the second and third part, I discuss the distinction between the discursive and the non-discursive in the theories and methods of Chantal Mouffe and Ernesto Laclau, and Norman Fairclough respectively. Here my aim is to combine the theoretical discussion with methodological questions in order to highlight the consequences that ontological and epistemological choices have on the methodological procedures of a researcher.

Roots and struggles of discourse analysis

Linguistic turn

Historically, discourse analysis has its roots in the linguistic turn in humanities and social sciences (Winther Jørgensen and Phillips 2002:9). One of the people who had a decisive impact on the linguistic turn was the Swiss linguist Ferdinand de Saussure who developed the structural theory of language. The major contribution of Saussure was his focus on the linguistic sign. He claimed that language should be analysed as a system of signs independently of the possible referents. Performed in this way, a linguistic analysis addresses language as a socially constructed structure where meaning is being expressed through linguistic means, such as speech or writing, rather than as a reflection of a certain reality. Moreover, language is but one of many systems in the society where signs are crucial, others being for example rites and customs (Saussure 2006: 17). Another contribution was his distinction between language and speech: the former referring to the system of language (rules and conventions) that has a collective character and the latter to an individual utterance (a single act of use of language by an individual user of the language) (Saussure 2006: 13). Moreover, Saussure formulated a theory of sign as composed of a signifier (sound pattern) and a signified (the concept that the signal carries), the two related to each other in an arbitrary way (Saussure 2006: 67). Finally, Saussure underlined the importance of difference and similarity for the creation of meaning. These four contributions were particularly important when Saussure's theory was translated, under the name of structuralism, from linguistics into other disciplines, such as literary studies, psychoanalysis, anthropology, and other human and social sciences.

While retaining the idea of the arbitrariness of the relation between language and reality and of the emergence of meaning through difference, post-structuralists modified the understanding of the relation between language and speech so that it could provide for change. The main figure of post-structuralism in general and of discourse analysis in particular is Michel Foucault, whose theories of archaeology/genealogy of knowledge and

his analyses of sexuality, prison systems and madness throughout western history serve as prototypes of discourse analysis.

At the same time as linguistics was influencing the humanities and social sciences, a linguistic turn took place in the field of philosophy. Here the main protagonist was Ludwig Wittgenstein who introduced the concept of 'language-games'. With his famous example of a builder and his assistant communicating about the building stones while building, Wittgenstein illustrated how meaning in language is constructed through an activity (Wittgenstein 2009 I, §2). Thereby he pointed to the conventional and context-bound nature of language as well as its embeddedness in human interaction: words are not just names of objects, they can have a variety of meanings depending on the language-game they are used in (Wittgenstein 2009 I, §23-28).

It is in these traditions that discourse analysis is rooted. The method can be seen not so much as a simple application of linguistic analytical tools (such as the analysis of syntax, or lexis or of the use of metaphors in the studied texts) – this is what a linguistic text analysis does (Bergström and Boréus 2005: 19-20); rather the post-structural origin of discourse analysis suggests a particular approach to the social world as if it was a text which the researcher is to deconstruct by identifying the ways in which the meanings are constructed.

Reformulations of marxism

Another defining moment for discourse analysis is its special position towards the materialist theories, in particular towards Marxism. Antonio Gramsci's theory of hegemony was here the main influence (Winther Jørgensen and Phillips 2002:32). According to the materialistic conception of history formulated by Karl Marx, all the developments in the political and legal spheres of a society and, more generally, in all forms of consciousness (what Marx calls superstructure) depend on the material conditions that exist in this society and more precisely on the modes of production (base). In his analysis of Italian history, Gramsci shifted the emphasis from base to superstructure, showing how the manufacturing of consent is crucial for maintaining order in a society. He distinguished the social hegemony from the use of force and stressed its importance in building and legitimizing status quo. He also pointed out the intellectuals as those playing a particular role in establishing and maintaining hegemony (cf. Gramsci 1992: 153). Gramsci's emphasis on the importance of the superstructure for changes occurring on the level of the base had a great impact on the discourse theories and methods, such as in cultural studies. Still, his work has been interpreted and adjusted in different ways by different scholars. Also Althusser's reading of Marx, and in particular his concept of over-determination, has been of importance for the reformulation of Marxism.

Discussion

To sum up, discourse analysis is rooted in two distinct traditions: on the one hand, it constitutes a part of the linguistic turn by drawing on structuralism and on the philosophy of Wittgenstein; on the other hand, it is inspired by reformulations of Marxist theory and

in particular by Gramsci's concept of hegemony. In the next part, I will present how these influences shape theories and methods of the two versions of discourse analysis and how they are used to establish different approaches to the distinction between the discursive and the non-discursive.

The discursive as a theoretical horizon – Laclau and Mouffe's discourse theory

Taking as a starting point a critique of historical materialism, or, more exactly, of the "economist paradigm" within the Marxist tradition (Laclau and Mouffe 1999: 76), Laclau and Mouffe reject the distinction between the discursive and the non-discursive (Laclau and Mouffe 1999: 107)[6]. This is done on the theoretical level and has consequences for both ontology and methodology used in their version of discourse analysis. For Laclau and Mouffe every object is constituted as an object of discourse and "no object is given outside every discursive condition of emergence" (Laclau and Mouffe 1999: 107). Thus all the societal formations must be understood as constructed through discursive processes.

Definition of discourse

To understand this better it might be useful to look closer at their definition of discourse: "… [W]e use it [the term discourse] to emphasize the fact that every social configuration is meaningful" (Laclau and Mouffe 1987: 82). Laclau and Mouffe do not understand discourse in linguistic terms: "what must be clear from the start is that by discourse we do not mean a combination of speech and writing, but rather that speech and writing are themselves but internal components of discursive totalities" (Laclau and Mouffe 1987: 82). Instead, discourse stands for all the practices and objects that in some way involve meaning and since everything social is defined by Laclau and Mouffe as meaningful, all social practices are treated in the realm of discourse. Discourse is here synonymous with practice (Laclau & Bhaskar 2007: 1). In other words, language is only one of the instances of the social and in this sense only one of the possible ways for meaning to be articulated and for discourse to be disclosed.

However, Laclau and Mouffe's definition of the discursive as an all-embracing and constitutive feature of the social does not imply that objects do not exist materially.

6 It should be mentioned here that most of the texts written by Laclau and Mouffe, and in particular their co-authored book *Hegemony and Socialist Strategy* do not explicitly address the question of discourse analysis as a method. *Hegemony and Socialist Strategy* is a book on political philosophy and in it one can hardly find any methodological discussions concerning the analysis of discourse, even less any concrete methodological tools. Still, the book, as well as other texts by the authors, offers analyses of political practices that can be treated as examples of discourse analysis. Moreover, Laclau and Mouffe are the founding figures of a school of ideology and discourse analysis based at the University of Essex.

