palgrave pivot

POWER, CULTURE AND SITUATED RESEARCH METHODOLOGY

Autobiography, Field, Text

Cecilie Basberg Neumann
Iver B. Neumann

Power, Culture and Situated Research Methodology

Cecilie Basberg Neumann · Iver B. Neumann

Power, Culture and Situated Research Methodology

Autobiography, Field, Text

palgrave
macmillan

Cecilie Basberg Neumann
Oslo and Akershus University College
Oslo, Norway

Iver B. Neumann
Norwegian Institute of International Affairs
Oslo, Norway

ISBN 978-3-319-59216-9 ISBN 978-3-319-59217-6 (eBook)
DOI 10.1007/978-3-319-59217-6

Library of Congress Control Number: 2017944180

Cover illustration: Pattern adapted from an Indian cotton print produced in the 19th century

Printed on acid-free paper

This Palgrave Macmillan imprint is published by Springer Nature
The registered company is Springer International Publishing AG
The registered company address is: Gewerbestrasse 11, 6330 Cham, Switzerland

Preface

Not long after we met, the two of us (and a baby to be) were in London doing research. Cecilie researched public health visitors, while Iver researched diplomats. Every morning we would stack up on a full English breakfast before we hit the field. Iver spent his days in conferences and embassies interviewing people in suits about ministerial work. Cecilie spent her days in tiny offices interviewing public health visitors on everything from the spread of tuberculosis to violence against women and children. The world in which Iver spent his London days was a version of our own everyday world, writ large and dressed up. The world in which Cecilie found herself seemed to us as if it had been lifted straight out of a Charles Dickens novel. Suppers were spent discussing what we had seen, experienced and learned, and we were struck by the markedly different social realities we had participated in, although physically we had been only 5 miles or so apart. Our conversations revolved largely around the meaning of situatedness and of being situated. Which preunderstandings, emotions and prejudices are activated in the field towards the researcher, and how do these dynamics affect what the researcher sees and does not see? And if reflected upon at all, which of these reflections are reported or integrated into the final research product?

Thus were born the questions that we answer in this book: what is situatedness, how does the researcher come to be situated, how can we talk about different types of situatedness and make it relevant for our research? Here lurks a whole range of methodological problems that are

normally swept under the carpet. How can we specify and make awareness, emotions and body relevant in our data collection, in our analyses, and in the texts we write, without letting the reported research slide into a story about the researcher herself? What is at stake here, and where can we find inspiration to do this reflection-work? In this book, we use three different approaches to answering these questions. First, we specify what situatedness is. Different parts of the research process calls for different types of situatedness. Second, we provide several examples from our own research. Third, we have tried to write this text in a way that not only says what situatedness is, but also shows it.

Cecilie is not only alphabetical but also actual first author. Most of the empirical examples throughout this book are taken from her research. Thanks to Thomas Hylland Eriksen, Jane Dullum, Cathrine Egeland, Arne Johan Vetlesen and Gro Ween for useful comments and corrections and to Susan Høivik and Simone Tholens for textual assistance. Finally, a word of caution. Chapter 7, parts of which have appeared earlier in our 2015 article "Uses of the Self: Two Ways of Thinking about Scholarly Situatedness and Method" (*Millennium* 43, 3: 798–819) and are reused here with permission from SAGE Publishing, situates the book in relation to philosophy of science debates and contemporary scholarship on situatedness. Although we followed the same adage as in the other chapters, that things should be written as plainly as possible, not plainer, this chapter is still rather technical, which means that some readers who might enjoy the rest of the book, should probably just forget about that particular chapter.

Oslo, Norway, April 2017

Cecilie Basberg Neumann
Iver B. Neumann

Contents

CHAPTER 1

Introduction

Abstract We start our venture by introducing the tree phases of the research process and the different ways in which they invoke our situatedness in the pre-field-, in-field- and post-field phase. Inspired by feminist objections to value-free research, we ground our understanding of situatedness in Sandra Harding's concept "strong objectivity". This concept opts for transparency through the researcher's context awareness, as well as the researchers situated self-awareness throughout the three phases of the research process. To further our aim of doing situated research, we argue that the Gestalt traditions concepts, and phenomenological understanding of human beings as fundamentally relational, may provide us with valuable tools in order to do situated research in practice, complementary to techniques accounted for in manuals on qualitative research methods.

Keywords Situatedness · Strong objectivity · Knowledge · Gestalt tradition · Professional research ideals

There is no such thing as first contact, for we always come to situations with expectations about it already installed. A student who enters a field to interview people about a specific subject, or spends time immersed in fieldwork, will have required a certain amount of basic knowledge of the field in which she is about to interact. When she has chosen her specific subject of research, she will have to be certain that at least a part

C.B. Neumann and I.B. Neumann, *Power, Culture and Situated Research Methodology*, DOI 10.1007/978-3-319-59217-6_1

of her research questions can be answered through qualitative interviews or ethnographic methods. She will have to plan interview questions and conversations prior to her actual interactions with her informants, as she must decide in advance on at least some of the topics she is going to explore in the field. Before she enters the field, she must be as aware of what she is looking for as possible, and she must have clarified her prejudices and prior understandings of the field. If her chosen method is qualitative interviews, the interview questions or interview guide will have been planned and written down in advance and ought to have been rehearsed in order for her to avoid fumbling with papers, pencils, microphones and tape recorders during the interview. The student may have learnt that questions can be formulated in many different ways, and that questions which invite a yes or no answer should be avoided. She has probably also learnt that interview questions evoke different levels of meaning in the informant and his relationship with colleagues, friends, family, organization, institution and structures, different levels of meaning that are important to distinguish or keep analytically apart when she analyses her material. She has learnt that it is wise to be a keen listener who is seen to respond with encouraging signals such as nodding, attentive body language, saying "mmm" and "yes". This may help the informant to talk, memorize and reflect on the specific questions more freely. But she may also have learnt that if she gives too many encouraging signals she risks that the informant will take control of the interview situation. If this happens, she may experience problems not only with asking critical and challenging questions, but also with uncomfortable questions, and she may have to end the interview without having exhausted her prepared list of questions. In addition, she has (fingers crossed) learned that she is the responsible party in the interview, and that she, qua her role as researcher, should stay in the background as much as possible. She is there not to talk about herself, but to help bring out the informant's knowledge and experience on specific questions and topics.

This book is not an attempt to challenge these and numerous other pieces of good advices on data collection techniques, research strategies and suggestions to preparations and approaches prior to and during work in the field. This kind of knowledge is a fundamental part of basic research knowledge. Still, we strongly feel that there is so much more that needs to be said and done. In addition to mastering specific research techniques, there is also another logic present. This is what we may call

the dialogical logic, which is fundamentally relational and deeply human (Holstein and Gubrium 1995; Staunes and Søndergaard 2005). It is impossible to have a conversation with someone, to conduct an interview or being present in a field, without being personally involved in one way or another. This book is an invitation to reflect on how these dialogical and relational aspects affect or interact with the three main levels of research. There are, first, field preparations and choice of topic, theoretical perspectives and data gathering techniques. We call this the pre-field stage. The pre-field stage bleeds into the way we create empirical data, and our understandings and analyses hereof. We call this the in-field stage. Finally, the two previous stages connect with how we write texts based on these data, principally but not exclusively through note-taking. We call this the post-field stage. In practice, a researcher always keeps an eye on the phases to come, and often continues work on previous phases as she has moved on, for example, by adjusting There is recursivity between the three phases, in the sense that one will adjust a theoretical perspective or collect complementary data during the writing phase. Still, it makes obvious sense to treat the three phases as analytically separate.

Reported research findings will be the result of the fact that the researcher has moved through these three stages, and in order for the research to be as good and as accountable as possible, it is imperative that the researcher should pay attention to the way she conducts her research. By discussing and problematizing the epistemological assumptions implicitly made in the large swathes of research literature which take for granted that the researcher should be "neutral" and "withdrawn" (Haraway 1991; Brigg and Bleiker 2010), and by addressing explicit situations that the researcher may encounter or be confronted with during the research process, we hope that this book will succeed in demonstrating that all research is situated, and also in suggesting how this situatedness may be dealt with in the three stages of the research process.

Our point of departure for reflecting on how research is situated and how the researcher may situate herself is the so-called Gestalt tradition. Specifically, we relate to the founders of Gestalt psychology and their observations and thinking about humans as fundamentally relational. We are also inspired by some of the Gestalt therapy's relational terms and techniques regarding human interaction within the therapeutically conditioned conversation, for here we have an arena that is similar to the research interview and that has been analyzed in much more depth,

which should mean that there lurk lessons to be learnt for the social scientist. Our intention is not, however, to create a new methods book for the social sciences *grounded* in the concepts of Gestalt psychology and the techniques of Gestalt therapy. The intention is rather to show how the Gestalt psychological thinking and concepts may provide inspiring and relevant tools for researchers in the social sciences. We relate Gestalt therapy to interview and field situations, to the process of reflection on collected empirical data, and to the process of doing/writing social scientific analyses. The three stages of the research process each presents the researcher with the challenge of situatedness. The pre-field phase calls for autobiographical work: how did I end up researching this topic, asking these questions, applying these theories? The in-field phase raises the question of how the pre-field experience impinges on what is observed, how it is observed, with what results for data creation. The post-field writing phase demands that we, as the creator of the text, situate ourselves, be that as hidden or present writer subjects, and if the latter, how present.

Gestalt psychology emerged as part of the philosophical movement called phenomenology. Phenomenology is an independent research tradition within the social sciences, perhaps primarily in anthropology but also within qualitative sociology. For people working within this tradition, this book will have the added attraction of making the acquaintance with a prodigal daughter called Gestalt therapy, which departed from academic Gestalt psychology prior to the Second World War and is only now being brought back in this book.

Our aim is to apply some of these Gestalt insights in a constructivist understanding of knowledge production.[1] We try to get at how to make our own data production as transparent as possible, and how to connect our understanding of situatedness to ways of conducting ethically sound research.

The researcher's professional ethos

Our claim is that the researcher, regardless of how well prepared she is for an interview or for field work, cannot avoid forming relations with her informants. The very idea that she should be able to stand on the outside of the relation, or not engage in any relation at all, and simply be a neutral and objective observer that would not influence her informants and the field of research, is, we would argue, founded on false assumptions (see Harding 1991; Smith 2005). Furthermore, since human interaction is by definition open ended, the researcher cannot know what will

happen during an interview or in the field beforehand, no matter how well prepared she is. The need for improvisation is therefore always there during data creation. If everything was simply there for the picking, social research would be like picking mushrooms or collecting firewood. It is not. Data is created, not collected.

This simple insight has wide-reaching implications. The informant will be affected by the presence of the researcher, influenced by her way of being, impressed by her way of posing questions. In some situations, the researcher may be forced to alter or modify her interview and field preparations. She may have to begin the interview differently than what was planned. She will be challenged intellectually and emotionally in ways she may not have anticipated. Informants will frequently, and often out of the blue, ask for her personal point of view on a specific question, ask her to comment on a difficult situation in the field or criticize one of the topics of the interview or angles of a question.

In situations like these, many researchers and students will experience that they depart from or even break with some poorly defined ideals for the professional researcher implying the normative "silent", "neutral", "you should remain in the background" ideal attached to the researcher's ethos, that probably stems from the quantitatively oriented social sciences and their methods suggestions (Justesen and Mik-Meyer 2012; Holstein and Gubrium 1997). Often this translates into the ideal of being "objective" in the interview situation, or at least appearing to be objective, and not to "influence" the situation too much, neither through unnecessary verbal expressions nor expansive dressing, and bringing in too much of oneself by expressing personal opinions about specific topics. We do not disagree that it is often crucial to keep one's own opinions, personal experiences, likes and dislikes in the background, in order to generate good empirical data (Silverman 2005, p. 256). We do observe, however, that an interview or the researcher's presence in the field is always already an irregularity and a provocation. It is this relational and realistic understanding that should be the point of departure for discussions of what may follow between researcher and informant, and not some ontologically mistaken idea of the researcher somehow not being present.

The researcher has no choice but to bring herself and her own perspectives, history and points of view with her into the situation. Contrary to what many seem to believe, this situation is also "hermeneutically productive" to paraphrase Hans-Georg Gadamer. Understanding and

interpreting social phenomena are in fundamental ways about the awkward meeting of different standpoints, that of the researcher and those of her informants. The contrast between the two makes both parties mutually aware, exactly because the situation is extraordinary. When "the other" appears to be different from me, it means at the same time that I become aware of my own particularity. The ideal of objectivity thereby runs counter to the possible (Haraway 1991).

A second characteristic of hermeneutic productivity is how it is exactly by twisting questions and taking advantage of situations that the researcher may be able to extract the most relevant data. It is right to strive towards bringing out the other person's viewpoints and perspectives, but the way in which it happens is precisely not by staying in the background, but instead by stressing certain topics, guiding the conversation towards the research questions, bringing out the informant's opinions by alluding to one's own and so on. It follows that good research is dependent on a completely different—or, more politely put, a more complex and socially situated—type of data gathering than what is recommended by many books on methods, and than is performed or otherwise accounted for in numerous academic books and articles (Brigg and Bleiker 2010; Sjoberg 2014).

Although many methods books in qualitative methods would run counter to the image of the withdrawn researcher, it is our experience as teachers and researchers, that the emphasis on discipline with regard to the preparations of, outline and conduct of research produces lip service to reflexivity (Mann 2016), rather than actually situated research. This, taken together with the overall lack of attention to what the researcher's own participation and style of communication may mean in practice, prepares the ground for the image of the controlling and reticent researcher, within qualitative as well as quantitative methods (see also Holstein and Gubrium 1997, p. 118).

Understanding the researcher and the research process resulting in knowledge production as fundamentally situated and in opposition to the scientific ideal of the objective knower, is not new. Several feminist scholars, notably Dorothy Smith (1974, 2005), Sandra Harding (1991) and Donna Haraway (1991), have problematized the "objective" (universalist "all knower") position and methods of approaching scientific data gathering and subsequent processes of data analysis relevant to the social sciences. They have rightfully criticized this position for being positivistic and unrealistic. In practice, no one is able to remain silent,

neutral and unaffected in the field or in an interview situation. If, perish the thought, anyone would be able to do so, she would by definition not possess the necessary qualifications for understanding the social situations that she is meant to study. The impossible is a bad research guide, hence the need for an alternative.

At this point in the argument, we have to confront that old chestnut, that a lack of objectivity spells relativity. We follow Harding here. For Harding, objectivity is a concept that rests firmly on a situated concept of the knower, different from the classical epistemological concept of the knower implying social and personal separateness (Pohlhaus 2012, p. 716). Harding writes:

> In an important sense, our cultures have agendas and make assumptions that we as individuals cannot easily detect. Theoretically unmediated experience, that aspect of a group's or an individual's experience in which cultural influences cannot be detected, functions as a part of the evidence for scientific claims. […] If the goal is to make available for critical scrutiny all the evidence marshalled for or against a scientific hypothesis, then this evidence too requires critical examination within scientific research processes. In other words, we can think of strong objectivity as extending the notion of scientific research to include systematic examination of such powerful background beliefs. (Harding 1991, p. 150)

Harding argues that the situated researcher does not produce relativistic knowledge. According to Harding, it is the sociological aspects of the knowledge production that is relativistic—our assumptions of a specific topic and our specific gaze, the relational gathering and analyses of data—but the knowledge produced may be what she calls, with provocative and yet well-considered hubris, strongly objective if the research process and its presumptions are clearly accounted for. The basic idea is that the more of the subjective aspects of the research process that are actually accounted for, the more objective the research. Harding continues:

> To enact or operationalize the directive of strong objectivity, is to value the Other's perspective and to pass over in thought into the social condition that creates it – not in order to stay there, to "go native" or merge the self with the Other, but in order to look back at the self in all its cultural particularity from a more distant, critical, objectifying location. […] Strong objectivity requires that we investigate the relation between subject and

> object rather than deny the existence of, or seek unilateral control over, this relation. (Harding 1991, pp. 151–152)

If, however, the understanding of knowledge production as situated is not an active and integrated part of all aspects of research activity, one risks doing research and writing up texts that, although they may not intend to do so, will present knowledge and analyses that appear to be universal, neutral and objective, that is, in the "weak" sense of Harding's concept of objectivity. Beverley Skeggs thus states that:

> It is the recognition of the situatedness of the knowledge production that has been key to the continual modification of [feminist] methodology. If methodology is a theory of the research process then the politics (interests, values, what is at stake in knowledge) are made explicit through the use of the performative 'feminist research'. (2008, p. 678)

In other words, to situate research means situating research in practice, and, we would add, somehow demonstrate this in the final text or research report. Perhaps part of the tendency to cover up the researchers co-constitutive position as a co-producer of knowledge can be attributed to the uncomfortable feeling associated with becoming conscious of the possibility of being positioned as inferior towards the generalized "academia" (intersecting with a particular social scientific style/fashion, editorial advice, academic culture, etc.), as observed by the French sociologist and anthropologist Pierre Bourdieu (2000) in his studies of social class. One of his examples concerns children with working-class background, with specific preferences for music, clothing and literature, who may experience sudden feelings of inferiority when introduced to the school system, which is constituted around the culturally authoritative set of preferences of the upper middle classes. The parallel to research and researchers is clear: to express and validate emotions, personal considerations and, at times, mistakes that have occurred during the research process—and to later make this relevant and visible in text production—may be linked to distress, shame and possible anxiety because such disclosures or confessions may indicate that the researcher has stepped out of her position as researcher/professional, and into the position as human/non-professional. The urge to avoid including such considerations and emotions in the resulting text may simply be due to a lack of comfort with one's own situatedness (Dauphinee 2013).

Let us immediately clarify that this is not an argument in favour of highlighting the researcher's personal emotions and experiences to the point of obscuring the knowledge transmitted from the field to the final text.

The point is that we situate ourselves not as a self-indulgence, but for the very specific reason that we want to clarify how our data actually appeared. It is here, on the level of method, that our book has something to add. While Smith, Harding and Haraway have successfully challenged the ontological and epistemological underpinnings of an objective research ideal, it is still rare to come across explicit reflections on the uses of methods and how they may intersect with the researcher's autobiography, presumptions, ideas, theoretical affinities and choices of subject in scientific work. Nor do we often see explicit reflections on what may be the effect of the researcher–informant relation on knowledge production. Even where the researcher explains that she is aware of the fact that she has affected and influenced the field and the conversations in the field, or that her particular and personal gaze has coloured the way she reads specific texts or situations, it is uncommon to share more precisely how this may have occurred (for a rare exception, see Dauphinee 2013). This is nothing less than a great weakness of many scientific works. In effect, it means that the reader is not given relevant information on some of the central premises on which the knowledge production is based, and so cannot make fully qualified evaluations on the quality of the research.

In this respect, this book is also a contribution to the debate on the professional ethos in social scientific research. Social scientists have analyzed and discussed the relationship between ideals and realities, discourse and practice; in numerous institutional contexts concerning the practices and tensions between different professions and professionals, but the researcher as a professional operating in the field—both as interviewer and fieldworker—is rarely critically examined with regard to the final knowledge being produced. Anthropologists often talk about the importance of not "going native", but reflections on *how* this may actually take place are few and far between, and when they do occur, it is often in specific volumes (Rabinow [1977] 2007; Okely and Callaway 2002 are, as we will discuss further below, paradigmatic examples; see also Brady 2017).

National guidelines for research in the social sciences and humanities aim to establish minimum ethical requirements for research as an enterprise. These guidelines formalize certain requirements to the researcher,

who among other things must ensure interviewees' voluntary consent, that informants can rightfully stop the interview whenever he wants and without consequences, and that informants should not be recognizable in the final text. But they do not address the importance of that which the researcher brings to the field; how she ought to deal with personal emotions towards the field and its individuals; and how she can handle this ethically and fairly and based on explicit practice and experience accumulated in the research community.

Given that this book is concerned with three types of situatedness, and that we use some concepts and perspectives originating from the Gestalt tradition to shed light on and better understand these three types of situatedness, we have organized the book as follows. To illustrate how this works in practice and (hopefully) show the readers how the Gestalt tradition may be helpful, the following chapter is dedicated to three examples from the field. We focus primarily on the research process—that is central to a book on research methods—but we also provide examples of autobiographic situatedness and text situatedness. In order to place the book and its authors historically, and to contextualize our approach and techniques, Chap. 3 introduces the Gestalt tradition. Chapters 4 and 5 present the concepts and perspectives from the Gestalt tradition that we use, before we return to the questions of doing autobiographic and textual situatedness in Chap. 6. In Chap. 7 we engage in a somewhat technical discussion of situatedness, departing from critical interventions in the philosophy of science debates on the self and subjectivism. We conclude by summarizing the main points of the book, provide a discussion on configurations of power between researcher and informant and suggest that situatedness may take two different form, depending on whether one starts from how the social reality studied changes the researcher or, alternatively, from how the researcher changes the social reality that she studies.

Note

1. Note that we do not understand or apply the Gestalt tradition in the strict sense of the phenomenological research tradition's emphasis on exploring peoples authentic meaning and life worlds as such (Järvinen and Mik-Meyer 2005, p. 29). Rather, we seek inspiration from some of the Gestalt traditions concepts and suggestions of how we, as researchers, can know ourselves better and situate our research and ourselves in relation to our involvement with different research contexts throughout the three stages of the research.

References

Bourdieu, Pierre. 2000. *Pascalian Meditations*. Oxford: Polity Press.

Brady, Ivan. 2017. Other Places and the Anthropology of Ourselves: Early Fieldwork in Tuvalu. *Qualitative Inquiry* 23 (3): 179–191.

Brigg, Morgan, and Roland Bleiker. 2010. Autoethnograpic International Relations: Exploring the Self as a Source of Knowledge. *Review of International Studies* 36 (3): 779–798.

Dauphinee, Elizabeth. 2013. *The Politics of Exile*. London: Routledge.

Haraway, Donna. 1991. *Simians Cyborgs and Women. The Reinvention of Nature*. London: Free Association Books.

Harding, Sandra. 1991. *Whose Science? Whose Knowledge? Thinking from Women's Lives*. Ithaca, NY: Cornell University Press.

Holstein, James A., and Jaber Gubrium. 1997. Active Interviewing. In *Qualitative Research. Theory, Method and Practice*, ed. David Silverman. London: SAGE.

Holstein, James A., and Jaber Gubrium. 1995. *The Active Interview*. Qualitative research Methods Series 37. London: Sage.

Järvinen, Margaretha, and Nanna Mik-Meyer. 2005. *Kvalitative metoder i et interaktionistisk perspektiv: interview, observationer og dokumenter*. copenhagen: Hans Reitzels Forlag.

Justesen, Lise, and Nanna Mik-Meyer. 2012. *Qualitative Research Methods in Organisation Studies*. Copenhagen: Hans Reitzel.

Mann, Steve. 2016. *The Research Interview. Reflective Practice and Reflexivity in Research Processes*. New York: Palgrave Macmillan.

Okely, Judith, and Helen Callaway (eds.). 2002. *Anthropology and Autobiography*. London: Routledge.

Pohlhaus, Gaile J.R. 2012. Relational Knowing and Epistemic Injustice: Toward a Theory of Willful Hermeneutical Ignorance. *Hypatia* 27 (4): 715–735.

Rabinow, Paul. [1977] 2007. *Reflections on Field Work in Morocco*, 3rd ed. Berkeley: University of California Press.

Silverman, David. 2005. *Doing Qualitative Research*, 2nd ed. London: SAGE.

Sjoberg, Laura. 2014. Gender/Violence in a Gendered/Violent World. *Millennium: Journal of International Studies* 42 (2): 532–542.

Skeggs, Beverley. 2008. The dirty history of feminism and sociology: or the war of conceptual attrition. *The Sociological Review* 56(4): 670–690.