Laclau and Mouffe do not deny that objects have an existence which is extraneous to any meaning, but they claim that "in our interchange with the world, objects are never given to us as mere existential entities; they are always given to us within discursive articulations" (Laclau and Mouffe 1987: 85). Still, the rejection of the distinction between the discursive and the non-discursive does not mean that discourse has an ideal character; Laclau and Mouffe have time after time emphasized the material character of every discursive structure (Laclau and Mouffe 1999: 108). This means that discourse is always articulated in material institutions, rituals, and practices.

In the approach where the discursive is what constitutes the social in its material articulations there is no need for drawing a distinction between the discursive and the non-discursive, since the only way that one reaches reality is by means of the discursive: human beings are not able to alienate themselves from their human condition and thereby to strip the world from meanings. "If the *being* – as distinct from existence – of any object is constituted within a discourse, it is not possible to differentiate the discursive, in terms of being, from any other area of reality. The discursive is not, therefore, an object among other objects (although, of course, concrete discourses are) but rather a theoretical *horizon*. … If the discursive is coterminous with the being of the objects – the horizon, therefore, of the constitution of the being of every object – the question about the conditions of possibility of the being of discourse is meaningless. It is equivalent to asking a materialist for the conditions of possibility of matter, or a theist for the conditions of possibility of God" (Laclau and Mouffe 1987: 86).

Consequences of the rejection of the distinction between the discursive and the non-discursive for research methodology

Which are the consequences of this rejection of the distinction between the discursive and the non-discursive for methodological questions and how does it influence the shape of research that can be carried out using Laclau and Mouffe's theoretical framework?

Text as just one possible material

The main methodological consequence that Laclau and Mouffe's definition of discourse has for the way a discourse analysis may be carried out is that there is nothing in this approach that says that texts should constitute the primary or exclusive material for analysis. Text – i.e. linguistic practice in the form of speech or writing – can of course serve as empirical material for a discourse analysis, but, since every action, practice, ritual and institution is meaningful, everything social can be subject to discourse analysis. Thus, in the case of Laclau and Mouffe's discourse theory Bergström and Boréus's definition of discourse analysis could be reformulated in the following way: it is meaning, not language, which is at the core of discourse analysis. In practical terms, this means that discourse analysis can be perfectly combined with different qualitative methods, since it is more about the way in which a researcher reads his or her material than about the way in which he or she collects it.

Let me illustrate this with an example: I am interested in the political advance of the Sweden Democrats[7] in the 2010 parliamentary elections.[8] What am I going to study? As mentioned above, texts (e.g. political manifestos, leaflets, election discourses, or articles published in the party's newspaper – *SD-Kuriren*) are just one type of the empirical material that I can use in my analysis. I can also choose to focus on rituals and practices such as political meetings and election rallies trying to read them as if they were a text: How are they organised? In which spaces? Who is taking part? Who is not? What symbols are used? Is there any resistance or opposition (such as counterdemonstrations)? Finally, I can also focus on the organisation itself and its transformation from the racist organisation *Bevara Sverige Svenskt* (Keep Sweden Swedish) to a political party. Here I would look at the political traditions that the organisation in these two incarnations build upon and at the way in which they are manifested in the members' ways of dressing and in the form of their meetings and practices. All of these are material articulations of the discourse; all fix some meanings and construct a certain image of the world. And the aim of a discourse analysis is here to decipher these fixed meanings and to identify the ways in which they are produced. Of course, combining all these kinds of material would give the most complete picture of the discourse produced by the Sweden Democrats.

Aims of the research

Another methodological consequence of the invalidation of the distinction between the discursive and the non-discursive in Laclau and Mouffe's theory is a special way of formulating aims of research. Laclau and Mouffe reject the idea that research can allow us to discover an objective reality or to identify the causes of social processes and changes. Instead they define the aim of a discourse analysis as being to explore how the apparently objective and natural character of reality is constructed. This means that Laclau and Mouffe oppose the materialist approach that seeks reasons for the social practices in what – in the materialistic approach – is considered to be outside the discursive: modes of production or class struggles. In discourse analysis concepts such as 'modes of production' or 'classes' are themselves elements of a discourse and not a reflection of real objects existing out there. The rejection of the very principle of causality contributes to a specific reformulation of the research questions that are possible to ask in a study conducted with the help of discourse theory. Instead of searching for the causal laws or contextualized self-interpretations, discourse theory aims to deconstruct the logics of particular practices or regimes of practices, i.e. "to critically explain their transformation, stabilisation, and maintenance"

7 The Sweden Democrats is a right-wing party with roots in a racist movement. Its main slogan has been to stop immigration to Sweden. In 2010, the party received 5.7 per cent of the votes – enough to enter Parliament.

8 It might be important to mention here that Laclau and Mouffe are political theoreticians and that the main field of their analysis has been that of political practices. Moreover, they have both written on the rise of the right-wing extremism and populism in Europe (cf. e.g. Laclau 2005a, 2005b, Mouffe 2002). Their method seems thus particularly appropriate to the study of political practices, although it can also be applied elsewhere.

(Glynos et al 2009: 9).

In addition, one aim of the analysis would be to identify nodal points – the privileged discursive points of the fixation of meaning. It might be argued that, in a way, the concept of 'nodal points' introduced by Laclau and Mouffe to highlight the struggles that exist in every society over meaning, and thereby over the hegemony, provides some space for the non-discursive. Laclau and Mouffe write: "The impossibility of an ultimate fixity of meaning implies that there have to be partial fixations – otherwise, the very flow of differences would be impossible. Even in order to differ, to subvert meaning, there has to be *a* meaning. … Any discourse is constituted as an attempt to dominate the field of discursivity, to arrest the flow of differences, to construct a centre. We will call the privileged discursive points of this fixation, *nodal points*" (Laclau and Mouffe 1985:112). When talking about a struggle over the fixation of meaning it is difficult not to consider power relations involved in the process, as not every discourse has the same chance to dominate a discursive field and this chance might depend not on the discursive but on the material.

Returning to my example of the research on the Sweden Democrats: Which are the research questions that I can formulate if I want to consistently use Laclau and Mouffe's discourse theory and follow their rejection of the distinction between the discursive and the non-discursive? I am not looking for the reasons of the Sweden Democrats' advance; rather I assume that this advance is a result of a (partial) naturalization and conventionalization of the discourse they used during the campaign so that it appeared as a true representation of the reality to those who supported the party in the elections. The main aim of the analysis is thus here to deconstruct the logics of articulation (Laclau 2005a) that are used in the political practices of the Sweden Democrats. Now, what I am interested in is how the discourse is constructed, i.e. I want to analyse the ways in which the meaning of some particular signs is fixed and conventionalized so that it seems natural to those who believe that the vision of Sweden presented by the Sweden Democrats is the true one. I will have to identify some central nodal points in the discourse of the Sweden Democrats. These might, for example, be 'Swedes', 'immigrants' and 'Muslims'. I would aim to disclose what it means to the Sweden Democrats to be a 'Swede' and to be an 'immigrant'; what meaning is fixed and what is included in these two nodal points? I would also focus on how the unity between different signs in the discourse of the Sweden Democrats is produced and what meanings must be excluded in the process (in dialogue with or in opposition to which other discourses the Sweden Democrats construct their idea of Sweden). An interesting question in the case of the Sweden Democrats would also be in what way they had to modify their discourse in order to transform themselves from a racist movement to a party that would run in parliamentary elections.