Smith, Dorothy E. 1974. Women's Perspective as a Radical Critique of Sociology. *Sociological Inquiry* 44 (1): 7–13.

Smith, Dorothy E. 2005. *Institutional Ethnography: A Sociology for the People*. Kanham: Alta Mira Press.

Staunes, Dorthe, and Dorthe Marie Søndergaard. 2005. Interview i en tangotid (Interview in a Time of Tango). In *Kvalitative metoder i et interaksjonistisk perspektiv*, ed. Margaretha Järvinen and Nanna Mik-Meyer. Copenhagen: Hans Reitzel.

CHAPTER 2

Pre-field Autobiographic Situatedness, In-field Situatedness, Post-field Text Situatedness

Abstract In this chapter, we move closer to our subject and give examples from pre-field, in-field and post-field situatedness in research. We begin to demonstrate how we can do situatedness in practical research, and to open the text to some of the ethical challenges and power/knowledge issues that research is always engaged. In the chosen examples, we apply some of the Gestalt concepts and understandings of relations, which we see as complementary to books and manuals on contemporary methods. Further explanation of the Gestalt concepts will follow in Chaps. 3–5.

Keywords Situatedness · Phenomenology · Researcher ethics
Emotions

To situate means to locate or to place. Situs is Latin for "site" or location, and position. In situ means on-site or in-place. Location or being placed is a relative phenomenon—you are located or placed in relation to something. A person can be in his right place or in a right location. In archaeology, in situ refers to an object that is still located where it was originally found, while other disciplines use in situ as a reference to a place that is reconstructed so that the object can be situated in its original environment. In the social sciences, situating concerns locating the author (or the producer of knowledge) in three ways: we call them field situatedness, autobiographic situatedness and text situatedness.

C.B. Neumann and I.B. Neumann, *Power, Culture and Situated Research Methodology*, DOI 10.1007/978-3-319-59217-6_2

Other researchers use the term "positionality", stressing the position of the researcher relative to interlocutors rather than the site taken up by her as such (Damsa and Ugelvik 2017; Dottolo and Tillery 2015). Given that sites are as relational as positions, the difference between the two terms "situatedness" and "positionality" seems to us to be of little consequence.

Let us start with the conduct of research in the field. When we carry out observations or do an interview, it is relevant how we experience and are experienced by those people we interact with and observe in the field, because such experience is one factor that determines what these people talk about, how they talk about it and how much they talk about it. The unsaid and undone may in this context be decisive for the data created in the field and how we understand that data. It is the researcher's task and challenge to reflect upon this. We call this field situatedness. To reflect upon this is important not only because it concerns the continuous positioning of ourselves as researchers in the field, how that positioning impinges on what happens when we are in the field, but also because there exists a field, in the psychological sense, between the researcher and her informants or interview partners. This type of inter-psychological field is thoroughly studied by one school of psychologists, the Gestalt psychologists.

Gestalt psychologists are phenomenologists. A key point in phenomenology is that the past and the future always exist in the present, as experiences and as expectations of consequences, respectively, and this has implications for our autobiographic situating. It follows that researchers not only are field situated—i.e. situated here and now in the present situation of doing research in the field–but are also situated by virtue of their past and of their future. The past situates the researchers socially in the sense that they carry with them their own social positioning into the research situation. Their background and their ways of being in the world (sex, ethnicity, class, religion, etc.) are relevant for what they study, what kind of research questions they choose to inquire and how they go about studying them. We are "always already" socially situated before we interact with somebody, to use a basic insight from hermeneutics, a near cousin of phenomenology. Psychologist George Herbert Mead divided identity in two: "I" experience that I am this or that, while "me" is what the significant others experience as me\attribute to me. While field situating is about reflecting on how the others' experience of the researcher-self (me) continuously influences the data collection processes,

autobiographic situating concerns reflections on how the researcher-self developed through a myriad of personal experiences and past relations (primary socialization by the family; secondary socialization into a specific type of social scientific researcher, etc.). What we call autobiographic situating is thus reflections on one's own social position, experiences and background pre-field, or, to use a colloquialism, "where we come from". To be aware of our autobiography is important because meeting informants in the field triggers certain aspects related to our autobiographies. Doing autobiographical reflection is also important throughout the research projects, because one's own history contributes to deciding the choice of research and how to approach it not only before we enter the field, but also during our stay there. Autobiography bleeds from the pre-field to the in-field phase.

While researchers are field situated in the present, and autobiographically situated by their pasts, they are also situated with respect to the future. Researchers usually have expectations with regard to the results of the research that concern writing up of text as well as the expected publications, and the possible effects of those publications. One social scientist who wrote a book about cannibalism chose the topic because he wanted to make a big public splash upon publication.[1] Such a pre-field choice cannot but impinge on the research and so should have been noted in the published book, but was not. By any standards, this is a non-choice that should be hard to swallow for any methods-aware social scientist. To generalize, the researcher is situated in relation to the processing and finalization of the collected material. Moreover, different requirements apply to the presentation of data in different contexts, such as the writing of a master thesis versus a scholarly publication. The publication will be presented differently according to the genre one chooses (essay, article, chapter, book). Using the material for the second time you, will think differently about it and use it differently than you did in the first analysis. Handling the material also has a legal side. If a criminologist writes about an illegal affair that she has seen or participated in in such a way that stakeholders can be recognized, she may be partially responsible for lending a hand in bringing these people to justice. If an anthropologist does not inform her reader that during her fieldwork sexual abuse of children took place, and such practices are illegal in that country, she may be subject to legal punishment. Furthermore, there are ethical considerations involved: is it, for example, ethically responsible to portray a politician in a way that damages his character based on interviews

with his political opponents? Is it ethically responsible to let one's feminist political stance on women's care work as embedded in the suppression of women, colour one's views of recipients of care in home service arrangements? These examples also point to the political situating of the writing situation. Researchers' publications are often used to legitimize or marginalize the topics, persons, groups or organizations they have researched. They can, of course, not predict the consequence of their actions any more than others—their rationality is limited, as political scientist Herbert Simon (1997 [1947]) pointed out 65 years ago. This, however, does not mean that the researcher may suspend the commitment to reflect on the consequences of depicting informants in specific ways, or of the commitment to reflect on what the consequences of publishing a specific text may be.

The research process is attached to the *Gestalt* world. When we write data into text, we seek the most complete and coherent work. This book, for example, started as a hazy idea, then bloomed first into a chaotic collection of observations and then a somewhat less chaotic collection of text fragments, only to end up as a series of consecutive drafts. All along, the basic thrust was towards greater coherence. Text situatedness is, not surprisingly, related to the *Gestalt* text. Note that the psychological fact that we search for Gestalts does not in any way imply that the world consists of ready-made Gestalts; our identities, research processes and texts are, and must be, far from perfectly complete and whole. If they already *were* wholes, there would be no point in striving towards wholeness. Moreover, both the researcher, the meeting between the researcher and the rest of the world, and the text will continuously appear in different versions. The researcher learns from the research process and is thereby changed by it, the research process changes the world (in most cases insignificantly, but the point here is that doing observation principally changes that which is observed), and the text will keep changing in the sense that it will have ever new receptions—or, more commonly, ever less reception.

In order to situate ourselves, we will now give some examples of in-field situating, pre-field autobiographic situating and post-field text situating from our own research. The first example of in-field situating is from a fieldwork and a subsequent interview with an adolescent from a residential care institution for youths. The example shows how the researcher uses information she has gained through her fieldwork; from participant observations of social interaction and conversations with

different actors, to display sensitivity and cautiousness during an interview. The second example, regarding autobiographic situatedness, originates from an autobiographic text from a meeting with a public health visitor in a public health clinic and reflects upon the meaning of this meeting for the author's subsequent research on public health clinics in Norway.[2] The third example, on textual situatedness, is based on a text where the researcher subject is only implicitly situated in the text. The example shows how the researcher's analytical perspective has grown out of her conscious work with emotional reactions and categorizations as part of her autobiographic pre-field situating and in-field situating. She nonetheless finds it difficult to include this analytical work in the published text.

Field Situatedness: The Pillow

I (Cecilie) am in the residential care institution where I am doing fieldwork. One of the girls, Vilde, one of the social workers and I talk about how it is to be a child/adolescent, how awkward and like an outsider one may feel, and how it is to make a fool of oneself and to feel embarrassed. Another social worker arrives to show us a poem written by one of the other girls, Gro. The poem is about loneliness. Each of us reads it, and we all tell Gro that we think it is a beautiful poem. The social worker says: "I wouldn't be surprised if she becomes a writer". Gro, the girl who wrote the poem, smiles shyly, but also looks proud and asks me: "did you really like it?" which I confirm that I do. We spend the rest of the afternoon making Easter eggs and having fun. Gro has the touch and makes beautiful eggs. I am clumsy, I spill glue and my eggs end up being rather ugly and uneven. The day after, Gro arrives with a large box. I am sitting at the kitchen table in the common room together with the social worker from the day before, who is also Gro's personal contact person. Gro opens the box. In it she has a collection of pictures from when she was newborn in her mum's arms and up until about 7 or 8 years ago. She is going to make a photo album and has asked the social worker to help her. Considering that she is so crafty and has an obvious sense of aesthetics (which I read from the way she dresses and applies her makeup), it seems to me that she does not need any help to do this job. It turns out that this is true; she does not need help to make the album. What she needs is to be with the social worker, it is to her Gro wants to show the pictures and share with her a part of her life. I sit at the

other end of the table and try not to appear too pushy—eager to see the pictures—or appear uninterested and dismissive by getting up and move somewhere else as if I assume she wants to be alone with the social worker. Gro asks if I want to look at the pictures. Both the social worker and I are deeply touched by the pictures, which nearly makes us cry (we talk about this afterwards). Gro shows us pictures from a life that looks completely standard. There are pictures from when she was an infant together with mother, pictures from several Christmas parties and birthday celebrations, from the first day at school, from playing in the living room with her siblings and at the playground outside of the house she grew up in. Then something must have happened, because after a certain age there are no more pictures of Gro.

Later, when I am about to do an interview with Gro, I have some information about her, and she about me. I assume that she wants to talk to me, she has consented to be interviewed, but I also think that she is understandably ambivalent towards me as a researcher and an outsider. My observations of her way of being in the institution and of her body language in this and other contexts have given me some ideas about which kind of questions and topics she will probably feel offended by and is therefore likely to avoid. What this means more specifically though, is at this point still a matter of guessing.

Entering the meeting room where we are going to do the interview she says: "Is this going to take a long time? You see, I'd really fancy a cigarette". I answer "No no, you decide, we talk as long as you feel like it".

My interpretation of this statement is this: Gro immediately tells me that she feels like doing something else. This may be because she actually feels like having a cigarette just then, but it may also be an expression of insecurity about what I will ask her. I interpret the statement as the latter, and as a warning. I need be careful in the sense that she wants to make her own decisions, and that I have to respect her personal boundaries in the interview situation. My answer indicates that this is the way I interpret her.

We enter the room and sit down, me on a chair, she on a sofa. There is a table between us. I comment that the room is a bit cold and ask if I shall turn up the heat. She answers that it is not necessary but simultaneously covers her belly with a pillow and wraps a blanket over her shoulders. She holds the pillow tightly over her belly. Registering this, I am thinking that this is another demonstration of personal boundaries, and

that I should not expect her to talk about things she is not ready to talk about. Then she says: "You have to make sure they don't take this institution away from us. It is a really good place to be".

I answer "Yes ok, could you say a bit more?"
She replies "The best thing about being here is that you can be left alone. Everyone here has experienced things, but we don't need to talk about it all the time. The social workers are nice, and they make us feel safe. This is a place where I can relax—and I really do relax here".

This is clear speech. She has two important messages for me. First, that she is afraid that I may be a researcher that could ruin the institution she is so fond of. Second, that this is an institution where everyone knows that the reason she is here is that she has been abused, but she does not have to talk about the difficult things all the time. This is a place where she can relax. In other words, she asks me quite clearly to avoid asking questions about the difficult parts of her life, like how it is to be at home, and why she is in the institution. The figure is fairly clear: Do not invade me.

During the rest of the conversation, we talked a bit more about the good things that characterized the institution and also what she missed and would she wants more of. She ended the conversation after 20 mins by asking: "Are we done now?"

For my research agenda, which was to evaluate the institution and, among other things, inquire into the collaborative work between the social workers and the youths and look for possible positive effects and developments in the youths spending time there, it would no doubt have been useful to learn more about Gro's background the way she understood it. However, I considered it completely unethical to ask her about this. She drew clear personal boundaries through strong bodily signals, and by repeatedly telling me that what was good about this institution was that she was not pushed or manipulated into telling anyone about her difficult early life or present situation. She could talk when she felt ready for it. Note also, and there is no point in denying this, that there is a confluence between ethical and instrumental concerns here. If I had thrown ethics to the wind and pressed on, what kind of quality would my ill-begotten data have had?

Of course one need not be a Gestalt therapist to understand that an informant who wraps a blanket around her shoulders and holds a pillow tightly over her belly most likely expresses a clear desire not to be invaded and to have her personal boundaries respected. But, if I had

been just a little less experienced or perhaps just another type of person, I might have interpreted the pillow and the blanket as a sign that she was cold, and the statement about the institution as an invitation to talk more about how her life was when she was not in the institution. To me, it was the first interpretation of her body language and of her verbal expressions, reinforced several times during the conversation, which became the figure—"don't invade me"—and which became decisive for the continuation of the conversation and the questions that I asked during the interview.

Now, I cannot be completely sure of this interpretation. I could not ask her directly. Still, since I spent time doing fieldwork in the institution and therefore had the chance to observe her in many different situations interacting with the other youths and the social workers, I am as certain as I can be that this was a correct interpretation. In addition, the staff at the institution later confirmed my interpretation, at least indirectly, in staff meetings.

How, then, should an inexperienced student that finds herself in a similar situation behave and react? Or, can an inexperienced student be sure that she will be able to handle a situation like this at all? What is the actual challenge here? The challenge, as we see it, is as simple/straightforward as it is complicated; a researcher may get a lot of help from taking bodily signals seriously. A pillow pressed hard towards the belly may mean several things to the informant, but it most certainly is a signal that the researcher should be careful, whether it is because the informant is very shy, has the need to demonstrate personal boundaries, is afraid to be invaded, or whishes but finds it emotionally difficult to talk about a specific topic. In addition, the more vulnerable, deprived or marginalized informants are, the more sensitive to bodily signals of personal boundaries the researcher ought to be.

In Chap. 1, we made the point that the researcher creates rather than collects her data. The example of the pillow is an example of this. It was awareness in the field situation that made me pick up what was going on—I was relationally situated then and there, in the present. One reason why no two researchers who do field work will come up with the same data lies in the fact that they will be differently relationally situated in the field. We have just given an example of how one of us was situated in one specific field, and should like the reader to think through how he or she would have been situated differently in terms of gender, ethnicity, age, and so on.

Situatedness does not stop there, however. How we are situated spatially in the field and temporally in the present also depends on how we are situated in a different temporality, namely that of our own personal histories. One reason why I picked up the bodily boundary setting so easily was to do with my own history of learning about bodily boundary setting. Like all humans, researchers cannot observe anything if they have no categories. One's categories come from one's cultural background, and also from one's personal history. What we observe in the present depends partly on our pasts. We now turn to this challenge of self-reflection, which we call autobiographical situatedness.

Autobiographical Situatedness: Women at the Public Health Clinic

As we mentioned in the beginning of this book, there are great similarities between the phenomenon called preconception and what we in this book call autobiographic situating—the latter is the process by dint of which the researcher strives to understand the former. Preconceptions originate from and are the result of the researcher's own life experiences from everyday life, studies and professional work—and is therefore to some extent available for reflection.[3] This process of reflection not only enables the researcher to be as conscious as possible to what he or she brings of perceptions, opinion, knowledge, experiences and prejudices into the research situation and how and in what ways her particular gaze influences and decides what she sees and finds interesting, but also brings into attention how and why she co-constructs knowledge with her informants about the subject of inquiry. The importance of being conscious of preconditions and judgments were paramount to the founders of both phenomenology (Husserl) and hermeneutics (Gadamer) when they outlined how our interpretation of texts and of the world works. It is, however, primarily feminists who have emphasized the influence of the researcher's autobiographies; our preconditions, presumptions and judgments on the production of knowledge within the research enterprise. They have insisted that all knowledge is situated, specific and local, and so have challenged the universalist claims of both knowledge and the "objective" and "analytically unaffected" knower (cf., for example, Haraway 1991; Harding 1991, 2015; Knorr Cetina 1999; Skeggs 1997; Jackson 2011; Sjoberg 2014). On the contrary, and as laid out in the

previous chapter, Harding in particular but also Knorr Cetina and others have convincingly shown how knowledge production even in the natural sciences are shaped by the researcher's preconceptions, specific and general cultural belonging, gender, nature, politics, etc.

When we choose to use the term autobiographic situating and not preconceptions, it is because we want to be more explicitly able to discuss the meaning of the researcher's personal background and experiences, although constantly shaped and reshaped when encountering the field, and, as we will address later in this book, to address the questions of awareness and body language. At this point, we choose to bracket the influence and meaning of empirical and theoretical preconceptions (professional), even if our own experience is that this will be included in the preconceptions as a part of a more experienced researcher's autobiography. We will return to this dimension of autobiographic situating in Chap. 6.

The next example is from Cecilie's doctoral research on public health visitors and their experiences with, and handling of, children they suspected to be victims of neglect, violence or other forms of abuse (Neumann 2009).

When I started interviewing public health visitors, it soon became clear that not many knew much about abuse and violence against children. The dissertation therefore ended up inquiring into the meaning of the public health visitors' worries about children and their parents in general, and their worries about children potentially subjected to violence and abuse in particular. I approached this by asking them what they regarded as their main interests and concerns during consultations, what they were looking for in children and their parents, and what they were observing during such meetings. The analytical framework of my research was guided by the relationship between normality and deviation, and the question of what were the conditions for action that shaped and conditioned the work of the public health visitors.

Choosing public health visitors as my subjects of study was not completely accidental, but neither fully planned. When I had my first child I was a graduate student in sociology. My first encounters with the public health clinic left me provoked to the extent that I often discussed with my fellow students how strange it was that so little research and debate was devoted to such an intrusive institution, which intervened in the life of parents and their parental practices to the extent that it did. I did not let go of the idea of studying health visitors, and several years later, when

I learned that I had received funding from the Norwegian Research Council for my Ph.D. project on public health visitors, I happened to have a clean out in my loft. There, in a little box full of old letters, I found a text I had written after a visit to the local health clinic when my first child was about one year old. It went as follows:

> Hedda and I have been at the public health clinic. Hedda is 1-year-old. The health visitor knows everything about children. She knows what Hedda must eat and drink. She knows how tall and how heavy Hedda ought to be, and she knows when she should crawl, climb, walk and run, when she should draw and read, and she knows which toys Hedda should play with and how she should play with them. "Does she pick crumbles?" "Does she play with bricks?" "Does she put toys into crates and take them out again?" the health visitor asks. And I answer yes and tells her what Hedda does and what she knows. But when Hedda and I are at the public health clinic, Hedda is not fully "herself". She doesn't like to be dressed and undressed several times. She doesn't like to be weighed on the scales, and she doesn't like to be placed on the bench for measuring. Hedda cries and the nurse asks if Hedda has had a bad day.
>
> The nurse introduces toys to Hedda to see if she plays with them "correctly". "Does she put the bricks into the crate? Does she add and remove the rings to the pole?" But Hedda is unhappy and doesn't want to play with the health visitor's toys, she only wants to sit on my lap.
>
> The health visitor is running short on time. She asks what Hedda eats and drinks, how often and how much. The health visitor explains that liver pate and brown cheese are important to eat because they contain iron, and iron is crucial for Hedda's physical and mental development. To have a bottle of milk in the evening after toothbrush is a no-no; the teeth must be kept clean and fluorinates taken every day. Then the health visitor asks if Hedda still breastfeeds and I say yes. Then the health visitor comments "we usually recommend weaning when the child is around one year, if not the breast can easily become a comfort object, and it will be difficult for her father and other adults to be care persons for Hedda".
>
> Was there anything else? No, that was it for the 1-year check up. See you again in 5 months. Hedda and I proceed to the doctor, and the health visitor receives the next child in line.
>
> Hedda and I get dressed. I put Hedda in the pram and we walk home. Do public health visitors really know everything about children, I wonder. Are there, for example, other good sources of iron than liver pate and brown

> cheese; for what if Hedda doesn't like brown cheese, and I don't want to give her liver pate? Is it really so that breastfeeding after 1 year is bad? And is Hedda really lagging behind in her development because she doesn't want to play with the health visitor's toys?
>
> I feel upset and angry on the way home from the health clinic. Darn control and bugger intrusive interference, I am thinking. I feel that both Hedda and I are surveilled without any reason whatsoever.
>
> For I know Hedda is a wonderful child. A normal, loving and healthy child, who plays and screams, smiles and cuddles, cries and laughs just like any other children her age. She crawled when she was 6 months old, walked when she was 10, she says hello and goodbye, she knows how to clap, wave and show how big she is, she turns the radio on and off, plays with cars and dolls, examines her environment and is curious and eager to learn. And I am proud of my daughter, for all she is and all the strange things she does, but after having visited the public health clinic I am full of doubt and indignation.

This was how I thought then, and it was an important part of my situatedness once I entered the field to study health visitors professionally. Many years and several children later, I have a different relationship to public health visitors and their clinics. In my doctoral work, I emphasized that this was due to less vulnerability on my part, but also that I later encountered nurses with different approaches and foci. I wrote that what I then reacted to with great vulnerability today appears as a standard programme for control, and potential education of me as a mother, as a way of ensuring (the public interest in securing) my child's well-being. I also wrote that I later experienced the public health clinic as a nice and friendly place to be. This experience, however, became important because it alerted me to move from ethical considerations defined by proximity in the health visiting work (the mechanisms framing the professional care given to the specific "other" in the here and now), to an understanding of the public health clinic as a place framed by a public health, second-order care perspective, and to develop an analytical framework to study power relations. I realized that one of the most vulnerable elements of the health visitor–parent relation probably is embedded in the fact that while the child is a specific and (hopefully) significant other to the parent, the child is a specific but first of all a generalized child to the health visitor. This analysis made it possible to see, perhaps paradoxically, that compared to other similar professions such as nurses or social

workers, public health visitors do not exert the same degree of authority. This is due to several structural conditions for action and circumstances not to be discussed in detail here. The interesting part of the analysis was that the public health visitors possess a type of power or influence that partly resembles but also diverts from the types of power normally discussed and problematized in research on professionals' relationships with their clients or users. The power of the public health visitors is more general and less specific, which makes ethical concerns of proximity less relevant to the health visitors, e.g. the possibility to offend or violate clients during consultations presupposes a relationship that is understood as important or even imperative to the client in the sense that the clients material or physical well-being depends on the professionals judgments and decisions, and this is not how the health visitor–parent/child relationship is structured. Their position as a part of the state's power apparatus through the indirect governing of parental practices becomes, however, quite visible.

Before reaching this insight, however, my thinking was for a long time affected by prejudices towards the public health visitors as a profession. More precisely, my personal reactions due to my earlier experiences had turned into a generalized understanding that public health visitors represent women who diverted from the female gendered expectation that they qua women and nurses *should* be adequate care providers. Despite the fact that I consider myself a feminist, and despite my knowledge of feminist voices having pointed out that these female gendered expectations towards female care workers are problematic for women as workers and carers in different public and private contexts (Hochschild 1983; Leira 1992; Skeggs 1997; Solheim 2007), I had a hard time getting rid of these gendered expectations. In effect, I had to confront them actively in order to be able to approach the public health visitors analytically. My preconceptions came in the way of taking seriously what they said about their work and their experiences as health visitors, and also stood in the way when I tried to understand the conditions for action that shape or constitute their work. It was this active situated confrontation with my gendered prejudices against the public health visitors, together with academic discussions with Iver (another example of situatedness), that paved the way for this particular Foucauldian power analysis.