Discussion

One piece of criticism that might be made of Laclau and Mouffe's discourse theory is that, although these researchers deny the importance of the questions on the causes of social phenomena, the theoretical horizon they use, i.e. the discourse as the constitutive force of the social, is actually an answer to such a question. In this sense, the all-embracing

character of the discursive may be criticized as a form of a disguised reductionism, in which all the social practices and phenomena are understood through the concept of discourse. Ultimately, this kind of approach does not differ much from the approaches offered by historical materialism where everything can be reduced to economic forces and class struggles. Similarly to Marxism, the basic premise of the discourse theory in the version offered by Laclau and Mouffe is more a result of an intellectual exercise than an empirically backed claim. At the same time, it is exactly this reductionism that makes it possible to construct a coherent and complete methodological approach in which the analysis is able to include all the elements of the social on the same level and consistently using the same analytical tools.

The discursive as one of the moments of the social – Fairclough's critical discourse analysis

Definition of discourse

Fairclough defines the discursive in a way very different from that proposed by Laclau and Mouffe. In his theory, discourse is practically synonymous with semiotics. At the same time, this does not mean that discourse is seen merely on the linguistic level. Fairclough defines the term 'discourse' as including "language (written and spoken and in combination with other semiotics, for example, with music in singing), non-verbal communication (facial expressions, body movements, gestures, etc.) and visual images (for instance, photographs, film). The concept of discourse can be understood as a particular perspective on these various forms of semiotics – it sees them as moments of social practices in their articulation with other non-discursive moments" (Chouliaraki & Fairclough 2007: 38).

Fairclough treats the discursive as only one dimension of every practice, along with other dimensions that must be accounted for in the critical discourse analysis: "a piece of discourse is embedded within socio-cultural practice at a number of levels; in the immediate situation, in the wider institution or organization, and at the societal level" (Fairclough 1995: 97).

As opposed to Laclau and Mouffe, Fairclough keeps the distinction between the discursive and the non-discursive sharp in the ontological and theoretical frameworks of his version of discourse analysis. Since social practices are composed of both discursive and non-discursive moments, the understanding thereof must involve both the analysis of discourse and the analysis of what Fairclough calls the socio-cultural. In this way, Fairclough distances himself from the philosophical assumptions of post-structuralism to get closer to the materialist traditions. The discursive and the non-discursive supplement and condition each other, without it always being clear which of them should be given explanatory priority. Gramsci's concept of hegemony remains central in Fairclough's critical discourse analysis and is used to show how power and ideology influence all

discursive practices on all the distinguished levels. Fairclough understands hegemony as power relations and the larger context that limits the possibilities of discursive practices. These possibilities would otherwise be endless, since discursive practices might in theory build upon an unlimited number of combinations of different discourses and genres, as suggested by the term of intertextuality that Fairclough – inspired by Mikhail Bakhtin – uses to underline the dialogical nature of texts always in relation to other texts, genres, and discourses (Fairclough 1995: 134). All discursive practices emerge as a result of the tension between this endless potential offered by intertextuality and the limiting nature of the hegemonic context. Fairclough uses the concept of hegemony more in order to make space for the non-discursive dimensions of the social that give shape to discursive practices than to undermine the economist paradigm of Marxism as it was in the case of Laclau and Mouffe.

Consequences of the sharp distinction between the discursive and the non-discursive for research methodology

Text analysis as the first step in research

These ontological premises and theoretical approaches have consequences for the methodological toolkit proposed within critical discourse analysis. "The method of discourse analysis includes linguistic *description* of the language text, *interpretation* of the relationship between the (productive and interpretative) discursive processes and the text, and *explanation* of the relationship between the discursive processes and the social processes." (Fairclough 1995: 97). This three-step methodological procedure (description, interpretation, explanation) that corresponds to the three-dimensional ontological model (texts, socio-cultural practices and social structures) leads to specific research questions just like it was in the case of Laclau and Mouffe's discourse theory. However, the difference here is that it also presupposes a particular kind of material. Fairclough is in fact an advocate of the use of texts as empirical material in social research: "Critical discourse analysis claims that close analysis of texts should be a significant part of social scientific analysis of a whole range of social and cultural practices and processes" (Fairclough 1995: 185). Texts are thus a starting point for the analysis and a way of reaching the processes occurring on a societal level. They are considered to constitute manifestations of processes occurring in a society. Critical discourse analysis is thus about introducing text analysis into social research. The practical methodological consequence for research is that there must be a text to be analysed. It can be a written or a spoken text; it can be a document or an interview carried out especially for this occasion but, to be considered a discursive practice, it must involve language.

Now, if I would like to approach my example of the research (the advance of the Sweden Democrats in the 2010 parliamentary elections in Sweden) this time using critical discourse analysis, in what way would my analysis differ from the previous one that used Laclau and Mouffe's discourse analysis? First of all, texts would have to be indispensable.

I would have to start then by choosing some examples of texts in which the Sweden Democrats formulate their political ideas and programme. Since, according to Fairclough, critical discourse analysis is particularly useful in the study of social changes that also occur on the discursive level and are thus reflected in the texts produced within a given discourse (Fairclough 1995: 96), it might be interesting to take two different texts that could be juxtaposed to illustrate a change. One of the texts could be from the period when the Sweden Democrats were still called Keep Sweden Swedish and were more of a racist movement than a political party, while the other text could be from the period when the party was already in the parliamentary elections campaign (e.g. the article published by the party leader Jimmie Åkesson in the daily newspaper *Aftonbladet* in October 2009 entitled "Muslims are Our Largest Exterior Threat").

Aims of the research

Another consequence of the maintenance of a clear distinction between the discursive and the non-discursive levels of the social is that aims of the research done with help of critical discursive analysis are formulated in a way that includes these two levels. The aim of critical discourse analysis is for Fairclough "to systematically explore often opaque relationships of causality and determination between (a) discursive practices, events and texts, and (b) wider social and cultural structures, relations and processes; to investigate how such practices, events and texts arise out of and are ideologically shaped by relations of power and struggles over power…" (Fairclough 1995: 132). Still, Fairclough is not consistent throughout his work when it comes to the direction of these causal relationships. In the above quotation, he states that it is the non-discursive that determines the discursive. In other places, however, the relationship is reversed, like, for instance, when he is urging for a critical discourse analysis that would account for the effects of discourse: "For critical discourse analysis … the question of how discourse cumulatively contributes to the reproduction of macro structures is at the heart of the explanatory endeavour" (Fairclough 1995: 43). In yet other places, he is mainly concerned with the reciprocal or circular nature of this relationship and stresses that discursive practices that are a type of social action presuppose the existence of a structure (both discursive in the form of genres, language codes, etc. and non-discursive in the form of power relations, etc.), while at the same time reproducing them (Fairclough 1995: 35). Here the influence of Gidden's concept of 'duality of structures' is implicit. Despite this ambiguity of the theoretical framework of critical discourse analysis, Fairclough seems, in his concrete examples of the analyses, to give explanatory primacy to the non-discursive macro level looking for the reasons of features of particular discourses or changes thereof in non-discursive social processes and power relations.