"I" have so far in this example been Cecilie. If I compare—and now "I" am Iver—Cecilie's notes with my own experiences with public health visitors, it becomes embarrassingly obvious how important situatedness

is. My preconception of public health visitors was that she was the one feeling our private parts at school to check if the testicles had fallen down as they should. It was she who served some wicked fluid on a biscuit as vaccine, who shot the Tuberculosis vaccination on the shoulder that left a scar that is still visible, and who was never there when we fell and scrubbed our knees in the schoolyard and needed comfort. The last time I encountered a member of this profession was during three visits to the public health clinic with our son Iver Jakob. The first time Cecilie and I went together. I have during my 54 years never—and I mean never—felt so ignored. The health visitor did not greet me when we arrived, consistently addressed the mother, and did not respond to my questions. The other two times I was there on my own. Then the health visitor insisted on teaching me, who have been involved in raising six children, how to change a nappie. They complimented me for protecting the baby's neck. They became insecure when I asked questions. What is interesting in our context is that they were probably unaware of the fact that they appeared socially and professionally wholly incompetent. My presence did not match their preconception, which dictated that mother should have been present during the consultations. While they probably had oodles of experience in dealing with men in many other situations, they had no training in relating to them in a professional context. Researcher, be aware, that is, beware: you do not want to appear to your informants the way the health visitors appeared to me. Moreover, beware that you, as a researcher, does not let your own preconceptions (or, if you like, prejudice) get the better of you and stop right here. Try to objectify the situation and take into consideration how your presence may *produce* what you see. In these cases, the health visitors were young, lower middle-class female, while I was an older upper middle-class male. My immediate judgments of incompetence may be unfair, even wrong, given that I did not see these health visitors interact with males whose age and class were more similar to their own. An alternative reading, then, is that the situation was not overwhelming for these health visitors because I was a male, but because I was a specific kind of male. Further research by a male other than me is needed before generalizations can be properly made.

The point in giving this example is to show that the autobiographical dimension is important and active, and that it must be subjected to critical self-analysis, whether its particularities become clear to us pre-field, in-field or post-field, so that our experiences do not sneak up on us from

behind and cloud our researcher's gaze. Here, students and researchers may learn from the Gestalt tradition, which deals precisely with how and why the therapist ought to have clarified as many of her central life experiences, values and attitudes (and neuroses) as possible, as this work will enable her to act ethically in the therapy situation, and to meet the individual client as openly and without prejudice as possible. This is an ongoing process through the different stages of research as well as in life in general. A challenge that one of my teachers (and now Cecilie is writing) in the Gestalt education once gave was this: "Can you recognize the aggressive impulse that makes you understand why people hit one another, and even, in extreme situations, will kill each other?" Most of us students answered by denying having had contact with such an impulse. My teacher then responded: "Well, what do you do, then, when you receive a client that needs to process such an impulse?" His message was that if the therapist does not acknowledge and know her own aggressive impulses, chances are that she will enter a morally condemning mode when confronted with a client's aggression, and subsequently, will be a far worse therapist than if she did understand and accepted this impulse as something she could recognize in herself.

It does not make sense to expect an identical self-analysis of a researcher. Our aim is simply to point out that in many different contexts during the research process, it is valuable to reflect on the autobiographic aspects activated when inquiring into a field, be it pre-, in- or post-field. Autobiographical reflections may be analytically useful before the physical meeting with the field (when choosing a project, asking why exactly this project was found interesting), when reading the relevant literature, when preparing research questions and questions for interviews, and during and after experiencing the field. The work of reflecting on how the researcher's own autobiographical status impinges on her production of data is in principle never-ending. It will follow us into our introduction of the third kind of situatedness that we will discuss in this book. If we think of the creation of data in the field as the present tense of research, then autobiographical situatedness will lie mainly in the past. What will also always at some level be with us in the field, however, are the intentions about what to do with the data we create later on.

One key bit of advice is not to concentrate too much on this, for that would detract from the awareness one needs in the field. Still, intentions are always there, and intentions concern what to do in the future. We are moving into the realm of what we call textual situatedness, by which

we mean which data to include and which to leave out in the texts that are the results of our research, and also how we situate ourselves in the finished text. The latter concerns what we tell the reader about our field positioning (was I having an affair with the main informant and, if so, what is the significance of that for data production?), what kind of autobiographical baggage we took with us in there (did I give priority to producing data about diplomacy since I myself was already fascinated by it before I entered the field, or was there something particular about local diplomatic practices that really commanded my interest?) or whether we make our own changing understandings of what is going on in the field a clue line of the text (see Neumann 2007). We will return to that in Chap. 6. Here, we want to concentrate on the anticipations of what will go into the text, what we could perhaps call pre-textual situatedness.

Textual Situatedness: The Example of Motherhood

The next example is an excerpt from Cecilie's field diary and the following analysis from an evaluation of a home-based children's residential institution (Neumann 2010). My task was to assess whether or not this institution supported the children's social and emotional progress, and to give an account of how the social workers conducted their care work in this regard. My autobiographical frame of reference as a mother was a central, but implicitly articulated, premise for the analysis, and this reference served as the basis for a brief theoretical discussion on different approaches to care and care work based on the differences between private parental practices and working professionally with children and adolescents in institutions. The challenge of textual situatedness is among other things an ethical one—how might it affect my interlocutors that I include this or that observation about their lives in my finished text (see also Dauphinee 2013)? Second, there is the challenge of style as well as that of courage. Although I included autobiographical points of reference in the text, I struggled with my courage. How would my employers, who had paid for the research, react when I used my experiences as a mother as an analytical strategy to interpret the social worker's professional care work? Here is an everyday narrative from the field that was included in the final text:

> Two of the youth, Ruth and Anders, are standing by the kitchen counter, joking. One of the social workers is with them. Anders constantly "bumps"

into Ruth. The social worker looks at Anders and says: "It would be great if you could fill the dish washer now, (smiling) I think you are able to do that". Anders replies: "Yup, it's good that I don't have to clean those darn cups". Then he puts his own cup and a couple of the other cups on the bench in the dishwasher. He grabs some food and milk from the fridge and climbs the stairs to the second floor where one of the other social workers is available for helping out with schoolwork. Ruth seeks more contact with the social worker than I have seen her doing for a while. She continues to stay in the kitchen talking to the social worker. They talk about which film they want to watch later that week. Anders noisily reappears. He walks with heavy steps. He asks if he can make a cake, and the social worker says he may. She helps him find the right kitchen wear and some of the ingredients necessary for making the cake. A bowl, measuring cup, cake mix and butter; the rest he manages on his own. When the dough is ready he proudly parades it around the kitchen and living room, asking if people are looking forward to taste his cake. Ragnar, another of the youths, seems calm and content. He talks more, jokes and smiles more than he did a few weeks ago. I move into the living room and sit down next to Ragnar and one of the social workers, thinking that their life in the institution to a large extent resembles life in an ordinary family. But there is still an institutional dimension to this life or way of being together.[4] It is something about the staff's patience and continued presence that contributes to the institutional feel. The staff is very pedagogical—much more so than I am with my own children. Episodes like the following are not uncommon:

Do you want me to help you with your homework?

No, I am tired

Ah, come on, let's go upstairs and do it, it's better to be done with it

"No, I want to wait a little". The boy picks up a magazine, and the conversation ends.

[…]

If this had been one of my children, I would have insisted on the homework. (Neumann 2010, p. 49)

Had I insisted on the homework, as in the example above, I would have created a situation that most probably would have ended up in a conflict. The question then is; what exactly is it that the social workers do, as opposed to what (good enough) parents would do, that is therapeutically helpful for the children?

It was this gap, between my own emotional reactions in the situation and reflections on how I would have handled my own children differently in similar situations, and the social worker's emphatic yet emotionally detached actions, that finally allowed me to appreciate the professional care work done by the social workers, as distinct from care given in a familial environment. Thus, my autobiographically situated categories of the idealized child–parent relationship, bled into my initial evaluation of the social worker–child relationship. Acknowledging this eventually allowed me to analyse care as a sociological concept with this specific institutional context as a point of departure. This is how I wrote up the published analysis:

> My observation is that the social workers have flexible and generous boundaries towards the youth, meaning they are much more patient with them than I am with my own children. This does not mean, however, that they do not set clear boundaries or mark the limits of what is and is not allowed and expected of the youths in the institution, and neither that I think they should set clear boundaries more immediately. Providing a youth with care in a public institution is different from giving care in a parent-child relationship, where mutual love and trust, at least ideally, defines the relationship (for different viewpoints and discussions of this, see Løgstrup 1997; Wærness 1992; Leira 1992). If establishing too strict boundaries and limitations to what is and is not allowed in a context of professional care and therapeutic activities, the social workers risk jeopardizing the main goal of assisting the youths to (re) build trust and confidence in themselves and their social surroundings, and to start seeing themselves as worthy and competent actors. Finding the right balance in the conduct of discretion between individual care for the specific youth, defining acceptable and not acceptable standards of behavior that may apply to all, and practicing rules in common and predictable manners, is indeed topics of discussion among the staff in this institution. They are, however, equally concerned with the professional ethos of self- reflexivity as a fundamental prerequisite for doing good professional social work. They talk about how they perceive the youths, how they react at their actions personally/professionally (like how they feel and what they do when the youths are displaying signs of being in need of care and attention through intimate as well as aggressive acts), while maintaining the professional focus of not making the youths into individual deviators (she is a hopeless case, something is wrong with her) or deviators in comparison to the rest of the group (he doesn't really fit in here). (Neumann 2010, p. 50)

Note the weak textual situatedness here: It should be obvious that I draw on personal experiences, but I do not say so directly. We will come back to this, but first, we want to dwell on how this paragraph demonstrates why autobiographical situatedness and reflexivity is such an important preparatory exercise in the pre-field stage: it preps you on your own categories and so increases the possibility of staying aware of exactly how you single out this rather than that phenomenon for data production out of the never-ending flow of phenomena that surrounds you while in the field. This is how the work on autobiographical situatedness comes into play in the field and so becomes a part of field situatedness. Becoming aware of the self and of what happens with you when you relate to the other(s) in the field may be highly analytically rewarding. Being aware of how I (Cecilie) categorize others (persons or texts) with regard to professional social work (or class, or gender, or ethnicity, or…) may open up possibilities for analysing power and care that I had not thought of before I entered the field. For me, this is an emotional path that follows from experiences of anxiety, uneasiness, or restlessness, that often result in productive analytical questions (see also Dauphinee 2013).

This way of making a part of one's autobiographical experience relevant in the analyses of the field is, as should now be clear, a variant of endless possible ways, and in Chap. 6 we turn to more principled and genre specific challenges in text situating. Even if the researcher subject, the I, is relatively clearly positioned in the example above—both as an analyst as well as a mother and a person who reacts and reflects on the interaction between the social workers and the youths—I, Cecilie, did not include the intense emotions that were triggered in me in the final text, even if they were important in two significant ways. First, they let me stay close to the field (the emotions were so present that I had to deal with them). Second, the questions I started to ask myself because of them (what is the difference between care given in private and professional contexts, and why are these differences important) gave an entrance to the analysis of the empirical data. However, I did not fully include the reflections on how I developed my analytical perspectives in the final text. I feared that my employers would judge me as unprofessional if I revealed in the final text that my analysis were partly based on my emotional reactions and autobiographical work on motherhood, even though I believed—and believe—my scientific reasons to highlight the reactions of the "researcher subject" and to specify how I filled the

subject position as researcher were relevant and interesting, thinking here in particular of Harding's concept of "strong objectivity".

Was it a question of courage, or the lack of it on my part? Yes, and it certainly sometimes takes courage to include stuff about how data were produced. One famous non-example concerns Pierre Bourdieu's doctoral thesis, *The Bachelors' Ball* (written in the early 1960s, published 2008). Pierre needed many decades to pluck up the courage to tell the reader that the whole project had been about the prodigal intellectual son's reintegration in his rural Bearn and how his main informant had been his mother. According to positivist research ideals, these were big no-noes, and for all his seeming barricade-storming, young Pierre did not find it in him to come clean. And that was only about himself and his career. In my case, the question of other people was in the balance. Young people who had a bad start found this institution to be a partial redress. That had to count for something, and so I left out certain data and wrote certain other sequences up like this rather than that in order to give them a break. To give them that break was certainly a break with the ideal that researchers shall account for as much of what she observes as possible. Another researcher might have made a different call. This aspect of textual situatedness is particularly tricky because it concerns non-events, namely the leaving out of stuff from texts. The text itself does not bear witness to what went on, only the subsequent extra-textual intervention of author, interlocutors or third parties may bring them to light.

Many of the fundamental questions concerning child protection work, and in particular where children have been removed, fully or partially, from the care and custody of their parents, challenge one of the most central values of Western societies, i.e. that parents and children belong together. Even as an observer, as I was in the example above, it can be painful and emotionally challenging to witness how young people who have experienced different kinds of abuse and neglect have little self-confidence, struggle to trust other people, display that they do not know what is expected of them in different social situations. It is an emotional challenge on many levels that may trigger the researcher's own experiences of abuse and neglect, or her fears that something could happen to her own children. The pain I experienced tempted me to distance myself from the field. As my job was to evaluate whether the "institution" were successful in creating positive changes in the youths or not, I realized

that in order to do this, I had to *adopt a gaze* that made me capable of seeing the youth, and to recognize the work that was being done by the youth as well as by the social workers on an everyday basis in everyday social interactions in the institution, to be able to discern possible changes.

Many researchers who have done fieldwork have articulated how boring it can be to observe interaction for hours, when you are not a direct part of that interaction yourself (see, e.g., Patton 1990; Album 1996; Fangen 2004; Erickson 2011, p. 50). When the observations are directed towards situations that resemble ordinary family life, it is easy to become analytically inattentive. People eat, shower, watch TV, read magazines, talk, do their homework, go for a walk, do the dishes, drink coffee, and so on; small seemingly aimless undertakings that most people do in some form or another when coming home from work or school. Knowing this, I consciously had to activate my own frames of family references and experiences, my classed and gendered expectations and preconceptions, so that I could be able to see—and be attentive of—what was taking place in the institution, understanding that what I observed of social interaction that resembled things we all do in everyday life, had implications far beyond what I usually attribute to my own family life. This is a trivial but important Goffmaninan point, still, it was painful to have dinner with seven adolescents knowing and seeing that none of them had the experience of eating dinner regularly, observing how they practiced simple things like passing the spaghetti bowl (with careful support from the social workers), asking each other questions about how the day had been, learning to wait for the response before a new question was asked. If I had not acknowledged this, I would not have been able to recognize the progress they made.

The idea behind this chapter was to give three empirical examples of how situatedness is a part of, often a central part of, the research process. We will return to the questions and challenges involved in situating oneself and give more examples of the three main forms of situating the research. We will also discuss other dimensions and challenges the researcher may encounter in the research process. But before we do that, in order to provide the reader with a conceptual platform, we will first present parts of the historical and philosophical background to the Gestalt tradition.

Notes

1. Personal communication to one of the authors from Harald Olav Skar.
2. In the UK, the health visiting services are located at the General Practitioners' offices. In Norway, the services of the health visitors are offered to all children and their parents in specially assigned public clinics. Here, the children's health and growth are medically monitored, parents receive information and advice on everything concerning their child's physical, cognitive and emotional development, and the children are offered vaccinations.
3. We write "to some extent", for, as Lakoff and Johnson argue on the strength of neuroscientific data, most of our own thinking and feeling processes seem to remain unavailable for reflection (see also Damasio 1994).
4. That I discuss institutional characteristics may seem paradoxical, since this institution is an initiative based on youth staying at home as much as possible. The institutional dimensions are activated due to the fact that about 10 youths between the age of 12–16 spend three afternoons and three evenings a week in a house, and also go out for weekend trips once a month.

References

Album, Dag. 1996. *Nære Fremmede*. Oslo: Tano.

Bourdieu, Pierre. 2008. *The Bacherlors' Ball. The Crisis of Peasant Society in Béarn*. Doctoral Thesis Written in the Early 1960s, Chicago, IL: University of Chicago Press.

Damasio, Antonio R. 1994. *Descartes' Error. Emotion, Reason, and the Human Brain*. New York: Avon Books.

Damsa, Dorina, and Thomas Ugelvik. 2017. A Difference That Makes a Difference? Reflexivity and Researcher Effects in an All-Foreign Prison. *International Journal of Qualitative Methods* 16: 1–10.

Dauphinee, Elizabeth. 2013. *The Politics of Exile*. London: Routledge.

Dottolo, Andrea L., and Sarah M. Tillery. 2015. Reflexivity and Feminist Interventions. In *Reflexivity and International Relations. Positionality, Critique and Practice*, ed. Jack L. Amoureux and Brent J. Seele. London: Routledge.

Erickson, Frederic. 2011. A History of Qualitative Inquiry in Social and Educational Research. In *The SAGE Handbook of Qualitative Research*, ed. Norman Denzin and Yvonna Lincoln. Los Angeles: Sage.

Haraway, Donna. 1991. *Simians, Cyborgs and Women. The Reinvention of Nature*. London: Free Assosiation Books.

Harding, Sandra. 1991. *Whose Science? Whose Knowledge? Thinking From Women's Lives*. Ithaca, NY: Cornell University Press.

Harding, Sandra. 2015. *Objectivity and Diversity. Another Logic of Scientific Research*. Chicago, IL: University of Chicago Press.

Hochschild, Arlie Russel. 1983. *The Managed Heart. Commercialization of Human Feeling*. Berkeley, CA: University of California Press.

Jackson, Patrick. 2011. The Conduct of Enquiry in International Relations: Philosophy of Science and its Implications for the Study of World Politics. London: Routledge.

Knorr Cetina, Karin. 1999. Epistemic Cultures: How the Sciences Make Knowledge. Harvard: Harvard University Press.

Leira, Arnlaug. 1992. *Welfare States and Working Mothers*. Cambridge: Cambridge University Press.

Løgstrup, Knud. 1997. *The Ethical Demand*. Notre Dame: University of Notre Dame Pess.

Neumann, Iver B. 2007. "A Speech that the entire Ministry may Stand For," or: Why Diplomats never produce anything new. *International Political Sociology* 1 (2): 183–200.

Neumann, Cecilie Basberg. 2009. *Det Bekymrede Blikket* (The Worried Gaze). Oslo: Novus.

Neumann, Cecilie Basberg. 2010. LINK – *en evaluering av et barnevernstiltak*. Oslo: AFI- notat nr. 1.

Patton, Michael Quinn. 1990. *Qualitative Evaluation and Research Methods*. Newbury Park: Sage.

Simon, Herbert. 1997 [1947]. *Administrative Behavior: A Study of Decision-Making Processes in Administrative Organizations*, 4th ed. Glencoe: Free Press.

Sjoberg, Laura. 2014. Gender/Violence in a Gendered/Violent World. *Millennium: Journal of International Studies* 42 (2): 532–542.

Skeggs, Beverley. 1997. *Formations of Class and Gender*. London: Sage.

Solheim, Jorunn. 2007. *Kjønn og modernitet* (Gender and Modernity). Oslo: Pax.

Wærness, Kari. 1992. On the Rationality of Caring. In *Women and the State*, ed. Anne Showstack Sasson. London: Routledge.

CHAPTER 3

A Century of Thinking About Situatedness: The Gestalt Tradition

Abstract If we want to master a conceptual apparatus, we need a certain overview of its history. This is important because terms and concepts mean different things within different traditions. Unless we situate the term field, a social scientist could be led to believe that we mean field in the Bourdieuian sense, i.e. a bit of a social reality tied together by a common focus. We do not; we mean the immediate relations within which the researcher does her work. The development of a conceptual terminology often reveals interesting connections with other disciplines and knowledge traditions. By having knowledge of this, one may better understand ambiguities and tensions embedded in concepts that one would otherwise have overlooked.

Keywords Gestalt history · Gestalt psychologists · Perception Phenomenology

Much in the same way as the therapist acquires a fuller understanding of the patient's field as traumas emerges on a background of his social and psychological history, a knowledge tradition is relayed to the student when he, among others things, is taught its history. Being aware of one's own intellectual genealogy is an important aspect of autobiographical situating. Here is one important strand of the intellectual genealogy that enabled us to write this book.[1]

C.B. Neumann and I.B. Neumann, *Power, Culture and Situated Research Methodology*, DOI 10.1007/978-3-319-59217-6_3

We start by noting that the concept Gestalt was first used by Goethe and in art theory. From the middle of the nineteenth century, at certain German universities experimental psychology grew out of philosophy, and it was within the debates in this field that the term Gestalt developed towards the meaning we attach to it today. We briefly mention a few of the predecessors of the Gestalt psychology, before we go on presenting the development of an independent Gestalt psychology from 1910 onwards.

Preconditions for Action

In German, *zu gestalten* means "to form", and the resulting noun Gestalt means the essence or shape of a complete form. It remains an everyday word in German, as it does in our native Norwegian. The man who first systematized the term was Johann Wolfgang Goethe. To Goethe, "Gestalt" meant the organism's own formation as a whole. The point of using the term rested with the latter part, the whole. The organism, be it a plant or a human, cannot be fully grasped through its individual parts, but only through its whole.

In the 1800s, the study of the human mind—psychology—was not an independent discipline. It was a part of philosophy, and of interest also to physiology, another dominating contemporary science. Experimental psychology, aiming at understanding how humans experience the world under different conditions, emerged in the middle of the nineteenth century. Typically, such experiments singled out a specific sense, such as the eyesight, then specified a specific sense object, such as a colour chart, and tried to pin down what the human eye could and could not perceive. It revolved primarily around specific physiological conditions, and what the human senses could and could not do, given the physical apparatus we are set up with.

Where people approach a field of knowledge from two different perspectives, they will bring with them different prior understandings and expectations, and subsequently approach the specific field of knowledge differently. This was the situation in early psychology, and particularly in its epicentre, which was the German-speaking world. The leading driving force from the perspective of physiology was Wilhelm Wundt. It is primarily Wundt who managed to establish experimental psychology as an independent field in German academia. From philosophy, Carl Stumpf was the key protagonist, with Christian von Ehrenfels as another

important name. One of philosophy's central claims against physiology concerned its infatuation with bits and pieces of the human mind and human senses, or perceptions, to use the psychological term. Philosophy is concerned with wholeness, and philosophically inclined psychologists were concerned with the wholeness of the human mind and perception. It was partly because of the philosophical critique from physiologists such as Wundt that the term Gestalt emerged in psychology.

The main foundational work of Gestalt psychology is an essay by Christian von Ehrenfels from 1890, "Über Gestaltqualitäten", about Gestalt qualities.[2] Its main point was that a phenomenon, for example, a musical composition, is more than the sum of its different parts. Even if two musical compositions do not share a single note, it will still be easy to recognize them as the same melody (they may, for example, be set in two different keys or tonalities). That which is unique to the whole—unique in the sense that it is more than the sum of its parts, is what von Ehrenfels called the Gestalt quality. That von Ehernfels chose music as an example is probably also due to the fact that the gestalt term, since Goethe, had been appropriated by aesthetical theory (cf. also Zinker 1988). When one field of knowledge adopts a term from another field of knowledge, it comes as no surprise that the examples chosen are examples that reflect their prior use in a different knowledge field.

Parallel to these developments, Stumpf worked on similar issues, and it was Stumpf who constructed the philosophical foundation for an experimental psychology concerned with the whole. The three central names in early Gestalt thinking, Max Wertheimer, Wolfgang Köhler and Kurt Koffka, were all Stumpf's students, and they were all influenced by a philosophical tradition that developed during their student years, namely phenomenology.

In 1912, Wertheimer organized experiments with a view to understanding how humans perceive movement. He used a now outdated technological devise to run the experiment, and so instead of explaining the technology he used, we will explain the finding by visualizing a row of light bulbs. The light bulbs can be lit and then turned off, one by one, so that the first bulb in the row lights up first, a moment after the second comes on, then the third, and so on. If the speed between on and off is not too quick and not too slow, humans will perceive the series of changing points of lights as movement, even though the light bulbs stay still. Imagine a discotheque or a neon sign, where a light fan or a sentence

flash up, often from left to right, which is our culturally specific way of organizing the movement of reading.

Wertheimer made an important discovery. His finding was that humans not only perceive movement where an object actually moves, but that they also "see" movement where the objects they see do not move. In the age of cartoons and animations this may be obvious, we experience it as a film—i.e. something that moves—even if we know that what we see is in fact a series of drawings (objects) shown one after the other at a given speed. Technically we are witnessing stills arranged in a certain order and displayed at a certain speed, but what we see is movement, and what we experience is a whole, i.e. the film.

Wertheimer's experiment formed the basis for all subsequent Gestalt thinking. To the Gestalt psychologists, the object or the thing appears or presents itself to us, without us needing to think about which parts of our senses and our perceptions are involved. The same applies to understanding phenomena in general. We do not build our understanding of a phenomenon the way we build a wall of bricks; we perceive the phenomenon immediately. The brains's ability to understand a Gestalt or a whole comes before a possible breaking down into specific parts. In other words, the brain has an ability to self-organise, which in turn is the basis for the human ability to construct meaning. If we, for example, see a drawing of three sides of a square, we will automatically add the fourth side so that we see a complete square.

Koffka wrote his doctoral thesis on rhythm, a musical theme that von Ehrenfels had used in his essay from 1890. He had poor colour vision, and built his career as a scientist by exploring this weakness. (Here we have an excellent example of how autobiographic situating may be turned to scientific advantage). The ability to see colours had also been a topic to von Ehrenfels, so Koffka stepped onto safe ground. Koffka's contribution was to extend the area of validity for Gestalt insights and findings. Gestalt turned out to be at the core not only of how Koffka understood melody and rhythm, but also other creative activities such as drawing or writing. He was primarily concerned with showing how the brain's ability to organize wholes works. This was courageous, in particular when considering that contemporary brain research was unable to support him; it had not maps of which functions that were performed in what parts of the brain.

Köhler moved Gestalt thinking from the study of single-object perception to the study of behaviour. Behaviour is what living creatures

do. One sub-group—perhaps not very large—of these things is strongly informed by thinking, and is therefore called action. For the social scientist, Köhler is interesting not least because he moved Gestalt thinking out of the laboratory and into less tightly edited interactions. This brought Gestalt thinking one step closer to the core subject matter of the social scientist, which is interaction. Köhler's own arena was the monkey cage. Around the time of the First World War, he ran the world's first test station for research on hominidae apes in Tenerife. The topic was learning and the most important finding for us was that learning often happens instantaneously, as epiphanies or aha moments. For example, Köhler placed several objects inside the cage that could be useful tools if combined correctly. These could be crates that had to be stacked on top of one other to reach a banana dangling from the ceiling, or sticks that needed assembling so that the banana outside the bars could be reached. The apes became incredibly frustrated by these exercises, and reacted by roaming around the cage and destroying the objects—their potential tools—against the walls and bars. The point was, however, that when learning took place, it happened in a flash. All of a sudden things added up, and new possibilities for action appeared. The most illustrative example Köhler gives was his trying to make the apes learn to go around a glass wall in order to reach the food on the other side. After a whole lot of bluster, the most gifted ape suddenly understood how it worked, and went off around the glass wall and accessed the bowl of food. Köhler then placed his 2-year-old daughter in the cage. After a short while the trick dawned on her, and she was around the glass wall and into the trough in one fell swoop. (Köhler's dog was rather less fortunate. He could smell the food through the glass wall, with the result that he was glued to the glass wall, paralyzed by his own acute sense of smell). The point is the sudden insights of apes and children, and the seamless movement in which understanding resulted. Both these phenomena—the learning and action—are characterized by their irreducible wholeness. The different parts become a whole, and the whole is more than the sum of the different parts. One may attempt to break down the smooth movement around the glass wall into different motoric factors—one leg in front of the other, the torso leaning forward, the hand around the wall, etc.—but this would simply miss the point, which is that the resulting action must be understood as a whole greater than the sum of its parts. It may, in other words, be understood as a Gestalt. Köhler, therefore, posited that he could prove that not only perceptions

but also learning and action were characterized by Gestalts, and that the principles of forming Gestalts applied not only to humans but to at least some other creatures, such as chimpanzees.

This may seem far removed from research practice, and rightly so. Still, these experiments inspired their instigators to formulate the basic ideas on which the Gestalt therapeutic practice rests, which we believe may also inspire the research practices of social scientists, via the insights of phenomenology. This is often characteristic of intellectual work; you fumble around in the abstract, trying to make order out of chaos. The effort lies in trying to understand an abstract field as precisely as possible in order to talk about it with some clarity. Then again, even if you have succeeded in talking about the abstract with precision, it is not given that it speaks to others, and can be put into practice. But in this example the theoretical projects on light and apes bore tangible fruits, the Gestalt tradition. If we turn to one of the original texts in this tradition, such as Wertheimer's essay from 1912, it may not offer much, since for our purposes we are dependent on later insights generated by other people. One may do well to remember, in the moments when the research process seems to stall and we may be on the brink of giving up, that fundamental insights have more often than not been preceded by a frustrating period of confusion and sometimes pain. There is no reason to be ashamed of the trying periods of the research process.

All this is deep background for our work on situatedness. History of science stuff is not everybody's cup of tea. In all walks of life there are great practitioners that cannot explain what they do, or why they are good at it. They may be theoretically weak, but they are still dependent on other people's knowledge in order to do what they do, be that building bridges or stitching together research stories. One way to think about this is that what they have learnt has happened through osmosis. There is no absolute need to know about the Gestalt tradition in order to be good at situating oneself, and yet it makes for nice sensitizing training.

Gestalt insights demonstrate that the way humans seek and construct meaning is not only socially and relationally conditioned, but is also supported by the way the brain organizes information, and how it creates meaningful wholes. Two Soviet researchers developed Gestalt psychology in a direction particularly fruitful for social scientists. The first was Bluma Vulfovna Zeygarnik. She ran laboratory tests where she allocated assignments to the test persons, only to interrupt them before they were

finished. She was especially interested in understanding how human memory worked, and could establish that her test persons remembered interrupted assignments better than completed ones. Maria Ovshankina was inspired by Zeygarnik's experiments. Where Zeygarnik had left her research subjects with the memory of the interrupted action, Ovshankina was concerned with what happened afterwords. She was able to prove that the test persons were often spontaneously inclined to pick up the assignments given during the experiments, even in those instances where there was no other reason to do this than to complete what they had started. The interrupted and unfinished actions stayed with them, longing to be completed. This speaks to all researchers! It is a necessary and inevitable part of a research process to always go at it, get interrupted, then start again, discard sidetracks that cannot be made into a meaningful analytical whole and start again. It is perhaps good to know then, that besides social conditions—the encouragement of the supervisor, the restrictions laid down by the grant authorities, the expectations of parents, the teasing by friends—there is also our own psychological composition that drives us in the direction of finalizing research projects.

Gestalt, Phenomenology, Social Sciences

Social scientific methods and the Gestalt tradition share a common philosophical lineage with, and subsequent anchoring in, phenomenology, as well as with the related philosophical direction existentialism. Hanne Hostrup (2004) identifies four levels on which a Gestalt therapist builds her therapeutic practice. These are levels, she argues, that the therapist not only has to be conscious of but also has to have integrated as a part of her awareness qua Gestalt practitioner. The first level, the meta-level, is existentialism. The second level is psychological theory, i.e. Gestalt psychology. The third level, the methodology, draws on phenomenology, and the fourth, the techniques, is the gestalt traditions' own way of posing questions, meeting the client, etc. in other words style and approach (Hostrup 2004, pp. 30–36). Translated to social scientific terms, these levels are equivalents to ontology (the study of the nature of being, and of what the world consists of), epistemology (the study of knowledge and of how we know), methodology (how we link our knowledge-production schemes to worldly concerns), and methods or data gathering techniques (how to produce data).

The Gestalt tradition insists that these levels are connected in internal ways. Fritz Perls, the founder of Gestalt therapy, was preoccupied with this holism (see Perls 1948; Perls et al. 1973). Anyone trying to think and reflect upon a methodological issue will soon discover how difficult it is to keep the different levels apart. What do I actually learn (of what do I gain knowledge) when I ask Anne how her day at work was? A range of factors influences the answer I receive, and how I interpret or understand that answer. Which words do I use to formulate the question, and how do I appear to Anne? What do I expect her to reply, and what does she believe that I expect her to reply? Which preconceived ideas and knowledge do I have of Anne and the work she does, and how does this affect the way I pose the question? Even the simplest of questions may encompass an infinite number of assumptions about the other person, about myself, about my being in the world, how I think that humans and social relations are tied together, of approaches to discover these relations, and so on. At the same time, many researchers have experienced with regard to one of the defining constituent of the social sciences—understanding social structures and individual action—that the structural conditions humans in different positions are subjected to, creators of, and maintainers of, also contribute to the fact that the employees in a given institution may have surprisingly similar experiences and understandings of these same structures.[3]

Although therapists and researchers acknowledge that the levels are interconnected in complicated and internal ways, it is critical to reflect on them as individual levels, separately. Converted to research practice, this equals how we always have to work with human as a whole—and always in interaction with others—while we are also after specific research topics which make it necessary to single stuff out. The conversation about the difficult needs a focus. To talk always implies talking about something specific. This applies to all other activities too. A central point in phenomenology, with roots back to David Hume, is that thinking always means thinking *about* something. Something is foregrounded, necessarily at the expense of other stuff.

Another thing that follows from this is that a small adjustment of thoughts on the foundation of human perception may have unknown consequences. When von Ehrenfels decided to move a few colour charts behind some holes, it turned out to lead to a whole new theory of how humans perceive the world. We will give another example of this, from the same time period and the same environment, directly relevant to

Gestalt thinking. Philosopher René Descartes posited in the seventeenth century; "I think, therefore I am". This idea, that man is a thinking animal, ultimately relying only on his own thought, became the basis for not only much subsequent philosophical thinking, but also for everyday thinking. Many people started thinking of what a self "is" in this way. Two hundred years later, one of Carl Stumpf's students of philosophy, Edmund Husserl, posited that thinking must always be thinking about something.[4] It is not like humans are separated from the world, sitting with their chin resting in their right hand, thinking about nothing in particular. The world is always there, as something we think about. And to Husserl this meant that humans and the world are intertwined in direct and inevitable ways, also when we are seemingly "alone". We think about phenomena; objects, events, types of events, animals and, fundamentally, other humans. Thoughts reach out to the other and the others in the world. This means that humans are not alone with their thoughts and their doubts. We are directly and inevitably connected with the world, and with each other. And this means nothing less than that what a self is, after Husserl, appears as something else than what a self was, before Husserl. Husserl invented a whole system to describe how humans think about the phenomena the world consists of and called it phenomenology. While Gestalt psychologists concentrated on what humans perceive, Husserl and his predecessors—the phenomenologists—focused on a broader issue; what this ability to sense entails, and what it is to be a human in the world. The two groups of thinkers were directly influenced by each other. Husserl dedicated his magnum opus to his teacher Stumpf, and Wertheimer, Koffka and Köhler repeatedly referenced to Husserl in their work. This continued beyond the first generation, and is also evident in the work of subsequent phenomenologists, such as French philosopher Maurice Merleau-Ponty, who used the 56-page introductory chapter of his main work, *Phenomenology of Perception*, to discuss Köhler and other Gestalt psychologists. We may conclude that the two traditions, Gestalt psychology and phenomenology, were inherently intertwined from the moment of conception, and remain so. Phenomenology is the philosophical basis for Gestalt psychology. We will now present some basic phenomenological concepts. They will further add to our exploration on the fundamental problem of to what extent the researcher can sense the same things as the person he is talking to.

Sides, Aspects, Profiles

When social scientists talk about humans with the capacity to act and interact, they call them subjects (to separate them from other humans and animals without this capacity). Sides and aspects are concepts intended to capture that which is intersubjective—something that may require certain negotiations in order to display identical properties, but which in principle is evident to all who see, say, the cube. When phenomenologists explain (see Sokolowski 2000) how human beings sense, or perceive, and what causes us to see the same thing, but differently, they use the concepts of sides, aspects and profiles. Two people can view the same cube from different positions and agree that it is a cube, even if they see different sides of the cube. If you and I are diametrically opposed across a table and you see the four side of a die while I see the two side, it is not hard to agree that what we have in front of us is a die. So it is with elephants; you see the trunk, I see the tail, but a debate about what kind of animal we see is unlikely to ensue. So far, so good; sides are simple.

Aspects are also fairly simple. Here we see the same side, but from different perspectives. I see the trunk from the left, you see it from the right. The elephant has just picked up a peanut and has swung the trunk to the left, where I can see it, but you only see the trunk without the prehensile part, and without the peanut. It is not hard to swap information and agree that the prehensile part and the peanut are indeed there.

If we try to eliminate the different spatial perspectives and place two people in the same position, however, the elephant perceived may not be the same, for there will be a temporal difference between the perceptions of the two different people. Alternatively, our autobiographical situatedness may make us see different things. In addition to sides and aspects, there is a subjective component of perception, one that is a given only for some subject and that cannot readily be shared with others. This is what the phenomenologists call profile.

Profile denotes the individual's unique view on the aspect of the side that he or she initially shares with no one else. The concept profile is not primarily related to space, like the concepts side and aspect, but to time. Let's take illumination as an example. If I see a cube in bright morning light at nine in the morning, while you see it in the twilight of dusk, the cube will look different, and we need to enter into more extensive negotiations in order to establish that we saw the same cube. We can

negotiate what we have seen, a cube, but we cannot necessarily communicate or negotiate our subjective perception of the cube—here it is the difference in light due to time that makes us assume that the different subjects will experience the cube differently—thus the light is shaping or giving the cube its profile—that cannot be shared. At 9 o'clock in the morning, the elephant's trunk may be freshly out of clean water, whereas 2 h later, it may have a fine film of mud. If we move to autobiography, an elephant-keeper may see some stuff on the trunk that needs medical attention, but that stuff will not be visible to someone who does not have special knowledge about elephants. It is the same with a golf ball. The ball may have tiny scratches or cracks after a hard hit or a tough landing. There may be some earth, grass, sand or cow dung stuck to it. The golf ball will thus appear with a different profile. This has significance to a golf player, since it means that the ball will behave subtly different; it will move differently in the air, and it will definitely roll differently. But a subject who does not play golf is not likely to see or pay attention to the golf ball's different profiles, because he has never thought about how these things matters. Since they do not look for it, they do not see it, as we say colloquially. This implies that profiles are fundamental to perception, and that perceptions cannot be solely intersubjective. There may still be many elements of intersubjectivity (two non-golf players may see the same sides and aspects of the ball, and not notice its different profiles), but there will also be a subjective component to it (golf players will see different profiles, and look for different profiles in different situations). If I attempt a transport hit, where what is glued to the ball is important but not all-important, I may not even see it. If I'm about to put, however, I will see the added stuff because I look for it. Hence, profile (the subjective view) is present even when we discuss material objects, not only perception of situations, memories, etc. The subjective aspect of perception is present even when fully capable subjects see tangible and initially simple things, like a cube or a golf ball. Note that the problem is there not only between two different observers (intersubjectively), but may also crop up because the same subject is in two different states of mind (intrasubjectively).

Inspired by phenomenology and hermeneutics, Norwegian sociologist Dag Østerberg (1993) notes how the meaning of action is given its exact meaning only after its completion, and how the same action may mean a number of different things; if someone lights a cigarette, it may be because he is happy, needs nicotine, is nervous, is bored, and the

list goes on. This point may apply also to aggregated levels. One of the great twenieth-century historians of science, Thomas Kuhn, makes the same point in a more all-encompassing and intricate, and therefore more nuanced, way when he attempts to explain how people may live in different worlds, or experience the world in different ways:

> If two people stand at the same place and gaze in the same direction, we must, under pain of solipsism, conclude that they receive closely similar stimuli. (If both could put their eyes at the same place, the stimuli would be identical.) But people do not see stimuli; our knowledge of them is highly theoretical and abstract. Instead they have sensations, and we are under no compulsion to suppose that the sensations of our two viewers are the same. […] On the contrary, neural processing takes place between the receipt of a stimulus and the awareness of a sensation. Among the few things that we know about it with assurance are: that very different stimuli can produce the same sensations; that the same stimulus can produce very different sensations; and, finally, that the route from stimulus to sensation is in part conditioned by education. (Kuhn 2002, p. 180)

Kuhn is not so much concerned with how the researcher and her interlocutors see things differently. Instead, he is concerned with how perception is taught within specific social contexts, and how those persons sharing this context perceive or see the same thing. The example above of people socialized into the world of golf, illustrates this. Kuhn is interested in how science develops. He begins with observing how a scientific discipline tends to see the same things and the same problems throughout. When scientists craft articles, they try to solve the same problems in approximately similar ways, and they criticize the same premises and effects of this and that in roughly the same manner. But at times, Kuhn argues, problems of a certain magnitude will present themselves that cannot be solved through available premises. If these problems grow sufficiently urgent, scientists will start to question their own premises. They will start to wonder if they grasp and represent stuff in ways that are adequate for the task at hand. At a given moment the situation may become revolutionary: some scientists will advocate for new premises to define the discipline. Old colleagues can no longer continue like before, as their version of reality will be invalidated (and the things they write about it will no longer gain support or generate the attention it used to). The difference in what is perceived and how it is perceived will lead to

a condition of scientific civil war. When Kuhn (2002) discusses what a scientific revolution does to the scholarly community (which shares a scientific set of premises that he calls a paradigm), his point is that they see the world differently through the new paradigm. They view the phenomena differently than they used to.

Here, Kuhn is thinking explicitly of and with Gestalt psychology and their experiments on perception.[5] As he is concerned with explaining the changing paradigm, he is not content with an explanation that suggests that a revolution occurs based on competing interpretations of the shared empirical field. Something instant, like a strike of lightening or a sudden revelation, that is, a sudden intuition forms the basis for great discoveries, and thereby paradigmatic changes. Note the similarity between this point and Köhler's insights into the Gestalt quality of learning. After hitting against the same problems and challenges for years, they all of a suddenly appear in a new light, or should we say with a new profile. Kuhn gives different examples, but for the purpose of this book it is primarily interesting that he uses Gestalt thinking itself as an example of a paradigmatic change in psychology. Kuhn sees Gestalt thinking as a paradigmatic shift compared to the earlier theorizing of perceptions, as it appears from Descartes and Hume until the older contemporaries of Wertheimer, Koffka and Köhler. In order to strengthen this case Kuhn draws on philosopher Michael Polanyi's understanding of what he calls silent or tacit knowledge, i.e. that there is always an unarticulated rest in a field of knowledge that is either not yet articulated or is not possible to articulate.[6] Modes of thinking from past paradigms may stick with us; in the case of the early Gestalt psychologists it is likely that they did not immediately realize the consequences of their own insights, and how they necessarily had to correct a number of their own earlier premises. Rapid paradigmatic changes can create new and surprising ways of organizing a field, which in turn generates further changes in thinking and doing. Attempting to break down the nature of such a change into its different individual parts or components, such as attempting to identify every rule involved, is hardly possible. Thus, there will always be an unexplained rest.

Within psychology there was a split between those who adopted and developed Gestalt insights further and those who retained the old ways of thinking. Psychologists dealing with perceptions in the pre-Gestalt psychological way are still around. They are typically concerned with how specific stimuli correlate with specific changes in the brain's

function, and so on. They continue an experimental practice similar to the one psychologists conducted before Gestalt thinking made its appearance. They are, to use Kuhn's terms, pre-revolutionary. They refuse to evolve, and continue to revolve around the same old problems.

Having now placed our undertaking in its historical context and provided some phenomenological insights into observation that should come in handy when researchers think about situating themselves and what their situatedness does to their data production, we move on to discuss some concepts from the Gestalt tradition that should also prove eminently useful.

Notes

1. The other main strands are feminism, touched on in the former chapter, and poststructuralism, touched on throughout. This is a book that is primarily written for social scientists, who must be expected to know something about these traditions. We cannot expect the same where the Gestalt tradition is concerned, and so we have, pragmatist fashion, singled that genealogical origin out for special treatment.
2. Where no other source is given, this section builds on Ash (1998).
3. This, in turn, points to a basic problem that social scientists have grappled with for decades, namely the relation between stability and change when theorizing the relation between individual actors and the structures that precondition and are reproduced by action. We will not pursue the agent-structure problem here (but see Neumann 2002).
4. Descartes had insisted that we think, therefore we are, in Latin *cogito ergo sum*; Husserls added that there will also be something to think about, a *cogens*. Again, this does not happen from scratch; he was inspired by the insights of other philosophers, notably Hume.
5. See http://eis.bris.ac.uk/~plajb/research/papers/Kuhn_for_DLB.pdf, and http://plato.stanford.edu/entries/thomas-kuhn/#4.2: Kuhn likened the change in the phenomenal world to the Gestalt-switch that occurs when one sees the duck–rabbit diagram first as (representing) a duck then as (representing) a rabbit, although he himself acknowledged that he was not sure whether the Gestalt case was just an analogy or whether it illustrated some more general truth about the way the mind works that encompasses the scientific case too.
6. See Bertil Rolf's (1991) discussion on Polanyi, and also Thomas Kuhn (2002), p. 51, and note 1, Chapter V.

References

Ash, Mitchell G. 1998. *Gestalt Psychology in German Culture 1890–1967. Holism and the Quest for Objectivity.* Cambridge: Cambridge University Press.

Hostrup, Hanne. 2004. *Gestaltterapi Inoføringi Gestaltterapiens Grunobecreber.* Copenhagen: Hans Reitzels Forlag.

Kuhn, Thomas S. [1962] 2002. *The Structure of Scientific Revolutions.* Chicago, IL: University of Chicago Press.

Neumann, Iver B. 2002. Returning Practice To The Linguistic Turn: The Case of Diplomacy. *Millennium* 31(3): 627–651.

Østerberg, Dag. 1993. *Fortolkende sosiologi I.* Oslo: Universitetsforlaget.

Perls, Friedrich S. 1948. Theory and Technique of Personality Integration. *American Journal of Psychotherapy* 2: 265–286.

Perls, Fritz S., Ralph F. Hefferline, and Paul Goodman. [1951] 1973. *Gestalt Therapy: Excitement and Growth in the Human Personality.* London: Penguin.

Rolf, Bertil. 1991. *Profession, tradition och tyst kunskap En studie i Michael Polanyis teori om den professionella kunskapens tysta dimension.* Nora: Bokförlaget Nya Doxa.

Sokolowski, Robert. 2000. *Introduction to Phenomenology.* Cambridge: Cambridge University Press.

Zinker, Joseph. 1988. *Creative Process in Gestalt Therapy.* New York: Random House.

CHAPTER 4

Conceptual Inspiration from the Gestalt Tradition

Abstract For students and researchers who are making an effort to situate themselves, an understanding of the basic concepts defining the Gestalt tradition may prove useful. In this chapter, we present and discuss the concepts of Field and Gestalt, figure and ground. In addition to clarifying the concepts, we hope to demonstrate how these Gestalt concepts may fit with a constructivist understanding of research practice.