Two things are thus clear about critical discourse analysis. First of all, Fairclough's distinction between the discursive and the non-discursive is not only sharp, but also hierarchical: the non-discursive, in the form of "structures, relations and processes" is what determines the shape of the discursive in the form of "discursive practices, events and texts". Secondly, the ultimate aim of critical discourse analysis is about causality, i.e. the

descriptive analysis of the discourse must be complemented by a critical analysis the goals of which are explanatory (Fairclough 1995:43). For Fairclough an explanatory analysis must necessarily involve "higher levels of social institution and social formation" (Fairclough 1995: 43) which means non-discursive levels. In other words, critical discourse analysis must be concerned with the macro level and it is there that it seeks for explanations of the features of particular discourses.

Such a formulation of the aims of critical discourse analysis has consequences for research methodology. It means that, even though the researcher must always have textual material as a starting point of the analysis, it is not enough. Critical discourse analysis – apart from the analysis of the text itself (text analysis of a descriptive character) and of the situation in which the text is produced and received/interpreted (processing analysis of an interpretational character) – must include an analysis of the socio-cultural practice of which the given text forms a part (social analysis of an explanatory character). And, at the end of the day, social analysis always has to involve the non-discursive level of the social world where the explanation of the discursive practices, processes and changes are to be found. The textual material is thus indispensable but not sufficient for the critical discourse analysis. The method presupposes that the final explanatory result be sought outside the analysed material. This is done basically through the reference to other previously formulated theories and explanations of the social processes, e.g. theories of the post-modern condition of contemporary society used in the numerous examples provided by Fairclough.

In my example of the study of the Sweden Democrats this three-step procedure would first entail a text analysis of the previously selected texts. The second step would involve the processing analysis of the situational and institutional contexts in which the texts were produced: the implications of the character of the organisation (racist organisation versus political party) that produces the text for its form; the medium of the text (extreme right-wing political pamphlets, leaflets or speeches at meetings of the organisation versus a debate article in a mainstream daily newspaper); the targeted audience (extremists, members of the organisation versus general public and potential voters); the reception of the texts (enthusiastic reception by the organisation members and ignorance or rejection by the general public in the case of the early text versus the debate around the publication of Jimmie Åkesson's debate article in the mainstream media); the context of the production of the text (activities of the racist organisation versus election campaign of the political party); etc. Finally, the third step would tie up the textual descriptive discourse analysis with an expletory analysis aimed at disclosing, not only the changes that occurred in the discourses used by the Sweden Democrats at these two points in time, but also the changes on the macro level of the society that made it possible for the Sweden Democrats to enter the mainstream politics. Here questions such as the following would be asked: What must have happened in the last decades in Sweden to make such a political advance possible? Who voted for the Sweden Democrats and why? Whose interests does the party represent? As mentioned above, the answers to these questions are not to be found in the previously analysed textual material, but in the knowledge of the developments of the social reality that constitutes these texts based on secondary research and intellectual exercise.

Discussion

Critical discourse analysis differs in two ways from Laclau and Mouffe's discourse theory when it comes to methodological and analytical procedures. Firstly, the linguistics of the linguistic turn is taken literally, which means that critical discourse analysis can be classified as text analysis: it works on texts by subjecting them to a thorough linguistic analysis. In critical discourse analysis language is central. Secondly, by drawing the discursive/non-discursive line in the middle of every social practice involving the use of language, Fairclough proposes an analysis that cannot be limited to the analysis of discourse. Discourse analysis is a starting point, but the explanation is complete only when the non-discursive is accounted for. This might be treated as an attempt to avoid the reductionism of post-structural theories on the one hand and of historical materialism on the other. Still, on the methodological level, this is perhaps the weakest point of critical discourse analysis. Sudden change from linguistic analysis on the level of a particular text to the one of general theories on the structural level makes the final explanation tautological: the text is explained through the processes the existence of which it is to prove. Rather than achieving its aim of offering a complete explanation, critical discourse analysis seems to lead to a kind of incoherence and eclecticism that might be difficult to defend methodologically.

Conclusion

The analysis of the ways in which the distinction between the discursive and the non-discursive in the versions of discourse analysis proposed by Laclau and Mouffe, on the one hand, and by Fairclough, on the other, shows that the issue has important consequences for how research can be carried out, what can constitute suitable research questions, what kind of material can be used, how it should be gathered and what tools can be applied in the analytical process. The two versions of discourse analysis analysed here cannot be treated simply as a method applicable to all kind of research. They entail different ontological and epistemological approaches. Thus choosing discourse analysis as a method of reading the material also means making theoretical and philosophical choices. The aims and scope of the research, the research questions, the material used and the explanation or interpretation offered all depend on which version of discourse analysis is applied. Discourse analysis is thus much more than a method: it both constitutes the theoretical and ontological framework of the research and has impact on how, in practice, the research is conducted.

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Exploring urban development in Malmö City through discourses on change

Ståle Holgersen

Abstract

This chapter uses *Critical Discourse Analysis* (CDA) to conceptualise changes that have occurred in the urban development in the Swedish city of Malmö. The concepts of CDA and its philosophical foundations in Critical Realism are discussed, and linked with the methodology of *dialectics*. The two texts *Västra Hamnen – City of Tomorrow* (the Western Harbour – City of Tomorrow) and *Områdesfakta för Malmö* (Area facts, Malmö) are subject for analysis, and are discussed in relation to each other and also to extra-discursive underlying structures. Through the analysis, the notions of *class* and *change* are given priority. Indeed, CDA allows conceptualising class relations in Malmö grasping simultaneously change and non-change, ´fluidity´ and ´permanence´.

Introduction

During the last two decades the city of Malmö has undergone an extraordinary transformation from an industrial city in deep crisis, to a post-industrial 'knowledge city'. This chapter explores how the method of Critical Discourse Analysis (CDA) can be used for understanding these recent transformations in Malmö.

Why have I chosen CDA? In general, the choice of methodological approaches and methods depends, to a large degree, upon the research questions, the object studied, the aims of the research, as well as the researcher's theoretical background and orientation (Sayer 2000:19, Graham 2005:31, Jørgensen and Phillips 2002:141). The choice of analysing the transformation of Malmö through the CDA framework in this chapter is partly related to the fact that I personally share the underlying ontological and epistemological grounds with CDA. Jørgensen and Phillips (2002:140) also argue that CDA is the version of discourse

theory that is more explicitly interested in studying *change* - a concept that will become central in this chapter.

A discourse will here be understood as a *"particular way of representing certain parts or aspects of the (physical, social, psychological) world"* (Fairclough 2010:358, see also Fairclough 1989:25). Text analysis, which is the main point of departure in this chapter, is only a part of a broader discourse analysis which also assesses the process of *production* and *interpretations* of the text, as well as the social conditions and the context of these processes.

The main methodological problem to be investigated through this chapter can be formulated as follows:

> How can Critical Discourse Analysis help us to understand changes in the urban development of Malmö in general and of Västra Hamnen in particular, in relation to social class?

This chapter uses the city of Malmö as a case study. The main text to be analysed is *Visions for tomorrow in Västra Hamnen* (from now on: *Visions)*. This text is published by the municipality, and I argue below that the text is very much part of a broader discourse which contains strong arguments that the transformation of Malmö into a 'knowledge' city has been a success. This is also the way the municipality wants to present itself. This text will be read dialectically against some quantitative data in *Områdesfakta för Malmö* (from now on: *Områdesfakta).* These are 'area facts for Malmö', i.e. data produced by the municipality on issues like demography, income, education, unemployment etc. *Områdesfakta* does not necessarily show the same optimistic picture as the municipality wants to draw in *Visions*.

For analytical purposes, I have made a distinction in this chapter between *methods* and *methodology*. While the former is understood, in this case, as the modalities and techniques through which data is produced in the research process, the latter deals with the analysis of the principles of methods and organizing methods and empirical findings, and is also, according to Ollman (2003), a way of organizing the 'way of thinking'. I will use the method of *discourse analysis* to investigate texts and the methodology of *dialectics* to 'organize and present' the relations between the various discourses and their relations to the extra-discursive[9].

The chapter proceeds as follows. In part 2, I will discuss some core concepts that form the theoretical basis for analysis. Firstly, the philosophical and ontological foundation of the CDA, which has its roots in Critical Realism, will be discussed, and then the methodological approach to dialectics. This is looked upon in relation to the concepts of *changes* and *class*. Part 3 will focus on the changes that have characterized Malmö, and also analyse the text *Visions for tomorrow in Västra Hamnen* and look deeper into some numbers from *Områdesfakta för Malmö*. In part 4, I look at the dialectics between the discourses and between discursive and extra-discursive dimensions/entities, which I finally put in relation to the notion of change. I end the chapter with some reflections upon the use of methods in the chapter.

9 This is *one* way (however not very conflicting with textbooks and hegemonic uses of the terms) of conceptualizing the distinction.

Discourse Analysis, critical realism and dialectics

There are various types of Discourse Analysis, and Critical Discourse Analysis (CDA) is only one of these. Jørgensen and Phillips distinguish in their book, *Discourse Analysis, as Theory and Method* (2002), between: i) Laclau and Mouffe´s discourse theory, ii) CDA, and iii) Discursive Psychology. Bergström and Boréus argue, in *Textens mening och makt* (2005) (approx. "The meaning and power of text"), that Discourse Analysis is often divided into three approaches: a) the continental or French (often associated with Foucault), b) the Anglo-Saxon (where Laclau and Mouffe are examples) and c) CDA[10].

The categorizations above bear witness to the difficulties in identifying and agreeing upon any clear definition of discourse. While some define discourse as almost synonymous with semiotics, some argue it is one kind of practice, while others see discourse as *equal* to practices (ibid, see also Laclau and Bhaskar 1998, Fairclough 1989). Below I will outline the concept of CDA, also in relation to ontological stands and other views of discourse where that is needed.

Critical realism and CDA

The various types of discourse analysis mentioned above, have different philosophical and ontological positions. CDA is often associated with a position labelled *Critical Realism*. One of the main claims from the critical realists is that the world exists independently of our thoughts about it. Although we can only come to know, and give meaning to, extra-discursive processes through discourses, they nonetheless do influence us directly, for example as in catching a virus (Sayer 2000:37).

The distinction between discursive and extra-discursive spheres is a central aspect of CDA. Extra-discursive fields do not only include 'natural', physical phenomena like stones and other physical objects, but also structures that are constantly (re)produced by humans. One such structure, also elaborated upon by Fairclough (1989), is that of *class relations*; even if we were not aware of the existence of class relations or if we did not have a language to articulate the phenomenon, class relations would still exist and have effects.

Discursive practices always function in dialectical interplay with other dimensions of social practices. Within CDA this is not understood as a one-way relationship:

> As well as being determined by social structures, discourse has effects upon social structures and contributes to the achievement of social continuity or social change. It is because the relationship between discourse and social structures is dialectical in this way that discourse assumes such importance in terms of power relationships and power struggle… (Fairclough 1989:37).

10 On Laclau and Mouffe's discourse theory, which Bergström and Boréus relate to as "Anglo-Saxon", see especially Ernesto Laclau and Chantal Mouffe's *Hegemony and Socialist Strategy - Towards a Radical Democratic Politics* (2001). For an example of Discursive Psychology see Derek Edward's *Discourse and Cognition* (1996); of the continental or French, see *The Archaeology of Knowledge & the Discourse on Language*. Rount , by Michel Foucault (1972).

In other words, discourses are both socially-structuring and socially-structured (cf. Fairclough et al. 2002, see also Jørgensen and Phillips 2002:139-140). Dannestam (2009:ch.2), who also writes from a Critical Realist perspective, follows this line of argument when claiming that the discourse, although not solely determining, does have an impact on the social practice; discourses can have extra-discursive effects.

It is a complex matter to make the actual demarcations between discursive fields, and to draw the line where one discourse ends and another starts. Neither is drawing the lines between discursive and extra-discursive fields necessarily a straightforward task. But as is the case with demarcations in general in social science, in order to be able to analyse the world we have to create analytical boundaries and analytical categories. However, it must be kept in mind that these categories are never purely isolated to one single sphere. For example, the economy is never purely about 'economics', as it always interacts in multiple ways with other spheres and categories.

In this chapter, class will primarily be seen as economic differences in income. This is of course a problematic reduction, as the concept is much more complex. But for analytical reasons and the length of the text, I do consider this to be a favorable demarcation.

Dialectics

While CDA in this chapter is used primarily as a particular method, *dialectics* are used as a methodological frame. This is an analytical distinction which is made to clarify arguments and avoid confusion. Other authors may rightly use different definitions. Just as CDA could be framed more in the direction of theory (of science) or methodology, dialectics could be described as a 'method'. What *are* needed however are clarifications and definitions that do not confuse terms and categories, and through the clear separation between method and methodology, I aim at avoiding such confusion.

Delimiting CDA to methods opens up for introducing a particular take on methodology. Methodology is in this chapter also related to how we think about, organize and understand a reality, and the way we as scholars present thinking and understanding (Ollman 1971, 2003, see also Castree 1996). So methodology is not only related to the actual collection and production of data (i.e. the "method"), it is also how we organize, think about and present it.