Keywords Field · Gestalt · Figure · Ground

Field and Gestalt

The name of a scientific discipline often reflects its foundational concept. The basic concept of the Gestalt tradition is seemingly Gestalt. And yet, it is probably "field" that is the foundational concept of the Gestalt tradition. A field is the whole range of social contexts that forms backgrounds for actions; that releases or triggers specific memories in social interaction, that forms patterns that will gestalt themselves and become figures to the (attentive) interlocutor. A field is a part of the dynamic internal structure of the individual and makes up our sense of self. A field is also something that is created between you and me, or between me and the external world in the here and now (Brownell 2010). Hostrup (2004, p. 84) emphasizes that the understanding of the field should be included

C.B. Neumann and I.B. Neumann, *Power, Culture and Situated Research Methodology*, DOI 10.1007/978-3-319-59217-6_4

in all situations and all relations; humans are fundamentally relational and cannot be imagined without a field.

Two clarifications are needed, though. First, the field we discuss here is not the same as the "field" we referred to in the previous chapter outlining field situating. Field situating is done and goes on in the field, with the field simply being the chunk of social life amidst which the researcher works. What we are talking about here is one aspect of that field, not the physical aspect, but the aspect that is to do with social facts; how people represent phenomena, including their relationships. Second, field as we understand it within the context of the Gestalt tradition is different from how it is usually referred to in the social sciences, namely in the Bourdieuian sense, where field is referring to an analytically refined set of interactions (such as the economic field) that is different from other analytically refined fields (such as the political and religious fields). Where Bourdieu's term field is socially located and refers to the researcher's delineation of a large macro field constituted by several people by virtue of what they have done and do together, the Gestalt tradition's understanding of a field is socio-psychological and tied to subject; it refers to an individual's mental landscape or to the psycho-social relational landscape between two or more individuals. Where the first postulates a way of understanding social organization analytically, at the aggregated level, the latter is postulated as an ontological process that shapes and reshapes the relational individual, as well as being understood as something that is created between two or more individuals. The Gestalt field is a hermeneutical field; it is there for the researcher to understand, and not for her to postulate. In this way the Gestalt understanding of field enables articulation of the shifting relations between the two layers, internally and between the individual and her social surroundings (Asking "what happens in the field between us now?" enables this articulation. By asking you this, I invite you to articulate your experience of how you relate to me in the here and now as well as how you experience yourself when interacting with me). We will not address Bourdieu's use of field further in this book, but only mention it to avoid conceptual confusion.

The Gestalt tradition's field concept is not a very precise concept. It is simply a concept used to cover the entirety of the subject and her interlocutor's boundaries towards the social world. Since human thinking always implies thinking about something specific, it is impossible to imagine a person without imagining a field. The researcher's point of departure is that a field is unique to each individual person, since the

experiences of two people will never be identical. It follows that their fields cannot be identical, either.

To elaborate, the field refers to the individual's experiences of their specificity within the social, and is thus activating the phenomenological distinction between subjectivity and inter-subjectivity that we discussed in Chap. 3. While the Gestalt tradition will facilitate subjects in exploring their individual particularity and experiences with reference to the psycho-social (their subjectivity), the social scientist will search out individual meaning related to conditions for actions, and is thus exploring inter-subjectivity from the subject's point of departure. The social researcher's project is not primarily to get an in-depth understanding of the individual informant, but to understand the social organization and political institutions through individuals shared and/or contested experiences related to different positions and discourses within these organizations.[1] Understanding field in this way may help the researcher to situate herself, but also to become more aware of and sensitive towards her informants and what goes on in the field between her and the informant.

Thus, it is crucial to remember that the researcher herself will become a part of the interlocutor's field, since she; through her presence, body, clothes, tone of voice, etc. may trigger reactions and memories in the other in the same way as when this person is recognizing an object, or is reminded of a memory or an interaction with another person. Moreover, in the interaction between the researcher and the interlocutor, both persons' fields will be modified, and a "new" field will be created. Intersubjectively speaking, a field is the sum of the shared experiences of two or more people. It is a premise for all research practice that the researcher and her interlocutor develop a new field; no interaction, no data. In the following, we will describe and discuss the relation between researcher and informant from a Gestalt perspective.

One of the researcher's tasks during an interview is to explore the different figures that may arise in the field. What the researcher is particularly interested in is how the field is formed or "gestalted"—how the informant specifies particular and meaningful experiences (so-called figures), in which contexts the informant specifies her experiences (what becomes figures against which background) and how this is reflected in the informant's language and body language. Field and Gestalt figures are what a social scientist would call mutually constitutive.

In the Gestalt tradition, it is not only an ethical principle, but also a practical method, that the interlocutor's (often a client's) recognition and understanding should arise as a consequence of his becoming aware of the field that he shares with the interviewer. To the interviewer, it will always be a temptation to "help" her interlocutor analyze his experiences for her, in order to be done with the job. Here lies an important source of tension for any researcher: it is the researcher's task to lead the interlocutor onto topics that the researcher thinks important, even where the interlocutor is interested in talking about something else.

It is impossible to imagine that the researcher, who earns money or at least kudos from the work done and whose job it is to run the research processes, should not control the conversation in a larger degree than the interlocutor. Similarly, it is hard to imagine that the researcher, as well as the therapist, should not analyze what the client or interlocutor recounts, for a narrative is in itself an analysis.

The imperative for the interviewer, then, is to keep her own opinions and analyses back. This is the prohibition to interpret; it is of course not a prohibition to interpret at large—that is the researcher's job; neither is it a prohibition to interpret as a conversation is taking place—that is a human impossibility.[2] The prohibition is against holier-than-thou pontification in response to what the interlocutor has said or is doing. We say holier-than-thou, for it is inevitable between humans that *some* kind of reaction will be forthcoming (be it only a non-reaction, which is, as we all know, often the worst kind of reaction). The imperative is to follow the interlocutor with attentive and supporting presence, as it is her job to assist the interlocutor in becoming aware of the conformation or figuration of his and their fields and his experiences hereof. The point is to assist the interlocutor so that he may consciously experience his own reality and challenges, without the interviewer telling him what the challenges are. To insist on a point, this does not mean that the interviewer will not analyze or have an opinion of about those challenges. Neither does it mean that she should not share these opinions if asked. *The point is to keep back so that the space the interlocutor needs to unfold and explore his narratives is being created and remains open, with the interviewer remaining aware of the fact that the field between them evolves as they both bring their stories to this field*. It is the researcher's job to be there for the other, and that is by necessity a power-laden being (see Chap. 5).

Figure/Ground

We did not include the Dane Edgar Rubin (1886–1951) in the historical overview above, for his thoughts did not turn out to have significant impact on the philosophical foundations of the Gestalt tradition. He was, however, influential in suggesting two interrelated concepts of significance when the Gestalt tradition took a therapeutical turn, concepts that may be useful for social scientist. From his doctoral work, "Visually experienced figures" (1915), he developed the conceptual couple figure/ground. The main point was that the way we experience a figure depends on the background against which we see it. It was Rubin who drew the perhaps most famous Gestalt figure existing, where a white vase figures on a black background, or was it rather two black silhouettes on a white background? Both images are present, but they cannot be seen simultaneously. They depend on each other as background. The point may be generalized: Any figure will be experienced differently given different backgrounds (how different will of course vary according to what kind of figures and what kind of backgrounds we have in mind). Rubin discussed figures and ground literally, by concentrating on material objects such as vases, but Rubin's insight was soon applied to non-material areas, where figure/ground were used metaphorically. We are thus again dealing with the basis for relational understanding of perception, and the same focus on meaning that we find in phenomenology, which formed the basis for Gestalt psychology's insights (and experiments) about humans as meaning-seeking creatures. Many readers will be acquainted with another figure illustrating the same point as Rubin's vase/heads; the one that shows either a Jugend-inspired, female body bent to an arch, or, alternatively, an old woman's face framed by a black veil.

The Gestalt psychologist Gordon Wheeler (1998) has elaborated the idea behind the concepts like this:

> For example, take the figure of, say, a haystack, within or against the gestalt or map of the 'war landscape'. According to the subject's goals of the moment—survival, conquest, escape, reconnaissance, forage, rest, and so forth—the same haystack may be perceived as threat or shelter, protection or obstacle. And more than this: its value and even identity will be perceived differently, according to where and how it lies in relation to other perceived objects on the 'map'—lines of sight, battle lines, distance and access, and so on. Thus perceived and located (and thus by definition

> evaluated/categorized) the haystack becomes part of the changed and organized ground, or map, in relation to which new figures may then arise. (p. 28)

Where the concepts Gestalt and field describe an individual's psychic landscape or psycho-social relations between two or more people at large, figure/ground is used to circumscribe smaller, more specific psychological stuff. Figure/ground is thus a conceptual couple that defines the present situation here and now, while the pair Gestalt/field points to more protracted and all-encompassing phenomena. Figures can be considered as Gestalts and ground as (part of) a field, but the point is that we need a tangible set of concepts which signifies that we are positioned in the research situation, not in life in general.

Figure and that which develops into a figure in the field between researcher and informant is what the researcher should look out for. It refers to the specific, and is often an expression of those problems, challenges and modalities (ways of being and doing) important to the informant, that is not only interesting to the researcher to explore as such, but is also often that which has contributed to making the informant willing to be interviewed in the first place (see the following section on blind researchers). These mechanisms are present also where the research is less exploratory, or where the researcher is set to evaluate something on behalf of an employer. The figure becomes meaningful against the informant's specific background and experiences, which again form a part of the informant's field. An example is when a "general concern" for child abuse becomes a figure to a public health visitor, and when this concern (that is figure) is linked to a general feeling of powerlessness vis-à-vis the public apparatus she has to cooperate with and which makes up a part of the field she must relate to. Furthermore, that which develops as a figure against a given background can in a different context form the background for different/new figures.

A useful phenomenological manoeuvre, which is also a common piece of advice in qualitative methods which may reveal valuable information, is to start an interview by asking what a day at work usually looks like, ask for detailed descriptions of what the informant does when he comes to work (takes off his coat, prepares the coffee, opens his mail, reads yesterday's reports, attends a meeting, etc.), whom he talks to, and so on (cf. Patton 1990; Smith 2005). Why? The reason is that it is often useful to warm to the theme, to make the interview situation less awkward,

get to know one another a little, and thus remove some of the tension and nervousness both parties may feel prior to the interview. Within the framework of the Gestalt approach these are also important elements, but in addition to this kind of "warming up" through introductory questions and "benevolent" conversations, the Gestalt approach will emphasize that the "warming up" will contribute to distinguishing the informant's figures. The field that develops between the two may be thought of as the ground on which figures emerge. The figure will be that which is of most interest to the informant, something that preoccupies him in relation to the topic(s) of research. There is nothing wrong with the "I start carefully and see what happens, before I begin asking the real questions" approach, but one may easily increase the fold of what is sown by being on the alert for emerging figures. Thoughts that have come to the surface since the informant agreed to be interviewed often appear as figures without further ado.

Blind Researchers: Examples of Figure/Ground

Together with a colleague, I (Cecilie) was going to interview three project managers about their work on gender equality and diversity in a remote region in the northern parts of Norway. They had received funds for a project intended to implement parts of their gender equality and diversity research and work in practice. The practical project was to be implemented through the existing institutional frame of a research centre, where they all worked. It was a 3-h flight and an almost equally long car ride just to get there. The project managers received us in a lunch room overlooking the wine-dark sea. The project managers, themselves professional interviewers, had evidently planned a smooth transition to the formal interview. After some small talk about our trip and where in Norway we lived, the conversation turned to the topic we were there to discuss: their work on gender equality and diversity in the region. The figure was immediately evident: the practical work on equality and diversity in the local community—to facilitate gender equality and diversity in work places and within families—was already a well-integrated part of the overall activities (research and development) of the centre. We talked a bit about this but agreed to stop and return to the subject in the formal interview. Still, it was difficult for all of us not to talk about the topics we were there to explore. We hurried through lunch.

At some point during the interview, my colleague and I tried to isolate the topic we were there to evaluate; whether this practically oriented gender equality and diversity project was successful. Our mission was to evaluate the practical dimension of the project, that which had been allocated additional funding, and not the centre as such. The figure re-emerged immediately: to isolate the work conducted in the practical gender equality and diversity project from the overall research and development activities of the centre was meaningless. It was the synergy effect of the project and the established research activities going on at the centre that made the project managers enthusiastic about their work.

One of them explained: "If you consider it this way, that the project and the centre are two different things, one should have set up an independent project organization. I assume that the reason why this did not happen was due to the intention of synergy".

This is a simple example. The informants were three highly competent, adult persons carrying a message. They were passionate about their work, and to a large extent they directed the interview themselves—not just through facilitating the location and the lunch, but also through power point presentations that sought to transmit how they worked. We were explicitly told what was important to the informants; still they had to articulate the same figure repeatedly. We, the researchers, were locked in an understanding of our evaluation and did not hear what was important to the project managers.

My preunderstanding—which became clear to me during the interview—was first of all formed by the obligation I felt towards my employer. Our employer had explicitly stated that he wanted us to evaluate the project, and not the entire centre. This preunderstanding was thus linked to an aspect of my pre-field autobiography as being obedient and confluent (see Chap. 5). As a well-raised person, I listened respectfully to what my employer wanted. I also had a vague understanding of what it meant that the project was dealing with practical and specific facilitation of gender equality and diversity activities such as workshops, lectures and interaction and initiatives with the local community, and that this practical work was based on the knowledge produced by the research centre. Second, it appeared that my vague preunderstanding was linked to my autobiography. My way of understanding challenges related to gender equality and diversity originated from living in a capital city. I have lived in Oslo since I was a child, and national media transmit an understanding of gender equality and diversity that is fairly similar to my

own. Questions about minorities in large and medium-sized cities often concern cultural differences, conflict between groups and segregation, as well as problems attached to general integration, religious conflicts, the rights for women to wear the hijab versus the possible repressive social aspects of the same practice. In this region of Northern Norway, however, minority issues were about something different. One of the informants explained the difference in short:

> It's about communication.
>
> Me: "What, communication?" And another project manager replied:
>
> Yes, the women here need to have cars. If not they get isolated and can't move anywhere, meet anyone, can't do the shopping, take their children to school or work. We are concerned with trying to make women taking the driver's test.

This experience served as a useful reminder that the figure/ground concepts help to sharpen the lens to the situation, at what happens here and now. You and your informant/s will form a field. The interview takes place in a specific context in a specific room at a specific time, and this will influence the field between you. This also applies to what you are both wearing and the way you present yourselves to one other. Common, albeit gender stereotypical, pieces of advice that go for interviews and fieldwork, would be: If you are a woman, be careful to wash your hair before interviewing a female executive, who may be meticulous about such things, and will probably judge other women on this criterion. If you are a man, do not wear a coat when interviewing a construction worker, since it may give associations to the white-collar staff in the factory, such as controllers, who often wear coats, and so on. Be attentive during the interview. What do you sense? What do you see? And how does it feel to you? Are you nervous, uneasy, tense? Excited? At what point during the interview does this tension cease?

As already mentioned, many books on methods highlight the significance of starting an interview by talking about something benign that has to do with day to day practices and everyday life (for example Gillham 2005; Silverman 2005). This could be something that happened as you entered the designated room for the interview, or something that happened on the way to the interview, as seen in the example above. Many will experience that the informant starts to talk about one

of the topics of the interview during these introductory exchanges. This is promising; it means that something has already become figure to the informant. If you imagine that this person's experience with what you want to talk to him about makes up his ground, something will appear from this ground and become figure. This figure is probably your entry point to the field—to the informant's experiences, what he is interested in and finds important—and to your analysis, as shown in the case above. When your interlocutor has poured out the issues that are figure to him—e.g. a relationship with another person in the institution, or a staff member, or the way they do things there—you may move on to other topics you want to discuss with him. Our experience is that allowing the figure to emerge this way eases the interview and gives you better data. You acknowledge the informant's points of view and considerations, and you may get knowledge about people and institutions that you would otherwise have missed. This is the first step. The next step is the rest of the interview.

Notes

1. This is what sets social studies apart from individual-oriented studies such as psychology or literary studies; see also the programme on institutional ethnography articulated by Dorothy Smith (2005).
2. That is what we would argue, with Gadamer. This is contra Husserl, who held up the ideal of what he called epoché, that is, the suspension or bracketing of judgement, so that the world may be taken in as if the person experiencing it were a tabula rasa or clean slate.

References

Brownell, P. 2010. *Gestalt Therapy: A Guide to Contemporary Practice*. New York: Springer.

Gillham, Bill. 2005. *Research Interviewing. The Range of Techniques*. London: Open University Press.

Hostrup, Hanne. 2004. *Gestaltterapi Inoføringi Gestaltterapiens Grunobecreber*. Copenhagen: Hans Reitzels Forlag.

Patton, Michael Quinn. 1990. *Qualitative Evaluation and Research Methods*. Newbury Park: SAGE.

Silverman, David. 2005. *Doing Qualitative Research*, 2nd ed. London: SAGE.

Smith, Dorothy E. 2005. *Institutional Ethnography: A Sociology for the People*. Lanham: Alta Mira Press.

Wheeler, Gordon. 1998. *Gestalt Reconsidered: A New Approach to Contact and Resistance*. Cambridge: The Gestalt Institute of Cleveland Press.

CHAPTER 5

Interview Techniques

Abstract The researcher's goal in the field is instrumental; it is to create as much data as possible within the worldly constraints that bear on her situation. The objective is to make the informant talk about what the researcher is interested in, be that the life stories of prisoners, the identity of diplomats or the body techniques of female cleaners. The better situated the researcher, the better the data yield. In Chap. 4, we discussed some Gestalt concepts that may be of help in this regard. In this chapter, we look at another Gestalt contribution that is important, not least as a dampener on the instrumentalisation of interlocutors that inevitably follows from the researchers seeing them as sources of data to be mined.

Keywords Self · Awareness · Body language · Contact mechanisms Interpretation

Acknowledging the Researcher Self

Gestalt insights complement contemporary method literature in two major ways. First, most methods books only briefly concern themselves with the researcher's own pre-field autobiographical situatedness. This is probably due to the ethos that researcher's should not be therapists, a demarcation line is drawn sharply, if often implicitly (e.g. Fog 1994). Second, and relatedly, this is precisely the reason why researchers should have some conscious autobiographical knowledge in order to prepare

C.B. Neumann and I.B. Neumann, *Power, Culture and Situated Research Methodology*, DOI 10.1007/978-3-319-59217-6_5

themselves for how they may manage active, in-field, relations (Holstein and Gubrium 1995). Researchers are not therapists, but if you have no idea about who you are, where you come from and how you normally relate to others, your chances of conducting ethically sound research are slimmer than if you have a least some knowledge of yourself. Researching marginalized persons in sociology as well as in international politics, may invoke a variety of personal reactions and responses in the researcher, that may be acted out as over-identification or judgemental assumptions about the others in-field as well as pre- and post-field, if they are not acknowledged and dealt with (Lynch 2008, p. 718).

In addition to these two major contributions, the Gestalt tradition also offers the field-working researcher a set of techniques of communication, namely awareness, being sensitive to body language, contact mechanisms, and the misleadingly named prohibition against interpretation. We explain these techniques and give examples of their use in in-field situating and pre-field autobiographic situating. We will return to post-field textual situating in the following chapter.

Awareness

To be aware is to be consciously and attentively present in the here and now. Being aware as a researcher means taking note of your state of mind (thoughts, emotions and bodily sensations), paying attention to the informant (words, how they are uttered, body language) and paying attention to the wholeness of the situation. Attention to body language is particularly important during interviews with people in vulnerable situations. Small things like a bouncing foot, staccato mimicry, a restless hand or hands folded hard over the chest may signal that you are talking about something which makes the informant uneasy, something that is difficult for her to talk about and that causes friction or resistance, or that reminds her of other unpleasant experiences. It is not the researcher's job to inquire hunches of the informant's resistance; it may, on the contrary, be downright unethical and even harmful to explore issues that you suspect might distress the informant during the interview, for example, by suggesting that you interpret her bodily signs as signals of some sort of inner distress. Read these signals as warning signs. Perhaps she perceives you as a bit offensive, or feels that you are not listening properly. Perhaps she thinks you are clumsy, or perhaps she is generally or specifically annoyed. You can check this out in a general manner by

asking her if there is something else you should know, if there is something about the topic she has not yet had the chance to share, or if your approach put the conversation on a track she did not feel is appropriate.

Body Language

Approaching the meaning of the body yields a number of ways of contextualizing what it is to be human. The challenge is how we can think about and exploit this in the context of social scientific research practice. We approach this through phenomenology, which we link to Gestalt thinking and the practical exercise of awareness within the interview situation.

One might have expected that the knowledge we now have about the significance of the body as the site for human experiences (Damasio 1994; Lakoff and Johnson 1999), combined with phenomenology's attention to our relationally conditioned existence in the world and Foucault's focus on docile bodies, Bourdieu's focus on habitus, etc. would have contributed to research practices where researchers would include these insights in their interaction with informants in the field and in interviews. This is, however, rarely the case. The body is often left out of social science research, unless where the body is itself a topic of research (for a nice exception, see Staunes and Søndergaard 2010).

In the Gestalt tradition, body language is seen as an integral part of interaction. The biting of lips, the hesitation every time when a specific topic comes up, the restless gaze, all these are data. The researcher is not in a position to investigate body language directly during the interview.[1] It will most likely be perceived as offensive, rude or intrusive by the interviewee. It would also be unethical, for a researcher has no right to intrude wilfully into the informant's feelings. We illustrated this already in Chap. 2, where the young girl clutched a pillow to her stomach during the interview. The point is that researcher should pay attention to such signals, and read them as warning signs. However, if the researcher does not keep in mind that body language is a vital part of human communication, she will miss an important access point to data creation. The question is what to do with it, and how.

The researcher may use this information to situate herself, and ask "what does this body language and that interlocutor's body do with me"? What do my reactions to the interlocutor tell me about my expectations, my ethnicity and class background, and my gendered ways of

viewing the world? The researcher is a product of her social context, just like any other person. By being a product of a structure, there are significant insights of the structure to be found in studying one's own thoughts, emotions and patterns of behaviour. Indeed, we have to study structures through their effects, for they are not readily observable, so how would we study them otherwise?

Since the researcher is a product of social context, her observations of social contexts will always be implicitly comparative to those social contexts that shaped her. Studying one's own pattern of behaviour and interpretation is thus not just a source of understanding one's own background, but also a way of sharpening the data collection process: by situating yourself you reduce the margins of inaccuracy stemming from interpretations based on your own background, and focus instead on the social context from which the informant hails.

We will give an example of why caution is important when analyzing and interpreting others; and how misinterpretation can be exploited as a part of the researcher's work to situate herself. The example is taken from a "non-participatory" field observation from Cecilie's doctoral research, in which part of the data creation happened during observations of children, parents, public health visitors and others in the waiting rooms of public health clinics in the traditional working-class and middle-class areas of Oslo.

One of the first days of observations at a public health clinic in Oslo East—when I was still set to know as much as possible about child abuse—a father entered the clinic with a child that I guessed had been adopted from an East Asian country. He looked rather ragged, had big wounds on one of his legs, and was dressed in a wrinkled shirt and a dirty pair of shorts. His hair was in complete disarray. The man sat down on the sofa, and fumbled nervously with the child. He tried to straighten his shorts, dusted off his shirt, and shifted the child from one knee to another. He was called into the health visitor's office, and stayed in her office for one and a half hour. I became increasingly convinced that this was a father in trouble, that he could be a child abuser and that this was the reason why the consultation lasted so long. When the door opened, the health visitor appeared and seemed a bit hectic, while the father looked calm and happy. After father and daughter had left, I asked the health visitor what kind of man that was, and she replied that this was the happiest father she had ever met. He and his wife had longed for

children for years, and they had finally been able to adopt a little girl. The little girl was scared, however, and had problems with her attachments, so the father hadn't slept in a fortnight. This was the reason for his tiredness and disorderly looks.