Ollman (1971, 2003) and later Harvey (1996, 2010) propose *dialectics* as one way of organizing and presenting data. Their understanding of dialectics derives from Marx. However, although Marx did argue that his dialectic was 'exactly opposite' to Hegel's version (Marx 1995), he "never wrote a tract on dialectics, and he never explicated his dialectical method" (Harvey 2010:12). Ollman's and Harvey's interpretation is therefore based upon how they read Marx and upon how he actually conducted his research dialectically. Their understanding attempts to move away from the 'all-purpose (Hegelian) explanation' of 'thesis-antithesis-synthesis', and rather understand dialectics as a way of thinking and emphasizing the notion of "process" (time) and "relation" (space) over the notion of "thing". Furthermore, it is argued that such an 'open' way of thinking about movement and contradictions forces the argument to spin 'onwards and outwards to all

manner of new terrain' and that it precludes any closure of arguments at any particular points (Harvey 1996, Ollman 2003).

One of the strengths with this methodology is that it makes possible the double aim of a) discovering how something works or happens, and b) developing an understanding of the system and structures in which such things could work or happen (Ollman 2003).

Ollman (2003) argues that as structuralism turned into 'post-structuralism' the general tendency became to overestimate the speed of change, which is related to a tendency to underestimate everything that is holding the change back. Jørgensen and Phillips also argue similarly, when claiming that some poststructuralists see inter-discursivity as a "manifestation of the extreme instability and changeability of language" (2002:74).

Harvey insists that although the contemporary mode of thinking is characterized by 'flows', which are open processes, these "flows often crystallize into 'things,' 'elements,' and isolable 'domains' or 'systems' which assume a relative permanence (and sometimes even acquire limited casual powers) within the social process" (Harvey 1996:81). When considering and paying careful attention to these *permanences*, the question becomes how to understand the *pace* of change, and the correct relation between change and permanences?

As argued above, discourses are both products and producers of structures, and this also becomes crucial in relation to social change. As argued by Fairclough, depending upon power relations, these structures might be *reproduced* with 'virtually no change' or in 'modified forms': "Reproduction may be basically conservative, sustaining continuity, or basically transformatory, effecting changes" (1989:39).

The relations between discourses, i.e. inter-discursivity in Fairclough's vocabulary, thereby become the focal point for understanding *change* and *permanences*. Moreover, relations to extra-discursive structures must be grasped if we are to understand the stability in a seemingly unstable world. The aim in the analysis below is therefore to use dialectics to organize the relations between discourses and between discourse and extra-discourse; and also to use dialectics to understand relations between changes and permanences.

Visions for tomorrow in Västra Hamnen

During the last decades the city of Malmö has undergone various spatial and socio-economic transformations. Emblematic of these changes is the area of Västra Hamnen, the site of the former shipyard which is today being transformed into housing, offices and spaces for education, recreation and retail. This transformation began in the mid-90s and in 2001 the international housing exhibition *Bo01* opened. During the last ten years not only Västra Hamnen but also the whole city of Malmö has experienced a seemingly qualitative leap from an industrial into a knowledge based city. This transformation has by the dominant discourses been conceptualised as a great success, in terms of that it managed to establish a university, attract media-businesses, (creative) office-workers and more well-off residents, etc. (see e.g. Dannestam 2009). In fact, this qualitative transition has made the city more attractive to well-off social cohorts. The Western Harbour was actually built explicitly for 'the rich' in order to attract tax-money, and also to stimulate the economic

development by attracting entrepreneurs (see Dannestam 2009:145). Malmö has at the same time been a site for an increase in refugees from poorer countries and/or countries in war. These opposed tendencies have contributed to the deepening inequalities that are also characteristic of Malmö today.

The old Malmö and its transformation

The shipyard at Västra Hamnen was once one of the world's largest: it delivered the highest tonnage in the world in 1952 and 1953 and it housed the world's largest crane at the time (the *Kockum's crane*) in 1973-74. After this phase of flourishing, the shipyard went into a decline and then a crisis during the 70's and early 80's, and in 1986 all the production of civil shipbuilding had come to an end. New hope came to the city when Saab-Scania, backed by large state-subsidies, announced their plans for a car factory at Västra Hamnen. The car factory was in fact opened in 1989, but closed down no more than two years later.

The transformation in the 90s must be conceptualized within this spatial-historical context, with increasing unemployment and low income for the municipality. In the early 90s the newly elected mayor of Malmö, Illmar Reepalu, started the "vision work" for the development of a new Malmö. The changes were articulated in the *Oversiktsplan för Malmö* 2000 [The Malmö Plan 2000] (Malmö 2001), where the "new" Malmö was promoted, which can be exemplified by quotes like:

> "Growth should mainly occur within the business sector, through the expansion of existing companies and the establishment of new firms in the region. This expansion assumes that the population's educational level increases quite rapidly, which in addition to efforts in the area of education, also requires a relatively large immigration of highly educated people" (Malmö 2001:19, my translation).

The new Malmö was based on a mixture of attracting capital, the 'creative class', people economically well off, establishing a new University, building offices, a bridge to Denmark and promoting environmental friendly urban planning/architecture, culture and organizing 'events' (see e.g. Dannestam 2009 and Mukhtar-Landgren 2009).

Västra Hamnen was also the landmark of the industrial Malmö, and the *Kockum's crane* was the biggest symbol for industrial work and life. From the beginning of the first decade in the 21st century, the site has been transformed into (environmental friendly) housing, offices, education and leisure areas, and has arguably become the face of the new Malmö.

The *Kockum's crane* was sold to a company in South Korea for USD 1:00, and was "replaced" by a new landmark at the site; Turning Torso – Sweden's tallest building famous for its architecture. Figures from 2010 say that there are 2558 apartments, 9168 people working and about 12000 students at Västra Hamnen (Malmö 2010), and still there are large areas to be developed on the site.

Dannestam (2009) claims that Malmö was re-conceptualized into a post-industrial city via three different, but interrelated and overlapping, micro-discourses. First there was the discourse about a new and transformed Malmö – where the central aspect is to create something different to the industrial city, whether it is "knowledge", "experience",

"event" or the "creative city". The second micro-discourse is about the municipality as a "promoter of welfare and growth" – where the central argument is that the new role of the municipality as promoter of growth is compatible with the traditional role of the municipality as an institution for service and welfare. The idea behind this discourse is that growth will automatically generate welfare. The third discourse is the city as an engine for growth – where the main argument is that actors must position the *city,* rather than the municipality, within the new geography which is defined by urban and regional competition. (Dannestam 2009:120-121)

Visions for tomorrow in Västra Hamnen

In the following I will first look at a text written by the municipality of Malmö that is published on their webpage. After some discussion around the text, I will look at the dialectics between this text and other possible and existing discourses on the developments in Västra Hamnen and Malmö. At the end I will look at the dialectics between discourses and extra-discourses with the aim of understanding changes at Västra Hamnen.

Visions for tomorrow in Västra Hamnen.

The closure of the Kockums shipyard presented real scope for the transformation and creation of a new district in the city - Västra Hamnen, the City of Tomorrow. Thanks to this structural facelift, the city has now once more renewed its ties with the sea.

One important feature of this district is the university at Universitetsholmen, which in a relatively short space of time has undergone some major changes. New university buildings have been constructed at record speed and both this area and other parts of Västra Hamnen have altered in character.

In the former shipyard area, we now see students, business people and residents bustling about their lives and work. These used to be the Kockum workers. The area covers about 140 hectares and currently has a number of projects which are at the planning and construction stages.

The location of Västra Hamnen is excellent and the creation of the city tunnel will enhance the district's overall attraction as Malmö's largest and most important centre of development. A stone's throw from the older city centre, business and outdoor life.

Source: http://www.malmo.se/English/Western-Harbour.html

The text is written at the time of the actual development of the site, with some areas that have been established and built while other projects are 'currently at the planning and construction stages'.

Already in the headline, the text points distinctly forwards, towards 'tomorrow'. Although there are some references to the past, these are used as oppositions to the future;

the 'real scope for the transformations' was enabled because of the 'closure of the Kockum's shipyard' and the 'business people' used to be 'Kockum workers'. Västra Hamnen is also defined in the title as 'the City of Tomorrow'.

The notion of 'change' is very much present in the text, with 'transformation and creation of a new district…' with 'some major changes' at 'record speed'. It is clear that the municipality presents the processes at Västra Hamnen with an emphasis, not only on the 'changes' but also on the 'record speed' of changes. It is changing, and it is changing fast.

It is interesting to see who the people are in the City of Tomorrow. What used to be Kockum workers are now 'students, business people and residents'. The students and residents are new in the area, while business people have replaced Kockum workers in terms of labour. New classes have replaced the old monolithic, and perhaps romanticised, category of ´Kockum workers´. The *class-character* is present throughout the text.

Municipalities are normally considered legitimate authorities in Sweden, and the positive tone in the text would not have been read with the same authority had a Real Estate Broker written it. *Visions* is written by the municipality, that is also the planning authority. Not only are critical views on the development missing, but the text is also loaded with positive descriptions of the process; 'structural facelift', 'renewed its ties', people 'bustling about their lives and work', 'location is excellent and … city tunnel will enhance the overall attraction' in the area which is Malmö's 'largest and most important centre of development'.

The tone in this document is very much in accordance with a general hegemonic discourse on the development of Västra Hamnen. The Västra Hamnen development was very much criticized in the first few years, and it is even argued that there are very few housing exhibitions and new city districts that have become so infamous as the one in Västra Hamnen (Jansson 2006:87, see also Dannestam 2009). However, the attitude changed and Malmö was suddenly described in a similar way as *Visions for tomorrow in Västra Hamnen*. In 2005 the Turning Torso opened and by then people from all over Malmö had begun to use the Västra Hamnen for leisure and recreation. The Öresund Bridge to Copenhagen had been open for some years, and the university at Västra Hamnen had settled well into the city. The criticism that was attached to the site from 2000 to 2005 began to fade, and the story became one of success.

Malmö also started winning prices for its developments. Of several prizes and awards it is worth mentioning that in 2007 Malmö was number 4 in *Grist Magazine*'s list of the world's 15 Green Cities list, and it also won the "Grand Prize at the First Community Awards for Renewable Energy Sources"[11]. At the Expo 2010 Shanghai, Malmö was included in the exclusive "Urban Best Practices Area", where they used Västra Hamnen to show "Urban Sustainable Development in a Former Industrial City". And in 2011 the city was "Earth hour capital".

11 http://www.grist.org/article/cities3 (access 19.09.2011).

On top of it all, the Turning Torso has received 6 or 7 prices on its own[12], hoards of Swedish and foreign planners and architects come every year to study the area[13].

Statistics: "Områdesfakta för Malmö"

Every year the municipality of Malmö publishes statistics in Malmö, called *Områdesfakta för Malmö*. The data are for obvious reasons some years older than the publications, so that in the case below, numbers from 2002 are published 2004, and numbers from 2004 in 2006 and so on…

Figures from *Områdesfakta,* also produced and published by the municipality of Malmö, show large differences between the people populating the "new Malmö" in Västra Hamnen and the rest of the city. The inhabitants in Västra Hamnen are in general more educated: 62% have a tertiary education compared to 40% in the whole city. They have on average higher incomes: in 2006 the average in Västra Hamnen was SEK 345, 093 compared to the city's average of SEK 226,140 (Malmö 2008).

Numbers also show that fewer people in Västra Hamnen are unemployed than in the city in general, less people are recipients of social security, and the number of cars per 100 residents is higher (Malmö 2008).

The socio-economic differences have also increased during the last decade. In Figure 1, I have put together numbers from *Områdesfakta* in three different years in the first decade of the 21st century, and compared the average income between the whole city of Malmö and Västra Hamnen in 2002, 2004 and 2006.

Figure 1

Year / Average income (in SEK)	Malmö	Västra Hamnen
2002	198 300	258 700
2004	203 600	271 600
2006	226 140	345 093

(Numbers from *Områdesfakta för Malmö 2004, 2006 and 2008)*

Figure 1 shows how the average income in Malmö as a whole has increased by 14 % from 2002 to 2006 (from SEK 198,300 to SEK 226,140), while the average income in Västra Hamnen has increased by 33% during the same years (from SEK 258,700 to 345,093).

Other figures and research results confirm the argument about the class character of Västra Hamnen. Figures from 2008 say that housing costs in Västra Hamnen without a view of the sea were about SEK 30,000/m², and SEK 45,000 – 50000 /m² for an apartment

12 E-mail correspondence with Göran Rosberg, at the municipality of Malmö (14.10.2010).

13 Göran Rosberg at the municipality estimate that around 10-12,000 professionals within city-planning and sustainable city development come to visit Malmö every year (e-mail correspondence 15.10.2010).

with a view of the sea[14]. The average price in Sweden is SEK 18,233 /m², while the average price in Central Malmö is SEK 19,655 /m²[15].

Johnson et al. (2010) have investigated the economic structural changes in the housing sector in Malmö from 1986-2001, and looked at gentrification, filtering and social polarization, and argue that there has been a marked tendency for social polarization. They have also mapped the polarization to an east-west divide, where "super-gentrifications" have occurred in Bellevue and Nya Bellevue, i.e. Malmö west. Johnson et al. write that since 2001 there has been a "…continued increase in inequality and that the social geographic polarization mapped between 1986 and 2001 has probably intensified during this decade" (ibid: 2).