There are several interesting things about this event. Most obviously, my interpretations of the father's body and body language were completely wrong. I, who at that time of the data collection, was tuned into identifying children who displayed signs of having been abused and for parents who did not provide their children with adequate care, understood the man's body and behaviour in the waiting room as an expression of exactly this. It is an old saw that you see what you expect to see, but the researcher has to guard against this constantly. There is another trivial and, exactly because it is so trivial, important methodological insight to be gained from this. To be an observer who does not participate directly in the interactions with the actors under observation, also means that you acquire limited information on the meaning and opinions they attach to their actions. However, despite the limitations involved in this type of observation, the information generated from it—inaccurate as it may be—might also be a very important tool to conceive/understand of the central topics in a given field situation; the way interaction takes place between different persons, and the atmosphere characterizing it. Noticing how parents and children do or do not talk; who talks to whom and how; how people are dressed; which professional actors are visible in a waiting room; whether the doctor or the health visitor collect the clients; or if the assistant nurses or the secretaries do it, can all contribute to our understand of different practices that may reveal how this specific place appears to its users (authoritative/educational or friendly and welcoming), what is considered the basic objectives (e.g. children's physical health), and about the hierarchy existing between professions, etc.

What became central to the analyses that I developed later, and the reason for paying specific attention to the importance of the misinterpretation of this father, has to do with both field situation (how we experience and are being experienced or perceived by the actors we interact with) and with autobiographical situating. It was the episode with this father, combined with one concerning a mother who appeared to come from an upper middle-class background, and who appeared much more extrovert, confident and relaxed than the other mothers at the clinic,

that made me acutely aware of how I categorized other people according to my own gender and class background. There was something nearly shocking about the father's bodily appearance and ragged dressing, and there was something suspicious about the way he handled the child. He was, in addition, a father, not a mother—and fathers may not be as caring as mothers, I thought. I grew up under privileged conditions in Oslo's West End and my implicit expectation of parents (read mothers) at the health visiting clinic was that they, despite coming from a different class background than I, should behave with calm and confidence, and dress themselves and their children in clean clothes. And even though I am politically and professionally of the opinion that it is important to allow fathers to enter into care relations to their children, it became eminently clear that the discursive ideal of mother as primary carer worked, and works, in me, too (Hennum 2014). This confrontation with my own categorisations regarding fear of adults abusing children and the insecurity evoked as to whether men and women may be equally capable carers for their children, led me to explore how public health visitors establish boundaries for what is "normal" and "deviant" in parents' and children's behaviour. It also drew my attention to how views of parents and children are shaped by gender and class. This subsequently led to a theoretical discussion based on my empirics. The key thing is that it was primarily the work I did on situating myself by asking where I came from and how I was seen that opened up for these analyses.

Awareness and Body Language During the Interview

If the researcher has reflected on her own bodily experiences before she enters the field, and knows how her body feels and how she normally reacts when she is embarrassed, shameful, shy, docile, and so on, she will come much better prepared (Patton 1990). She will also be better prepared to read the other. In some fields of research (like migration studies or child protection studies) consciousness about bodily experiences and how we react to them (e.g. how we feel and act when we are met with irony or rejection) is very useful in order to ensure ethically sound research.

One of us was sought out by a student who wrote on refugee children who were seeking asylum. Her problem was that she could get no data. Upon inspection of the questions in her loosely structured interview

guide, it turned out that she had structured the interview in the same way as do police officers who interrogate these children when they appear on the border (why are you here, how old are you, whom do you miss most, etc.) As a direct consequence, the questions stirred memories of traumatic experiences such as fleeing, police questioning, loss of family. When asked, she had not experienced that as relevant. Here we have an example not only of an ethically unacceptable way of conducting an interview, but also of what may happen when a researcher does not know how to read body language. Most people have a capacity to increase their empathic abilities. Learning to read body language may be one place to start.

It is of course impossible to be fully conscious of what happens at all times, but it is possible to be aware that you are not aware, and in a 1- or 2-h interview situation it is fully possible to be aware throughout. If you become some kind of audience to your own interview, by being an interviewer who stops paying attention, the interview will most likely be of poor quality. It is hardly inspiring to be interviewed by someone who does not pay attention, or who repeats what the interviewee experiences as the same question. Awareness means paying attention, even if you should be bored, restless or mind-wandering.

At the opposite side of the scale, there is the shock. Interviews with war survivors or police who investigate ritual murders may, for example, bring to light information that is so upsetting that the researcher simply closes down. One way to guard against this is to ask yourself whether you will be able to handle the kind of information that will come up before you do the interviews. One of us (Iver) has done work on group identities and how they change during conflict. A logical next step would be to do interviews with norm entrepreneurs, for example, Serbs who instigated group rape as a political weapon against Bosnians during the 1990s. That road is cut off for me, however, for I was used as a play thing by my nanny as a very young child, and am still given to uncontrollable bursts of anger when issues regarding child abuse and rape come up. If one has done the autobiographical situating work and knows one's weaknesses, one may stay clear of foretold accidents. If one thinks one can handle such situations and they still blow up in one's face while interviewing, awareness of one's own bodily reactions will be of immense help in limiting unhelpful fallout.

Contact Mechanisms

Chances are high that a researcher behaves fairly similarly in interview situations as she does during more unrestricted conversations (at least after the first nervousness has ceased). The Gestalt tradition has identified six default patterns of individual behaviour and named them contact mechanisms. They are projection, introjection, retroflection, deflection, confluence and egotism. Contact patterns indicate the ways humans interact and otherwise relate to one another in general, and are not terms that fix a person according to a certain personality characterization (Clarkson and Mackewn 1993).[2] It may be useful for a researcher to know about them all, but two, confluence and projection, are of particularly pressing importance. We mark them out not only because these two contact mechanisms are the most common and thus easy to recognize in self and others, but also because they may be particularly determining of data production, in the sense that these kinds of behaviour may easily yield information that is markedly different from information culled from other informants who express views about the same phenomenon.

Confluence refers to a way of being with others that is characterized by an inclination to agree with the ones we are spending time with. It reflects an underlying desire to make things "flow", to find things and ways of being we can agree on; values, norms, political viewpoints, how to raise children, etc. Clearly, without the ability and desire to create confluence, there will be a great deal of conflict in human interactions and communications. Confluence is a social solvent. However, the need to create confluence is also the neurotic's problem: I subordinate what I think and desire to the need of avoiding conflict and disagreement. "We are going to be one big and happy family". Woody Allen's persona Zelig, from the eponymously named film, is the ultimate in confluence; he literally becomes the person he is speaking to.

Egotism is normally depicted as the opposite contact mechanisms of confluence. Persons characterized by an egotistic contact pattern will not be particularly interested in what others think or feel, but is instead intended on voicing his concerns, and his own opinions. He causes a lot of conflict and disagreement all around, but does not quite care. The person controlled by *introjection* searches for critical signals from others indicating that he should improve or do better. He often accepts the blame, without considering that others might be to blame. The one *retroflecting* does to others what he wants them to do to him. If,

for example, I love chocolate cake, I will automatically assume that others too will be thrilled if they are offered chocolate cake. A more topical example is if I like to be supervised in a certain way, and thus assume that my students also prefer to receive the same type of supervision, without checking out with them whether this is actually the case.

Projection is about me perceiving what you say and do according to specific expectations I have attached to you, and which do not have anything to do with who you are. If, for example, you remind me about my mother I will perceive what you say and do as a continuation of my expectations to her, not to you. The one who is *deflecting,* finally, seeks a form of isolation, and will in given situations try to pretend the social world does not exist. In situations she perceives as uncomfortable, for example, she will look the other way, and the impressions, expressions and looks she perceives as uncomfortable will make her distance herself further from the field, in thoughts and daydreams. When you ask her opinion it becomes apparent that she has not heard a word of what has been said or done.

How, then, can we use our knowledge about the contact mechanisms of confluence and projection in interview situations and in analysis? We start by looking closer at the researcher's own tendency to confluence. Confluence means that the boundaries between you and me become indistinguishable. Confluent persons wish to maintain that blurriness when interacting and communicating with others, because it prevents conflict and disagreements. If you have a tendency towards confluence, it is probably easy for you to make people talk during interviews. You will try to create a nice and disarming contact between yourself and the interlocutor, for this is what you normally do. You are likely to be good at listening and giving encouraging signals which invite the interlocutor to talk. The challenge to those inclined to confluence would be how to ask follow-up questions of a confronting or potentially distressing nature. Fear of breaking the confluence, destroying the friendly atmosphere, may lead to the researcher avoiding to ask about things that should clearly have been asked for. Fear that the interlocutor will perceive such questions as annoying or hurtful will come in the way of data creation. An example may be that when knowing that your interviewee is poor, you avoid asking questions about the person's income, even though you are conducting action research with a view to recommend initiatives intended to create income for persons such as your interviewee.

A second challenge is projection. We all project, all the time, and it is necessary in order to try to understand how others experience the world. This is the basis of inter-subjectivity; I experience the world like this and therefore assume that you experience it more or less similarly, and in this way we may assume we share a great deal of views, norms and thoughts. If projection is a very active contact mechanism, however, it may lead to over-interpretation of others in your image. You see them as you see yourself, and you accuse them of things that you do yourself but will not recognize. You may have a tendency to define others in your own image of how specific men or women are, without investigating what the world looks like to them. This may prevent you from being explorative and curious about your interlocutor in interview situations. If this leads to inattentiveness to the details of his stories and experiences it may cause the interlocutor to feel ignored and categorized. It may, furthermore, influence your analyses, and is thus a concern where all three forms of situatedness are concerned.

I (Cecile) have a tendency towards confluence. The need for seamless and friction-free communication is more intense when I feel insecure, as I often do in the beginning of interviews. This is often amplified when I research other well behaved, conscientious upper middle-class women. To be aware of this does not mean that I try to be less nice than I usually am, but I do carry with me a pre-written list of critical questions in order to make sure that I own up and actually ask them. This is a technique I use in order to be certain that my need for confluence does not get to command the interview completely. As we will demonstrate in the next chapter, being a researcher with an inclination towards confluence may also be a challenge when it comes to textual situatedness.

If confluence is about over-identifying with the interlocutor, then p*ro-jection* is about me perceiving what you say and do according to specific expectations. This has also been a concern among feminists. Drawing on Harding and others, Ruane (2011) puts it very well:

> As feminist standpoint research suggests, all perspectives are partial and rooted in particular experience […]. This can significantly limit efforts at inclusion, not because of bad faith, but because of lack of imagination or an inability to relate. Iris Marion Young (1994) argues that we may never be able to "walk in someone else's shoes"; but instead, may only project our experience onto others in frequently inappropriate ways. (p. 53)

In other words, to project is also an attempt to include the other and try to imagine what it is like to be him or her. If projection is a very active contact mechanism, however, it may lead to over-interpretation of others in your image. You see them as you see yourself, and a neurotic consequence that many of us may recognize is that you accuse them of things that you do or feel yourself but will not recognize. If I am irritable I accuse you of being irritable, and I will have a tendency to deny my own irritability if you correct me on my interpretation of you. If I understand academic achievement to be the most valuable of possible professional achievements, I will assume you do the same (and I will be surprised or even confused if this is not the case).

I (Iver) am impatient and have a tendency to project. During interviews or in the field, this means that I am at risk of not paying attention to things people say that do not fit with my generalized expectations. For example, Cecilie and I conducted an interview with two female diplomats on their experiences with being women in a male-dominated sphere back in the 1970s. After the interview, that we tape-recorded, my impression of what they had shared was that it was of little value to my gender research interest. However, when we transcribed the interview it turned out that they had said a lot of interesting things. What seemed to have happened was that they had shared their gendered experiences in relation to a number of seemingly unimportant events, and that they talked about this in a manner that I found to be chatty. What I had projected onto them was a certain state of knowledge and analytical clarity on the subject that they did not feel like giving it, at least not then and there. If the interview had not been transcribed, I would have missed valuable information because of my tendency to project.

When people interrupt me or do not listen, my impulse is to stop talking. Like most alert children, I had my fill of that back then, but the key thing here is, I think, that I spent the better part of 7 years (1989–1996) trying to introduce poststructuralism to Norwegian political science. Apart from the occasional furious attack, there was no reaction beyond a steady insistence that this had nothing to do with science. When I gather data and am stonewalled, an experience that all field-working researchers will sooner or later come across, my impulse is to walk away. That impulse has to be lived down, for no interaction spells no data. So I think to myself 'you little ignoramus' and change the tack of my questions.

The Prohibition Against Interpretation

The researcher is the one who is primarily responsible for the relation with the interlocutor. In the Gestalt tradition, this is expressed through a proscription or ban on interpretation.[3] Everybody interprets and cannot help doing so, but an interpretation can be open or closed. The issue here is not so much the formulation of specific questions, but rather what kind of attitude that grounds the questions. The prohibition against interpretation is also relevant when processing field observations and interviews in the analytical stages of the research.

Here is an example from a study project for students of criminology at the University of Oslo. We wanted the students to walk the streets of Oslo and observe beggars (who at that time were mostly Norwegian drug addicts). The idea was to train the students in observation by having them write down what they saw as accurately as possible, and later analyze what they had observed and write a paper about it. In this way, they would become conscious of their own emotions, thoughts and prejudices towards beggars. The background for this study project was the following: criminology is a subject that often raises strong emotions in students. Many come to criminology with the idea that there are dangerous people in society who deserve punishment, they believe prison sentences ought to be long and that the state should be building a strong and authoritative police force. The students come to read research that argues that these conceptions are twisted and wrong. Students then tend to start pitying prisoners subjected to violations; they start seeing them as persons who come from difficult backgrounds and have lived hard lives. After a while, students tend to think that we have enough police, and that prisons should be dissolved. The professor's challenge is to nuance these emotions related to the students' learning process, and stimulate reflexivity in their knowledge, writing and conversational skills. This challenge is more pressing in criminology than in many other disciplines because criminology deals with so many difficult and emotionally loaded questions—for better or worse. Better, because criminologists are better positioned to contribute to development in ethical research practice, research reflection and theory development. Worse, because criminology risks being a dogmatic, political and self-warranted project. For example, Järvinen (1998) challenges her own research on prostitution, and questions her own assumption that prostitutes speak more truthfully than their controllers and helpers.

Our exercise, in which students were sent out to the streets, was aimed at levelling out some of this. They were given questions, inspired by the Gestalt tradition, which invited them to distinguish between what you see, what kind of feelings you experience when you see it, and which thoughts, fantasies and interpretations emerge as a consequence. What do you see when you sit in the main street of Oslo? Who passes by, what do they wear, who do they talk to, who talks to them? How does it smell, and how does it look? By sorting out descriptions, senses, emotions and interpretations in this way, that is, by doing exercises in situating, it becomes easier for the students to meet the field in a more balanced way. For example, it becomes possible to separate political sympathy and antipathy (e.g. towards policies of the government), knowledge of those experiences many beggars have from institutions (some bad, some good), and considerations of individual beggars as moral actors. Without such training it is hard to develop nuanced analyses. It may easily become a story about nice, innocent, victimized prisoners who fight against bad and evil prison guards, where the analytical thinking about the prison's systemic, interactive and moral properties disappears (Damsa and Ugelvik 2017).

By entering directly into an analytical or interpretational mode, with clear ideas and assumptions about the informants and their opinions, there is a clear risk that the analytical research either breaks down completely or adopts a preconfigured track (cf. Alvesson and Sköldberg 1994). This is one interpretation of what phenomenologist Husserl meant when he pointed out that one must bracket one's experiences when meeting other people's opinions (Gallagher and Zahavi 2008). Husserl was convinced that people who conduct scientific work ought to be conscious of their interpretations and inclinations (preunderstandings), in order to set them aside altogether when meeting others. That is hardly possible, but the exhortation to become conscious of thoughts and emotions, and in this way having them under some sort of control or even bracket them, still stands. A large body of literature demonstrates how feelings and rationality cannot be separated, for feelings drive inquisitiveness (the place to start reading is Damasio 1994). Our experience is that emotions such as moral indignation and anger cannot be made to go away, but that nursing them is hardly compatible with the clarity required when analyzing empirical material. Difficult emotions must in one way or another be squared before we can enter into an analytical mode. This does not mean that we do not feel when we analyze,

but that it is imperative to vent before writing, for in realizing existing and active emotions, one ascertains a certain distance to emotions related to the research. In this way, emotions do not get to dominate completely, but can instead serve as analytical leverage and inspiration, as we discussed above in the paragraph on body language.

We do not mean to imply that this technique will bring the researcher closer to the truth about people's opinions or life world. It will, however, bring you closer to a good conversation, and also to writing a good text. What is at stake here is not "objectiveness", but the need to stay in the background in order to allow the interlocutor space, and avoid capturing him in closed interpretations. The prohibition against interpretation is an exhortation to the researcher to assert self-discipline, so that she can take fuller responsibility for data creation and textual production.

Notes

1. Unless this is explicitly clarified with the informant beforehand, and is a part of the research project.
2. The background to this introduction to contact mechanisms is normal neurotics. Psycho-pathology is beyond our field and area of expertise.
3. Typical interventions may be "What are you thinking of when I say…", "I believe that you are angry because he behaved like that towards you", "How do you feel when I say this?" or "When you tell me this about X I feel ashamed. How do you feel when I say that?"

References

Alvesson, Mats, and Kai Skölberg. 1994. *Tolkning och reflektion*. Lund: Studentlitteratur.

Clarkson, Petruska, and Jennifer Mackewn. 1993. *Fritz Perls*. London: Sage.

Damasio, Antonio R. 1994. *Descartes' Error. Emotion, Reason, and the Human Brain*. New York: Avon Books.

Damsa, Dorina, and Thomas Ugelvik. 2017. A Difference That Makes a Difference? Reflexivity and Researcher Effects in an All-Foreign Prison. In *International Journal of Qualitative Methods* 16: 1–10.

Fog, Jette. 1994. *Med samtalen som Udgangspunkt*. Copenhagen: Akademisk forlag.

Frykman, Jonas, and Ingvar Løfgren. 2003. *Culture Builders: A Historical Anthropology Of Middle Class Life*. New Brunswick: Rutgers University Press.

Gallagher, Shaun, and Dan Zahavi. 2008. *The Phenomenological Mind*. New York: Routledge.

Hennum, Nicole. 2014. The Aporias of Reflexivity: Standpoint, Position and Non-Normative Childhoods. *Journal of Progressive Human Services* 25 (1): 1–17.

Holstein, James A., and Jaber Gubrium. 1995. *The Active Interview*. Qualitative Research Methods Series 37. London: Sage.

Järvinen, Margaretha. 1998. Att konstruera och deconstruera sociala problem. I *Kvinner på randen*, (red). Annalise Kongstad, Britta Kyvsgaard og Anette Storgaard. Aarhus: Aarhus Universitetsforlag.

Lakoff, George, and Mark Johnson. 1999. *Philosophy in the Flesh. The Embodied Mind and Its Challenges to Western Thought*. New York: Basic.

Lynch, Cecelia. 2008. Reflexivity in Research on Civil Society: Constructivist Perspectives. *International Studies Review* 10: 708–721.

Patton, Michael Quinn. 1990. *Qualitative Evaluation and Research Methods*. Newbury Park: Sage.

Ruane, Abigail. 2011. Pursuing Inclusive Interest, Both Deep and Wide: Women's Human Rights and the United Nations. In *Feminism and International Relations. Conversations about the Past, Present and Future*, ed. J. Ann Tickner and Laura Sjoberg, 48–67. London: Routledge.

Staunes, Dorthe, and Dorthe Marie Søndergaard. 2010. Who is Ready for the Results? Reflections on the Multivoicedness of Useful Research. *International Journal of Qualitative Studies in Education* 21 (1): 3–18.

CHAPTER 6

Pre-field Autobiographic Situatedness and Post-field Textual Situatedness

Abstract In this chapter, we return to the two types of situatedness that come in addition to in-field situating; pre-field, autobiographic situating, which enter prior to and bleeds into in-field situating and post-field text situating, that follows after in-field situating. We start by problematizing autobiographic situatedness with regard to memory and biography, and situate our understanding of the self in relations. We discuss the importance of thinking though how the researcher came to pick and pitch the research the way she did, and move on to problematize textual situatedness in relation to the various contexts in which the text may be supposed to do a job.

Keywords Autobiographical situatedness · Textual situatedness · Power Ethics

Pre-field Autobiographic Situatedness

Autobiography is an unreliable genre. "Time is foreshortened, details selected and highlighted, action concentrated, relations simplified", historian David Lowenthal (1985, p. 216) writes in his widely read book *The Past Is Another Country*. Discussions in oral history and folkloristic environments ask what constitutes an oral source. The general answer is that oral sources are people's own presentations of their lives and its context, as it is written down by themselves or told to others. Langless and

C.B. Neumann and I.B. Neumann, *Power, Culture and Situated Research Methodology*, DOI 10.1007/978-3-319-59217-6_6

Frank (1981, p. 89) began their treatment of autobiographies by stating that:

> To put it simply, autobiographies are reports by individuals about their own lives; what distinguishes them from biography is that the author and subject are one and the same person. The authors of autobiographies typically narrate those events that went into making them unique persons. […] presumably, these are events that strongly affected the author's sense of self because, as one critic suggests, the author of an autobiography would have no reason to write one unless some sort of inner transformation had occurred.

They conclude from this that "an autobiography is, by definition, an account that focusses on the inner life" (Langless and Frank 1981, p. 90; see also Starobinski 1971).

The key methodological problem here is that memories often turn out to be the result of communicative social processes, rather than psychological time capsules. Memories are social. We are thrown into the world and stuff happens:

> Although everybody started his life by inserting himself into the human world through action and speech, nobody is the author or producer of his own life story. In other words, the stories, the results of action and speech, reveal an agent, but this agent is not an author or producer. Somebody began it and is its subject in the twofold sense of the word, namely, its actor and sufferer, but nobody is its author. (Arendt 1958, p. 184)

We are the result of a number of relations. Relations and the memories thereof change. Here is one reason why social scientists often study memory as a collective project. For our purpose it is however not the production of memories that is interesting, but how those memories are a precondition for the researcher's choice of research question and theoretical approach, as well as her behaviour during the data collection process. If you study how Roma are exterminated in Nazi concentration camps it is relevant why you choose to study exactly that. Is it because you have some ethnic or social attachment to the Roma? Is it perhaps because many other stories, such as the state of Israel's stories about the Jewish holocaust, have repressed it? Or is it due to a childhood fascination with travellers? We have here three very different ways into the field of research, and these ways will shape the research process. Given that we

are dealing with qualitative research, it is impossible to imagine that the researcher's situatedness will not colour the data production. It is therefore appropriate to reflect on how and to what extent this happens. And why did we use exactly this example about Gipsies? Precisely because one of us has a Roma great grandfather.

Tillery experienced this from the other end. When she herself was overweighed and studied overweighed persons, no one asked her why she studied the overweighed. However, when she lost weight and became slim, people started to ask her why she studied the overweighed (Dottolo and Tillery 2015, p. 131).

Anthropologists, who have trail-blazed field observation and interaction data as central to social analyses, were traditionally skeptical of conducting fieldwork in one's own society. Edmund Leach points to the factor of autobiography when he writes that "When anthropologists study facets of their own society their vision seems to become distorted by prejudices which derive from private rather than public experience" (Leach 1982, p. 124). This skepticism to fieldwork in one's own society is probably due to the lack of foreignness vis-à-vis the area of study, because the general view on data collection seems to be that "to be on fieldwork is to be a professional stranger" (Sjørslev 1988, p. 156).

It is a fact that it is easy to become home blind, that is, you cannot get a [w]holistic and detailed grasp on a society because what you see seems obvious, and so you do not really see it. There are, however, at least three counterarguments to this: first, it is problematic to talk about "one's own culture". There is not, for example, one French culture that all Frenchmen are part of. Any carrier of some variant of French culture will be a stranger if the French context is unknown. For example, an upper middle-class man from a big city may not easily blend in Béarn. A woman may not blend in a Marseilles football club board meeting. One may certainly be a stranger in what is ostensibly "one's own" culture. It is the researcher's duty to reflect on distance and closeness with respect to the object of study, and also on the consequences these may produce. It may actually be that it is necessary to accept your situatedness in order to understand the data. Iver, observed Norwegian diplomats within their own Foreign Ministry for 6 years. It is difficult, and in some countries impossible, to gain access in this way if you are not a citizen of that state. In Norway, Norwegian citizenship is a formal requirement for employment in the ministries, and the number of non-Norwegian citizens in the central administration is very low. In such situations, it will be very

difficult to find researchers without the "prejudices" Leach describes, because these potential researchers would not be able to do fieldwork in the central government's administration. Their possible prejudice is also their entrance ticket to the field.

A third counter argument to the proscription against field work within one's own country hails from the philosophy of science. It is impossible to sense social phenomena directly, if that means without some kind of prejudice, for without categories, sensing is not social, and so cannot be applied to the social. It takes a certain preunderstanding to organize what you see. The optimal relation between researcher and field is not, therefore, the greatest possible distance, with the researcher being a *tabula rasa* or clean slate relative to that which is to be observed. The optimal relation will be close enough to sort out and organize different kinds of information, and distant enough to afford analytical distance. The main point here is that it is the researcher's autobiography that determines how much work is needed to classify and analyze what she experiences. This calls for reflection on the significance of autobiographical situatedness.

The first question to ask is what one is situated as. Physiology is an obvious place to start. In many cases, gender and ethnicity show. The issue here is not self-identification, but how one is represented by interlocutors, and then looks are important. Then there is class, which is visible if not physiologically, then socially; the social shaping that appears in the body's form through walk, gesticulations, mimicry and sometimes also name and dress. Social scientists from Marcel Mauss and Norbert Elias to Pierre Bourdieu have done useful work in theorizing this as habitus.

In addition to physiology and social factors, one may also be situated according to experiences that have left their traces in the psyche. In a classic essay on body techniques, anthropologist Marcel Mauss (1936) argued that the body is the place where three social systems meet: the physiological system, the social system and the psychological system. To situate oneself autobiographically is not only about the physiological (sex, physical handicap) and the social (e.g. class, gender), but also about psychological situating. A language teacher at the University of Oslo had been in a Japanese concentration camp during World War II. If someone tried to force him into a position that he did not acknowledge as his own, he would be incendiary. He was conscious of this, and asked

his students to be considerate; both in the way they asked him questions and in the way they reacted when he had his outbursts. I, Iver, have a similar problem in my psychological luggage, and it is a problem that has fundamentally affected my theoretical approach to the social sciences. I grew up with a maid. From I was about 3 and a half to about 4 years old, I had one that used me as a sex toy. My arms were used in an intimate manner. In order to prevent me from telling my parents she would threaten me with needles, and let me suck her breasts. I still cannot bear the thought of changing my glasses for contact lenses. I have no doubt that when I so eagerly embraced Foucault as a theoretical lead, it was because his conceptualization of power spoke directly to these experiences. Foucault's is a relational understanding of power. He emphasizes micro-practices imprinted on the body, how such practices produce docile bodies, and, I think crucially in my case, how the subordinate is a participant in his own subordination.

Foucault's way of understanding power invites the researcher to begin with constructing concrete, everyday interaction data and the submissive party's participation in what is taking place, and only then move from those to analyze aggregated, abstract processes. Due to psychological factors—my autobiographic memories—I immediately knew what Foucault meant when he located power in the everyday, and emphasized the relational. A person does not have power while others do not; instead, the power relation requires a certain interplay between the oppressed and the oppressor in order to exist. Foucault's theoretical perspective related to my own understanding of everyday life, which was based on autobiographic experiences. It was therefore much easier for me to understand what Foucault was talking about than it would be for a person without these experiences. I repeatedly experience that otherwise theoretically gifted people struggle to understand Foucault's thinking on this. They probably lack the similar psychological and/or social experiences that I have, and therefore have a less intuitive grasp of what it means to participate in one's own oppression and which consequences follow from doing so. Colleagues have often asked me how a white, heterosexual, cissexual, socially secure male who lives a rather ordinary family life, ends up as a Foucauldian. The reason is to be found in autobiography, and understanding it has been absolutely central for me to make sense of my own choices regarding theory and method.

Post-field Text Situatedness

We have discussed situatedness pre-field. We have discussed situatedness in-field. Then there is post-field situatedness in the text. We both remember our flabbergasted student reactions to some of our own professors when they talked about "just writing up the material". There is no "just" about it, it's demanding and exhausting work. Stuff must come together. And then there is the question of situatedness—what about the writer's self here?

Textual situatedness includes the task of deciding what responsibilities the researcher has towards her informants when it comes to honouring promises about anonymity and non-publication of certain data and making certain that informants will not suffer legal or political prosecution as a result of your publications. Then there is the question of revenge. As we noted when discussing projection above, a researcher usually stacks up a good deal of frustration with informants during fieldwork (see also Sontag 1994; Hartman 2007). The text is *not* the place to take out that frustration direct. If the experience is put to use, it must be done in such a way that the interlocutors are not hung out to dry. This responsibility rests firmly with the author.

Researchers also have a responsibility of seeing to it that they themselves do not come into harm's way, if nothing else then to make the research available. A particularly pertinent example of the latter problematique concerns IR scholars who do work in conflict zones. The United States operates with a so-called terror list, with organizations that are considered to be orchestrating terroristic acts. Any contact with such organizations, say research into their representation of the conflict they are involved in or training in peaceful techniques of conflict resolution, is illegal, and, if discovered, automatically leads to the opening of a criminal investigation. Any researcher working with this kind of material should keep this kind of censorship in mind when authoring texts, so that prosecution of her research and indeed of her may be avoided. This brings up the matter of expediency: There *are* situations when you do not have to, indeed ought not to, tell how the data have been produced.

The question of how to situate oneself in relation to the implied reader comes to the fore when the issue is how to translate a text. A text is a piece of language that does work in a social context, so a translation is by definition not only a displacement of language, but also a displacement of audience. Horses for courses mean that changes are called for. It

is illuminating to consider how the positivist translator into English of a minor classic on the Carolingean Empire in German situates himself:

> The present book has aroused a certain amount of criticism in German scholarly circles because it is customary for German historians not only to be concerned with the disinterested search for historical truth, but also to regard themselves as the guardians of the 'greatness' of the historical past. The English scholar, working in a community whose national life has been less frustrated than that of Germany, is less jealous of guarding the elements of so-called greatness in the history of England; but when reading this book he ought to remember that the author, writing initially for a German public, has, at times, been forced to adopt an apologetic tone when he has been most critical of Charles's achievements. To a reading public accustomed to the tradition of English scholarship, there is less need for the author's insistence that in spite of his obvious shortcomings, Charles ought to be considered great, than there is to a German public. (Munz 1978, p. xxiii)

The aspect of textual situatedness that has been most elaborated over the last half century is, however, the question of who the "I" that writes is, and how that "I" is manifested in the text.

We have already touched on one kind of textual situatedness, namely the one that concerns the ethics of including or excluding field data in texts, as well as the embarrassment or shame that prevents us from reviling in text how our autobiographically primed responses towards something that happened in the field set us on track of a specific analyses. This point may be generalized; the writing process as such is worth a lot of consideration. The first seeds were sown in the late 1960s, when poststructuralists like Roland Barthes and Michel Foucault problematized the death of the author.[1] Poststructural anthropologists followed up in the 1970s by asking who produced anthropological data. The key work here is Paul Rabinow's *Reflections on Field Work in Morocco* (2007 [1977]), where there are very few Moroccans, but ample doses of Rabinow. The poststructural anthropological milieu around Rice University tied these autobiographical concerns firmly to textual situatedness and produced a string of works on the matter, the *locus classicus* being James Clifford and George Marcus's *Writing Culture: The Poetics and Politics of Ethnography* (1987). As Michael Shapiro (1988)[2] formulated the main issue at stake:

> Here is the epistemological rub: the idea that lives are "represented" by an obtrusive, scientifically oriented form of discourse. With this idea, central to a bankrupt version of empiricism, comes a failure to appreciate that biographers are writers who participate in representational practices, and that their texts impose meaning on lives. When one recognizes the existence of these practices, the knowledge problematic shifts from the accumulation of so-called facts about a life to the writing itself. (p. 72)

There is certainly an element of "what happens to me when I write in this and that fashion" in all textual production, and at the outset of the book we discussed how feminists have trail blazed the use of autobiography in the method of memory work. There is also an intersubjective element in textual situatedness. Every writer has one or more implied readers or imagined personae that they expect will read the text. These readers will not coincide with actual readers, for who knows by whom and in which contexts a given text will be read. Still, the more one thinks through who the implied readers may be, the better one situates the text for the implied reader. If you write about Norway in Norwegian, lots of stuff may be taken for granted. When the same material is to be pitched to an English-speaking audience, however, more general contextualization is needed. Such situation of the text is not explicitly visible in the text, as the operation rests with the disposition of the text; yet it is still very much an example of textual situating.[3]

Much research fails because the author has missed or not considered her audience. There are pitfalls on both side of the road, as usual: if you write too implicitly, the text might appear closed, and misses the readers. If you write too explicitly or journalistically, the text may be rejected due to lack of scientific rigour. If the text happens to be accepted for publication, the reader might miss the theoretical implications of the text, since the author is simply implying them. There are considerable differences in national academic tradition. Where an American student who stumbles over a difficult passage will lean towards putting in the extra effort to understand it, others may simply complain that it is too difficult and just give up. In British academic work one may imply a theoretical concept, and in French academic work any more than an allusion may lay you wide open to charges of being banal and obvious. In the American tradition, on the other hand, it seems that everything is supposed to be spelled out at great length. Europeans may easily find such a writing style to betray a lack of subtlety, or worse. If the researcher does not relate to

these questions, the result may be no publication, and if the piece is published, no reception.

Writing style is an aspect of textual situating that is rarely reflected upon in the methods literature. Where is the focus (process or result?); which statements should be highlighted (and how much?); how should it be highlighted (quotations or citations?); and how explicit should the conclusions be (say it or show it?)?

My (Iver's) experiences with writing my second doctoral thesis are a case in point here. Theorist Ian Hacking (2002) discusses modes of knowledge as styles of reasoning. Ontology (which objects compose the known world), conditions for proposing truth claims (epistemology) and "criteria for proof and demonstration" (methodology) influence and colour our styles of reasoning (Hacking 2002, p. 4). My problem was that I came to anthropology as a political scientist, and political scientists have a rather different idea of what "the objects composing the known world" of a social scientist are than do anthropologists. Political science is primarily preoccupied with effects or results of political games. Typical research questions concern who win and who lose in decision-making processes, and why. Anthropology, on the other hand, is more interested in how societies are constituted. This ontological difference—is the world to be treated as a bundle of processes or as a bundle of outcomes?—is tied to differences in how the two disciplines assume different theoretical commitments (understanding vs explanation), as well as different methodologies (phenomenological topics such as reflexivity, on the one hand, versus seeing the research object as a "dependent variable" determined by a "independent variable"—considered constant—on the other).

Hacking's approach is excellent, but it does not focus on what to me was the biggest challenge, i.e. that text situating—i.e. the style of reasoning and the way of presenting—was so different in political science and anthropology. The writing of text assumes very different forms in the two disciplines (Derrida 1985). To write anthropology is very different from writing political science. Karsten, one of my (Iver's) political science students, told me some years back that his first political science professor had written the following advice on the blackboard: "write boringly". The idea was that you should write from an objective point of view, about objective things, in an objective way. The form of presentation is thereby coloured by the fact that you pretend to take up a view from nowhere in the world. Such a way of writing has little space for

active verb forms, and "I", "you" and "me", and is more inclined to passive verb forms, and "we" and "one".

The "view from nowhere" is the antithesis of the situated view. It is also an impossible idea to most anthropologists (see Ardener 1989, pp. 213–214), but not for most political scientists (Kurki 2015). There are therefore many books with titles such as "Writing Anthropology", but none with the title "Writing Political Science" (see however Shapiro 1998). Herein lies the reason why political science often see situating and self-reflection as meaningless or even unscientific activities. Most political scientists conceive of their task as explaining which processes caused which effect in the most objective way possible. If everything aims to support the conclusion in this way, and the conclusion is reached by explanations, it follows that the style of reasoning is itself teleological (oriented towards a given goal, i.e. the conclusion). Focusing on outcomes also leads to the style of writing being concerned with results, i.e. the conclusion, rather than with processes. This means that political scientists perceive of anthropology's ideal, which is to show it rather than say it, or letting the data determine the form of presentation, as completely misconceived. The two disciplines have rather different ideas about textual situatedness. Since these things tend to be taken for granted, social scientists within different disciplines are often not aware of their own situatedness, and so cannot grasp that questions of textual situatedness are historically shaped choices, that they vary, and that one should have a scholarly conversation about the pros and cons of different ways of going about it. Textual situating style has become an effective marker of identity and a mechanism of exclusion. In fact, much learning on both the graduate and postgraduate levels is about disciplining students into a given mode of textual situatedness. Many students will not even notice this, but for me, Iver, who did my master's and doctoral degrees in one discipline after I had been thoroughly inducted into the textual situating ideal of another discipline, it was very clear that I was here being disciplined in the most concrete of ways.

In other words, text situating is something we all do, consciously or not. It is not the case that a text that avoids mentioning the author is not situated, for situating is unavoidable. James Joyce said it well: the authors' role should be as God's in the creation, hidden. That might be, but a hidden author is as much a presence in the text as is an overt author, he is simply better camouflaged. The reader will always be right in insisting that the author is to be found somewhere in there. Since the

reader has a right to know how researchers reach their results, we would insist that it is untenable for a scholar to remain undercover. We implore you: come out! At the very least, noises should be made in the preface. The preface positively invites short reflections on autobiographical and field situatedness and their implications for the text. Very few people are interested in reading an entire book about a researcher and her methods problems, but a lot of people would like to know about stuff that makes for biases in the text which they are about to read.

Notes

1. The key philosophical source on which they drew was Martin Heidegger, as did Hanna Arendt in the quote given at the outset of this chapter.
2. Note the epistemological focus; as did feminists, poststructuralists tended to highlight epistemological and methodological concerns, to the detriment of the perhaps more mundane challenges of method.
3. It is also a good example of what phenomenologists talk about when they insist that any action is informed by the past, which delivers the horizon of the action; the present, when it takes place; and the future, when the expected effects of the action will (or will not) be played out (Sokolowski 2000).

References

Ardener, Edwin. 1989. Remote Areas: Some Theoretical Considerations. In *The Voice of Prophecy and Other Essays*, ed. Edwin Ardener. Oxford: Blackwell.

Arendt, Hannah. 1958. *The Human Condition*. Chicago: University of Chicago Press.

Clifford, James and George Marcus. 1987. *Writing Culture. The Poetics and Politics of Ethnography*. Berkeley: University of California Press.

Derrida, Jacques. 1985 [1978]. The Question of Style. In *The New Nietzsche: Contemporary Styles of Interpretation*, 2nd ed., ed. David B. Allison, 176–189. Cambridge, MA: MIT Press.

Dottolo, Andrea L., and Sarah M. Tillery. 2015. Reflexivity and Feminist Interventions. In *Reflexivity and International Relations. Positionality, Critique and Practice*, ed. Jack L. Amoureux and Brent J. Seele. London: Routledge.

Hacking, Ian. 2002. *Historical Ontology*. Cambridge, MA: Harvard University Press.

Hartman, Tod. 2007. Beyond Sontag as a Reader of Lévi-Strauss: "Anthropologist as Hero". *Anthropology Matters* 9 (1): 1–11. http://www.anthropologymatters.com/index.php/anth_matters/article/viewFile/62/120. Accessed 20 June 2017.

Kurki, Milja. 2015. Stretching Situated Knowledge: From Standpoint Epistemology to Cosmology and Back Again. *Millennium: Journal of International Studies* 43 (3): 779–797.

Langless, L[ewis] L., and Gelya Frank. 1981. *Lives: An Anthropological Approach to Biography*. Novato, CA: Chandler & Sharp.

Leach, Edmund. 1982. *Social Anthropology*. London: Fontana Paperbacks.

Lowenthal, David. 1985. *The Past is A Foreign Country*. Cambridge: Cambridge University Press.

Mauss, Marcel. 1936. Les techniques du corps. In *Sociologie et anthropologie, 5e édition*, 363–386. Paris: Quadrige/PUF (Presses Universitaires de France).

Munz, Peter. 1978 [1968]. Translator's Introduction, pp. ix–xxiv in Heinrich Fichtenau. In *The Carolingian Empire*. Toronto: University of Toronto Press.

Rabinow, Paul. 2007 [1977]. *Reflections on Field Work in Morocco*, 3rd ed. Berkeley: University of California Press.

Shapiro, Michael J. 1988. Reading Biography. In *The Politics of Representation: Writing Practices in Biography, Photography, and Policy Analysis*, 55–82. Madison, WI: University of Wisconsin Press.

Sjørslev, Inger. 1988. Deltakelsens dilemma, et brasiliansk offer. In Hastrup, Kirsten and Kirsten Ramløv (eds). *Feltarbejde: Om opplevelse og metode i etnografien*. Copenhagen: Akademiske Forlag. 151–164.

Sokolowski, Robert. 2000. *Introduction to Phenomenology*. Cambridge: Cambridge University Press.

Sontag, Susan. 1994 [1963]. The Anthropologist as Hero. In *Against Interpretation*, 69–81. London: Vintage.

Starobinski, Jean. 1971. The Style of Autobiography. In *Literary Style. A Symposeum*, ed. S.B. Chatman. New York: Oxford University Press.

CHAPTER 7

Philosophy of Science: Two Ways of Going About Situatedness

Abstract This chapter ventures into the philosophy of science, and so is harder going than the rest of the book. We want to demonstrate that there are two basic ways of thinking scientifically about situatedness. The dominant one is the one that Jackson (2011) calls reflexivist. We will proceed to discuss this position and juxtapose it with another one that Jackson calls analyticist. The analyticist position relates to situated research not primarily through awareness of changes in the researcher self, like in the reflexivist position, but through awareness of the changes the researcher produces in his relations within fields. We start with a brief summary of reflexivism and engage with two basic criticisms of it, go on to present the analyticist position and end by comparing them.

Keywords Situatedness · Reflexivist position · Analyticist position Ontology · Epistemology

The point of departure in this book was that data production is intersubjective. We also stressed how it is the researcher, and not her interlocutor, who is in charge of the research process, for she is the one with the fullest information on where it is going. It is, after all, her process. This means that the researcher makes things (among them, texts) happen that would not otherwise have happened. In other words, she is, other things being equal, at the empowered side of the relation with her informants where scientific discourse is concerned.[1] One response to this fact is to

C.B. Neumann and I.B. Neumann, *Power, Culture and Situated Research Methodology*, DOI 10.1007/978-3-319-59217-6_7

ask questions like "which are my responsibilities?" "what happens to me as a human as I do this research?" "what happens to me as a researcher when I write this text?" We have already touched on the scientific preconditions that inform such an approach, when we noted how feminists like Sandra Harding place what Jackson (2011) calls a reflexive ontological wager by positing that there are structures out there that spawn phenomena like humans. We may find out which these structures are by thinking of ourselves as effects of those structures. The way to produce such data, that is, the method to use, is to trace changes in the self, be that by autobiographic memory work or by tracing how research experiences change us pre-field, in-field and post-field.

The reflexivist position has engaged two key objections. The first is that it presupposes a stable or fixated self that can be thoroughly known and recollected. The second is that the activity of doing autobiographic situatedness in reflexive research may decay into subjectivism (Skeggs 2002; Reed 2010; Hamati-Ataya 2014; see also Harding 2015, Chap. 2).[2]

To us, the objection against the stable self is valid. We commit as poststructuralist, which means that we understand the self as something that is in constant flux, not something that is stable and can be known once and for all. However, the fact that we cannot have a stable self through time and cannot know ourselves once and for all, does not mean that we cannot know ourselves at all. Our awareness of ourselves in the here and now, as well as in the past, and of our life stories and give us what we may call a narrative identity, however imperfect and partial, may still be the king's road to analyses in any or all of the three stages of the research process. For reasons that we discussed at length in Chaps. 1–2 under the heading strong objectivism, doing this work, while staying focused on the research context and the tasks at hand, does not mean that the research has to collapse into subjectivism. It may imply conducting ethically sound research by making relations of power-knowledge transparent (Lynch 2008, p. 716; Brigg and Bleiker 2010, p. 758; see also Justesen and Mik-Meyer 2012, p. 46).

The reflexive wager is not the only kind of ontological wager that invites self-situatedness, however. There is more than one way to use the self in research, for there is more than one way of hooking up to the world and choose one's criteria for proof and demonstration.[3] One way is the reflexivist way just charted, which results in the method of tracing changes in your own sense of self as a tool for analytical entries and

in order to induce structures. Another way is what Jackson (2011) calls the analyticist way. The analyticist, who may have varying specific (and internally conflicting) theoretical attachments such as Weberian, pragmatic or Foucauldian ones, does not focus on himself in order to induce the nature of structures existing out there, such as patriarchy, for he believes that a timeless structure such as patriarchy simply do not exist. He instead focusses on coming to terms with himself as an instrument of data production relative to the social world of which he is a part, be that in terms of forging an ideal type based on a value judgment, in terms of why he thinks a certain phenomenon constitutes a problem or in other relevant ways.

This difference speaks directly to poststructuralists like us, for poststructuralism is defined by its break with structuralism exactly over the ontological issue of whether structures exist independently of human action. Structuralists thought they did, which meant that research became a question of producing data about visible or manifest structures, with the intention of identifying the invisible or latent structure that could not be observed directly, but that spawned and held together the manifest structures (Dosse 1997). The break with this way of thinking came when people like Foucault suggested that latent structures simply did not exist. Manifest structures were not anchored in anything, and they did not necessarily hang together logically. The entire reason why Foucault hatched concepts like episteme, discourse and dispositif was to find alternative ways of thinking about how phenomena such as socially situated humans emerge. So, Foucault foreshadows theorists like Harding and Dauphinee in holding that humans are produced by the social. He went as far as defining critique as the art of not being governed so much, a statement that we think may be paraphrased to read that the work of situating yourself by intellectual means is a creative technique of the self. But Foucault would not agree that what produces selves is given beyond discourse. It follows that Foucault simply could not trust introspection to identify the structure that spawned him, for to Foucault, his own interpellation into or resistance to, not a structure in the singular, but to discourses in the plural, is co-constitutive of those discourses. The social does not arise out of something that is already given, it creates itself.