Dialectics of discourses

There are various discourses at stake here, some even overlapping, other conflicting with or adapting to each other. At one level, it is possible to talk about the two documents, *Visions* and *Områdesfakta*, as placed within two distinctive discourses; where the former is related to the 'official' story articulated by the municipality that the transformation into a knowledge city has been a success, while the other reveals the social polarization in the city and relates to a discourse on the *dual city* (Mukhtar-Landgren 2005). Which discourses that turn hegemonic and how these processes work is another interesting story that will have to be elaborated in texts other than this one.

Conflicting discourses

There are conflicts at several points between these two discourses. The emphasis in *Visions* is on the positive sides and is the agenda of the municipalities; to conduct a successful (discursive and material) transformation into the post-industrial society. Concepts like poverty, segregation and inequality would be discordant to such a goal and are indeed seldom mentioned nor hinted at in the municipality narrative. Although they are not explicitly spelled out in the *Områdesfakta* either, these concepts are clearly present here. When the city is establishing a new city district for the economically well off, and the class divisions between the city districts are increasing, one immediate question becomes – and which city districts are losing out?

The facts from *Områdesfakta* (that indicate that people in Västra Hamnen are well off) are dialectically related to the positive tone in *Visions*. The actual distribution of wealth in Malmö is paradoxically reflected in both these discourses. The reality contributes in

14 http://www.malmo.se/Medborgare/Stadsplanering--trafik/Stadsplanering--visioner/Utbyggnadsomraden/Vastra-Hamnen/Samlade-Skrifter/Vastra-Hamnen-i-siffror-2008/pagefiles/Vastrahamnenisiffror.pdf (at17.03.2010)

15 Article: Kraftigt stigande priser på bostadsrätter i augusti, on http://www.dn.se/ekonomi/kraftigt-stigande-priser-pa-bostadsratter-i-augusti-1.949019(at 17.03.2010)

this sense to constitute the discourse on Västra Hamnen and Malmö. It would perhaps be politically impossible, or perhaps seen as a bad joke, if the municipality wrote a similar text about an area which was primarily inhabited by the poor.

On the other hand, the municipality, i.e. a legitimate authority in Sweden and normally considered a reliable source is, through *Visions,* contributing to the production of a narrative about the changing 'new district'. When the municipality argues that the site is just a "*stone's throw from the older city centre, business and outdoor life",* this also affects people's choices on where to live. And within 'free housing markets' the rich can 'choose first' where to live (Harvey 1973), and when narratives and general discourses on Västra Hamnen also contribute in constituting who actually moves to the place; we can say that the discourse also dialectically affects and changes reality.

From the data in *Områdesfakta* we can see that Malmö is a class-divided city, i.e. a city with spatial differences concerning economy[16]. Class differences can be operationalized as real structures, existing in the real world, as extra-discursive, but yet constantly dialectically interconnecting with various discourses (see e.g. Fairclough 1992). There are also discursive representations of class in both texts and discourses; in *Områdesfakta* (as numbers) and in *Visions* (as descriptions, e.g. where 'office workers replace industrial workers').

Through texts like *Visions* and *Områdesfakta* we can start making claims that there exists a structure like class in Malmö which is real and could be classified as extra-discursive. And, by doing this, we can also constitute a discourse on class relations in Malmö. This claim should and could of course be strengthened by using more (and various kinds of) data and produced by various methods in future research.

Another way of looking at discursive change in Malmö is to claim that there existed *one* general discourse on the city, and that this has changed during the first decade of the 21st century in the transformation from an industrial to post-industrial city. An in-depth investigation into *why* this discursive shift happened is not within the scope of this chapter. However, what should be said is that explanations can be found both in other discourses, and in extra-discursive structures. It is arguable that both the texts produced by the municipality have influenced the broader discourse, and the actual increasing wealth in some parts of the city, has contributed to a new understanding of Malmö.

Changes and not-changes

Grasping the *pace* of change is not an easy task. As argued above, the text *Visions* very much emphasizes the changes in the city. A lot is new, like the spatial use of the land and the demography of the people using Västra Hamnen and so forth. Urban changes with impressive architecture are perhaps more than anything the materialisation of change. However, bearing in mind a completely new urban form, a municipality that campaigns for the "new city" and the figures in *Områdesfakta* that indicate large variations related to class in Malmö, is there not also a danger of *overemphasising* the notion of change, and a corresponding danger in underestimating everything that is holding the change back?

16 See also Billing and Stigendal (1994) for a historical analysis of Malmö during the last century.

If we ask a question like, what is *not* new in Västra Hamnen and Malmö, one answer is the class differences as such. Although the class differences are changing, as we can see in both the *Visions* and *Områdesfakta,* the actual existing extra-discursive structure of class-relations, and thereby the fact that Malmö is a class-divided city has not changed.

Although all social reproduction contains elements of change, the fact that Malmö is geographically divided in terms of economic resources, is basically nothing new. And in this respect the development at Västra Hamnen has *reproduced* with 'virtually no change' the class-divided city of Malmö. The actual composition of the class-divided city, however, has been a transformatory reproduction including effective changes.

Just as the general discourse on Malmö has changed, so has the particular composition of classes. The fact that Malmö is a class-divided city, however, seems rather to be a *permanence.*

Conclusions

Tomasi di Lampedusa is famous for the quote, "If we want things to stay as they are, things will have to change." (di Lampedusa [1958] 2007). I found the combination of CDA and dialectics useful in this chapter to attempt to conceptualize changes and permanences in class relations in Malmö. This was done through looking dialectically at various documents, and thereby making arguments that again correspond to unequal discourses on Malmö.

The operationalization of CDA as a method and dialectics as a methodology has also been fruitful. Although this is not a common approach, I would argue that the methodology of dialectics brings some kind of openness to CDA that is interesting and partly also needed. It forces us to think relationally and in movement, while still forcing us to grasp the permanences. The way of grasping class relations in Malmö as simultaneously change and *permanence*, derives from this way of thinking.

Nonetheless, there are some concerns that would need further elaboration and which demand critical focus; the distinction between the discursive and the extra-discursive in particular opens up for problematic situations, especially when including social structures like class within the sphere of the extra-discursive. As long as one claims that there are both discursive and extra-discursive effects, there are few alternatives other than continuing to research, and criticize, CDA.

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Pondering on methods

In this book, ten doctoral students share and discuss a number of methodological concerns they have faced in the process of doing research. In the first part, "Capturing the field," the contributors grapple over issues regarding how to define and demarcate the field of research, for instance in studies on immigrant organizations, red light districts on the Internet, and telephone interviewing. In the second part, "Relations in the field," the authors deal with methodological concerns regarding the relationship between the researcher and the researched, adressing for example research on disabled people, children, and people with HIV and AIDS. The third and final part, "Theory as methodology" contains contributions that emphasize methodological implications resulting from theoretical choices, such as the concepts of culture and discourse.

This book is the outcome of the course Qualitative Methods offered as an interdisciplinary course within the PhD programme in the social sciences. The contributors come from various disciplines, including Health and Society, Human Geography, Communication and Media, Political Science, Social Work, and Sociology.

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