Note that Foucault still definitely situates himself as a researcher, but his situatedness is of a different kind. As a young man, he experienced psychological turmoil (Eribon 1992). He wrote about the birth of a

clinic. His sexual orientation was sado-masochistic homosexuality (Miller 1993). He wrote about the history of sexuality. Nowhere, however, will one find Foucault using the reflexive methods of memory work or of introspection with a view to following changes in his own emergent self. Jackson (2011) sums up the difference as follows:

> An analyticist might articulate a value-commitment to beginning with everyday understandings and proceed to elaborate an ideal-typical model of everyday understanding […], but that would not make her work reflexivist because the warrant for her claims would not simply be the fact that they were connected to a social group's common-sense practices but would instead be the fact that they were connected to an ideal-typical model rooted in a particular value-commitment. (p. X)

In Foucault, this value commitment is on behalf of himself and/or specific groups. He defined his work as critiques, and defined critique as the art of not being governed so much. Situatedness was about picking out some historical sequence where something that had before been considered doxic or normal, becomes problematized, with a view to understanding the consequence for some group of which the researcher may, or may not, be a member. The value commitment that situates the researcher is towards understanding what the effects of categorizing and representing things like this rather than that does to a certain group of people, such as eighteenth century French hermaphrodites, nineteenth century French murderers or twentieth century French manic depressives.

Ontological commitments have methodological consequences. The reflexive way of doing situating is to ask what latent structure and immediate context do to me as a researcher. Reflexivists often write as if they have a monopoly on situatedness. For example, in the fullest IR treatment of methodological (as opposed to methodic) autobiographical situatedness to date, Brigg and Bleiker (2010) writes about one of us (Iver) that:

> Neumann uses his dual role of participant and researcher to scrutinize taken-for-granted entities [within a Foreign Ministry], such as the individual or the state. Yet while Neumann draws directly on his personal experiences to offer valuable insights, he does not explore the methodological quandaries accompanying his research. We learn little, for instance, about

> how Neumann negotiated his position as both the subject and object of research, as both knower and part of the empirical world under investigation. Add to this that Neumann, as any other author, is not a stable and given 'entity', but a person whose sense of self and whose knowledge of the world is constantly reshaped by historical, cultural and political influences. How might we draw upon the self and evaluate the resulting research in such circumstances? (p. 785)

This is a fair critique. Still, it is not accurate that I did not account for how I changed. The whole article is about how I learnt that the main point of Foreign Ministry speeches was not to convey a specific message, as I had always thought, but rather to give voice to the Foreign Ministry as such and so confirm the Ministry's *own* identity. Learning changes you. I learnt, and I changed. But this is not, I think, the kind of change Bleiker and Brigg have in mind. I would guess that what they have in mind is rather what all my failing efforts to have my drafts for speeches rejected did to me at a more aggregate level, that is, how it changed my view of bureaucracy and the state as such. OK dudes, it did. I was not much of a state fan to begin with, and after 5 years in the Foreign Ministry, I was even less so. After working in the Ministry, I certainly thought less about the road not taken, which had indeed been to become a diplomat. That road was cut off by my affair with Olga while I was posted to Norway's Moscow embassy as a guard and interpreter in 1980, which cost me my security clearance. On the other hand, and in counter-distinction to this, I also developed a new appreciation for those who man the state. As any anthropologist worth his salt, I learnt to love my tribe. All this is potentially relevant for a book on method, for if I had been a reflexivist like Cecilie, and also like Bleiker and Brigg, I could have used these experiences of having state structures working on me to say something about those structures themselves. There seems to be plenty to say: states tear people apart, the body of a state employee is not his own, etc.

So, why did I not undertake this particular task? The background to the answer is my ontological commitment. I am an analyticist. I do not think that state structures, or patriarchy, or any other kind of structure, exist separately from social interactions. I am not a methodological individualist either, I certainly do believe that there exist structural forces that shape us, but I think that these structures exist as relations between humans; structures reside *in* those relations and have no existence

separate from them. So for me, there are only relations. If there is no such entity as a structure outside those relations, there is no structure that may create phenomena like humans, and there is no point in looking for the effect of such a thing. The creative force must be the social relations themselves, and the focus must be how they make us emerge as humans (out of the given raw material that is our bodies, of course). That means that there are two possible foci for the researcher; what happens to the researcher in a certain process, or what happens to his interlocutors. I chose to focus on the interlocutors.

This choice does not in and of itself explain why I did not also focus on what happened to me, for Brigg and Bleiker are, as I have tried to demonstrate, absolutely right that I could have situated myself even better in the text by wheeling out the stuff that I have now wheeled out. The answer is that my primary object of research was not me, but my interlocutors. My story of speechwriting in the Foreign Ministry is one of how I actively used my relationship with my interlocutors to produce situations that exposed how they usually do things—that is, how they recreated status quo ante by living down my irregular actions and strange suggestions. I simply made the pragmatic choice of focusing on the Other rather than on myself. Too much detail about me, myself, I would have detracted attention from what I saw as the major task. It was, in part, a question of economy of textual situatedness, but first and foremost it was a direct consequence of an ontic commitment to an ontological position. I explored the methodological quandaries accompanying my research all right, but as a Foucauldian, I did not do that reflexively, but analytically. Ontological and methodological commitments have methodic consequences. An analyticist draws upon the self not by the method of introspection, as Brigg and Bleiker exhorted me to do in the passage just quoted, but by drawing on autobiography and situatedness in forming value commitments. Those value commitments spawn research questions, which inform data production or method.

The key methodic consequence is that reflexivists and analyticists approach data production from opposite ends of the researcher/informant relationship. Where a reflexivist researcher tends to handle the relation between interlocutor and researcher by asking how interlocutors affect her, an analyticist researcher tends to ask how the researcher affects them. Where the reflexivist is primarily self-regarding, the analyticist is primarily Other-regarding. For example, in Dauphinee's (2013) work, the accent is on how her main informant,

Stojan Sokolović, induced changes in her. Memory work is about how previous social settings affected the researcher's former selves. By contrast, the reader will probably have noted already, and if not he will note it if he goes back and re-reads this text, that most of Iver's examples concern how the researcher affects the informants. The reason for this is not only ethical, but analytical. Our specific value commitments in a scientific undertaking are linked to our general value commitments, and those commitments define us as selves. They are inscribed in our bodies and shine through in the way we dress and the way we speak, as well as in our comportment. They are, therefore, not only eminently present when we produce data, but constitutive of those data. It stands to reason that an analyticist must monitor how he constitutes data. The focus is, consequently on how the other reacts to what we do, and not on how we react to what the other does. Put differently, if we draw on George Herbert Mead's old distinction between the I (my experience of self) and the me (others' experience of my self), then the reflexivist's method fastens on following changes in the I, while the analyticist's method fastens on following changes in the me. These are two different ways to skin a cat, two different uses of the self in research. To ask which is better is a moot question, for they are the result of different ontological wagers, and designed to do different research jobs.

Any difference may be dedifferentiated. The reflexivist/analyticist distinction is no exception. We are particularly well placed to do this, since Cecilie is more of a reflexivist and Iver is more of an analyticist, which means that we have been quarrelling about how to frame a number of examples given here throughout the gestation of this book.[4] Note that Jackson's reflexivist has placed Cecilie's ontological wager on there existing structures beyond the social. Note also that poststructuralism's break with structuralism fastened exactly on the ontic status of structure. Poststructuralists are post exactly because they did not place their ontological wager on the existence of latent structures. It followed that they could no longer hook up to the world by studying manifest structures that would lead them to latent ones, for what is the point of chasing something you have placed a wager on not being there in the first place. The solution for poststructuralists was to substitute other concepts, concepts that were explicitly social and malleable, for the asocial and deterministic concept of structure. The most well known of these is discourse (and here we have the reason why Foucault must be categorized as an analyticist within this particular scheme). For poststructuralists, subjects

are produced by discourses, but discourses are not asocial and deterministic. On the contrary, they will by definition change over time. If we approach situatedness in this way, the methodic possibility opens up of combining the study of the I and the me, for the logic of discourses may be learnt, and relationist analysis accomplished. There is a dedifferentiation of the reflexivist and analyticist positions.

The better work the researcher does on situating herself in relation to her field of study, the better the quality of the ensuing data. The better the data, the better the texts that constitute the result of the research. The methodology of situatedness is a crowded shelf. For this pragmatic reason, we have targeted not methodology, but method. We have stressed the importance of doing memory work in the pre-field phase and to keep on doing so in the field. Choice of field, choice of research question and choice of theories are often tied to autobiography. Cognition in the field happens as an active process, where our categories help us grasp wholenesses or gestalts of data. The better the researcher knows her categories and where they come from, the higher the awareness in the field, and the better the data produced. Insights into modes of cognition, such as confluence and projection, also help. During the writing phase, there is ethical question to consider when decisions are made about what to keep in and what to leave out, and there is the question of how the presence of the researcher should be presented. Should she follow James Joyce's adage and be in her text as God in creation, namely hidden, should she give a succinct presentation about the whys and hows of research choices in the preface, or should she mark a running presence throughout the text?

The most important answer to this question is to be found in what kind of ontological commitments the researcher brings to the writing. If the researcher thinks of method as a question of producing data about how she is a product of a structure, then the thing to do is to trace how the research changes her self, for these changes are effects of structure, and so a study of them will tell us something about that structure. Situating oneself becomes a question of discussing how interlocutors and research context change the researcher's self. If, on the other hand, the researcher thinks of method as a question of producing data by bringing certain value commitments with her into the field, ask questions based on those value commitments and produce data out of the resulting answers, then that is a question of tracing how the self effects change

in the informants. The relationship between researcher and informant is approached from the other end.

Analytically, then, there are at least two basic ways of using the researcher self when going about the work of situating oneself in relation to the research process. The data-producing techniques that we have discussed here should come in handy for both approaches, and both approaches seem to share a very old professional ethos: researcher, know thyself.

Notes

1. Unless the researcher interviews other and more senior researchers. If we include other discourses than the scientific one, this becomes an empirical question, where the situatedness of the specific informant (disaster victim? Aid worker? Prime minister of donor country?) as well as the character of the general context (colonial? Neo-colonial? War?) weigh in heavily.
2. Note that the question of the self's stability also pertains to our interlocutors and how you present them in the text. Here is an example: Beverley Skeggs (2002) has argued powerfully against the indication of the stable self in reflexivity, on both ethical and theoretical grounds. When she situated herself in opposition to her parents in *Formations of class and gender* (1997), her parents were, she reports, not only hurt, they also felt that the implication of the way she situated herself as the product of a class journey fixated them in time and space (Skeggs 2002, p. 319). She had moved on, while they remained the same. The ethical challenges of autobiographical situatedness not only apply to how we depict our interlocutors in relation to ourselves (we should be aware not to hurt our informants), as in Skeggs' example, but also to how we let our own stories overshadow the knowledge we have coproduced with our interlocutors. Holstein and Gubrium articulates this risk of subjectivism in reflexive research and as a caution against letting the hows of research replace the whats of lived experience (1997, p. 115).
3. A number of texts on autobiography in IR, such as Brigg and Bleiker (2010) and Vrasti (2008), imply that reflexivity is the only scholarly way of situating oneself. This monological view is criticized not only by Jackson (2011), but also by Rancatore (2010) and Sande Lie (2011).
4. In a similar vein, Hamati-Ataya (2012, p. 689, n. 4) notes how Colin Wight and Patrick Jackson find Bourdieu to be an analyticist and a reflexivist, respectively. To us, Bourdieu is a particularly good example of positivism gone mad: Bourdieu purports to 'objectify' himself in order to gain a privileged position 'outside' discourse from which he Marxist style may tell everybody else how

they are 'objectively' situated in this or that fashion and cannot get out of it, only Bourdieu himself can; compare Mérand and Pouliot (2008).

References

Brigg, Morgan, and Roland Bleiker. 2010. Autoethnograpic International Relations: Exploring the Self as a Source of Knowledge. *Review of International Studies* 36 (3): 779–798.

Dauphinee, Elizabeth. 2013. *The Politics of Exile*. London: Routledge.

Dosse, Francois. 1997. *History of Structuralism: The Rising Sign, 1945–1966*. Minneapolis: University of Minnesota Press.

Eribon, Didier. [1989] 1992. *Michel Foucault*. London: Faber & Faber.

Hamati-Ataya, Inanna. 2012. Reflectivity, Reflexivity, Reflexivism: IR's "Reflexive Turn"—And Beyond. *European Journal of International Relations* 19 (4): 669–694.

Hamati-Ataya, Inanna. 2014. Transcending Objectivism, Subjectivism and the Knowledge In-between: The Subject in/of "Strong Reflexivity". *Review of International Studies* 40: 153–175.

Harding, Sandra. 2015. *Objectivity and Diversity. Another Logic of Scientific Research*. Chicago, IL: University of Chicago Press.

Jackson, Patrick. 2011. *The Conduct of Inquiry in International Relations: Philosophy of Science and Its Implications for the Study of World Politics*. London: Routledge.

Justesen, Lise, and Nanna Mik-Meyer. 2012. *Qualitative Research Methods in Organisation Studies*. Copenhagen: Hans Reitzel.

Lie, Jon Harald Sande. 2011. Challenging Anthropology: Anthropological Reflections on the Ethnographic Turn in International Relations. *Millennium: Journal of International Studies* 41 (2): 201–220.

Lynch, Cecelia. 2008. Reflexivity in Research on Civil Society: Constructivist Perspectives. *International Studies Review* 10: 708–721.

Mérand, Frédéric, and Vincent Pouliot. 2008. Le Monde de Pierre Bourdieu: Éléments pour une Théorie Sociale des Relations Internationales. *Canadian Journal of Political Science* 41 (3): 603–625.

Miller, James. 1993. *The Passion of Michel Foucault*. New York: Simon & Schuster.

Rancatore, Jason P. 2010. It is Strange: A Reply to Vrasti. *Millennium: Journal of International Studies* 39 (1): 65–77.

Reed, Isaac Ariail. 2010. Epistemology Contextualized: Social-Scientific Knowledge in a Postpositivts Era. *Sociological Theory* 28 (1): 20–39.

Skeggs, Beverley. 1997. Formations of Class and Gender. London: Sage.

Skeggs, Beverley. 2002. Techniques for Telling the Reflexive Self. In *Qualitative Research in Action*, ed. Tim May. London: Sage.

Vrasti, Wanda. 2008. The Strange Case of Ethnography and International Relations. *Millennium: Journal of International Studies* 37 (2): 279–301.

CHAPTER 8

Conclusion: Culture, Power, Ethics

Abstract Knowledge production is always local and contextual, even where it aims beyond the specific towards the general. In order to produce the best data possible, the researcher must know as much as possible about context. Part of that context she brings to the field herself. In order to produce the best possible data, the researcher should know this context as well as possible, and reflect on her pre-field autobiographical situatedness, her in-field situatedness and her post-field textual situatedness. We will conclude this book by focusing on two of the more general contextual issues that have arisen en route, namely the significance of culture and power for the relation between researcher and informant.

Keywords Culture · Power · Ethics · Situatedness

In order to produce the best possible data, the researcher should also reflect on the broad social and political context in which information with informants take place.

Culture

Humans are relational, communicative and able to learn. Due to a variety of factors, these capabilities are unevenly distributed. The resulting discrepancies mean that relations between humans are always saturated with power. Certain knowledge is socially superior to other knowledge.

C.B. Neumann and I.B. Neumann, *Power, Culture and Situated Research Methodology*, DOI 10.1007/978-3-319-59217-6_8

Social position is manifested in the body as habitus, and further accentuated by dress and manners of speech. The resulting discrepancies between researcher and interlocutor structure research practice.

It is a precondition for social research that these differences are not so insurmountable that social communication is impossible or that it is impossible to generalize about social life. At some level, homo sapiens are all made of the same basic material. Humans have a physiological and psychological unity that makes comparison and generalization possible. One should be wary of the idea that people are the same regardless of social and cultural history and context. Still, there are certain physiological and psychological regularities, such as the need for food and sleep, for care and sex. Social factors may change these intersubjective physiological and psychological origins dramatically. Through conditioning and training, it is possible to endure pain, tolerate lack of sleep, get used to surviving without care, and so on. The result is individual diversity. Our social systems present ideals and ways that generate cultural diversity. That does not mean, however, that there is no common origin. Where certain eighteenth century aristocrats, nineteenth century racists and twentieth century feminists saw themselves as completely different from the rest, social scientists insist on what we may call a democratic starting point. There are bodily differences, but that does not alter the unity of the species. When Köhler first conducted experiments on gorillas, and then on his daughter, he actually went one step further. He posited, and proved through his experiments, that there is a common origin for all primates.[1]

If humans are fundamentally similar, and if some of this similarity concerns needs that only other humans can satisfy, then humans must also be social beings. One cannot be human all alone. Humans need other humans to live. According to the idea that we share certain common origins also with other mammals it may be that other mammals (e.g. dogs, horses, cats) may be substituted for humans, but that still means that we stay in a situation of what is called functional equivalents, that is, we are talking about stand-ins that fulfil the same functions or tasks. There is one task that cannot be substituted, though, and that is talk. Other mammals may know how to communicate, but not verbally. To a social scientist, whose prime tool is language, the capacity to create a human relation through verbal communication is nothing less than an ontological imperative.[2]

Humans can communicate and learn, and learning is a social activity. Since people live in societies, and each society is dependent on shaping their members in order to create a minimum of societal coherence, it follows that much learning deals with socialization, that is, the interpellation into practices considered normal within a certain group. One of many definitions of culture is precisely the total set of abstract norms and rules defining how things are, must be and should be (Kuper 2000).

Two challenges follow for the researcher. First, she ought to have established what "adequate socialization" means to the people she talks to. Second, given the cultural difference between the researcher and the interlocutor, she ought to consider the extent to which the interlocutor "is" her culture, and whether this should be respected. We know that socialisation is overall quite successful; otherwise societies would be impossible, which is evidently not the case. We are subsequently faced with a challenge: on the one hand we assume that the entire human species shares common physiological and psychological origins. On the other hand, we assume that humans have the capacity to learn, which implies that humans that grow up in different societies will be different. Where does the universally human start, and the culturally specific begin? We will not discuss the pros and cons of political cultural relativism here, but simply note that from a scientific as opposed to a political point of view, cultural relativism is a methodological necessity. It is obvious that an interview based on disinterest or prejudice will be a bad interview, not only because it is ethically dodgy, but because it will not maximize the researcher's chances of producing the best data possible. That cultural relativism is methodologically necessary tells us nothing about what kind of values a specific researcher attributes to her or others' culture. The methodological—what to do to get to the data—and the political—opinions on relative rights and duties among groups—are logically two clearly separate questions. There are a number of examples of social scientists who have produced good data by being culturally relative in the field, while engaging at the same time in political campaigns against the very practices or even groups that they research. What we now take a closer look at is what we called methodological cultural relativism, the attempt to understand what informants talk about and to produce data about them that keeps the importance of the researcher's autobiography and field situatedness at a minimum.[3]

If researchers stay inattentive to these challenges they will easily apply faulty assumptions in conversation with informants with a different

cultural background. A common method challenge lies in the reading of body language. Where a Norwegian person indicates calming down with an open palm, a Greek will take the same palm as an insult. Receiving something with one's left hand is an insult to Arabs. A firm handshake indicates sexual interest in South East Asia. While wide-open eyes are an expression of surprise in Norway, it is an expression of anger in China and to Maori in New Zealand. The list goes on. Human beings everywhere tend to smile when they are happy and cry when they are sad, and this is correlated to the common physiological and psychological make-up of humanity. Still, social forces may intervene. In South East Asia, it is common to smile in order to cover up less happy feelings, for example, when receiving a death message, and it is practice in several parts of the world to engage professional weeping women during funerals.

From this follows another challenge: every researcher must necessarily have a set of categories for what is "normal" and what is "good" in order to conduct her work. What do we do when these categories are fundamentally different from those of the interlocutor? In Japan, it is common that a married women has warm food ready when her husband arrives from work, and that she waits for him even if he comes home long after midnight. Such behaviour would strike a Norwegian researcher as what the Gestalt tradition calls confluent (the woman lives for her husband and is without clear independent boundaries) and perhaps introjective (she does it because she thinks she ought to do it). Politically, most researchers would probably call such behaviour submissive or perhaps self-exterminating. A similar example is Indian women's relationship with their mothers-in-law. Most Indian women put up with considerably more from their mother-in-law, than what Norwegian mothers-in-law would be allowed in similar situations. The threshold against culturally accepted mother-in-law behaviour is simply much higher in India than in Norway. Unless a Norwegian researcher interviewing Indian mothers and daughters-in-law is fully aware of this, she may apply her own Norwegian premises and assumptions for the data production, and thereby potentially create political results of little scientific value.

We must not go to Japan or India to encounter this type of dilemmas. In Scandinavia, it is considered culturally problematic to discuss class differences, and yet they are everywhere. There are myriads of big and small cultural difference between classes. They may create challenges if the researcher and interlocutor have different class backgrounds. They cannot easily be transcended or suspended, but can be changed from

being potential sources of error to being sources of insight through the researcher's situating effort.

Power

Here is an insurmountable problem: regardless of the extent to which the interlocutor participates in shaping the path of the interview and decides how fast to walk along it, the researcher will be the one who commands the situation and who knows how she has planned for the interview to proceed. The interlocutor is expected to have empirical knowledge of some kind that the researcher does not have, for if this is not the case, then there is no point in carrying out the interview. Still, the researcher will always know the terrain better and thereby be ahead, since it is the researcher who defines the terrain in its scientific aspect, while most interlocutors have not even entered this kind of terrain before. The challenge in the relationship between researcher and informant is that as long as the researcher knows more about their relation and its purpose, the relation cannot be free of power.

This problem has no solution. Michel Foucault emphasizes how power comes from everywhere, and therefore that it cannot be removed from human relations. Relations between researcher and informant are no exceptions in this regard. In fact, they may be particularly exposed to this phenomenon. What the research can do, is to moderate the power aspect of the relation as much as possible.

Several times throughout this book we have mentioned the responsibility within the interview situation. We have pointed to the significance of listening to the informant and to be alert to his body language. Cecilie explained in Chap. 2 how she interpreted a vulnerable informant's body language as a set of warning signs. In such cases, it is imperative for the researcher to tread carefully. Many informants have probably felt offended or intimidated in an interview situation, and even more informants have probably been in for a surprise if they came across the results of the research in a scientific journal or, much more probably, in a newspaper or a magazine. Many researchers have tales about informants who do not recognize the analyses of their workplace, profession or institution. In some cases, informants may strike back against researchers they believe have misunderstood or misinterpreted them. Much as we should have liked to, we cannot provide researchers reading this book with an insurance against angry informants. Once again, the point to make is

simply that the chances of something like this coming out of the blue may be minimized when the researcher has done her situating work, and so is better able to identify sources of possible future friction.

The research process is fundamentally coloured by the researcher's own situatedness. We have attempted to clarify a number of challenges, and suggested steps that may be taken in order to minimize challenges and produce better data. The general exhortation, that by reflecting on who we are, we may also understand more about others, is hardly original. On the contrary, it goes back to the origins of the Western scientific tradition. *Gnothi seauton*—researcher, know yourself!

Notes

1. There is a line to be drawn from these pioneering experiments to present-day neuroscience, which emphasizes the similarity between the human brain and that of other mammals, see Lakoff and Johnson (1999).
2. Fritz Perls, a central 1960s name in the Gestalt tradition, had in the early part of his life an illustrative experience of what could happen if this imperative dissolved. Perls went in 1928 to the Freudian Eugen Happel for psychoanalysis. Happel, however, was nearly mute. Perls later wrote that Happel wouldn't speak for weeks. He would indicate things such as the end of the session by scratching the chair against the floor (Clarkson and Mackewn 1993, p. 11). The result was that Perls thought that he did something wrong and blamed himself that Happel didn't understand what he tried to communicate. It later turned out that Happel suffered from an advanced version of paranoia. This is not the place to discuss what such a diagnosis implies. Our point is to illustrate that when a human reaches a certain rock-bottom level of communication, it breaks with our expectations of what a human being is, and therefore appears as pathological. Humans' relational nature is first visible, as are so many other things, when it is suspended.
3. It should be eminently clear by now that we do not think this work may ever be brought to an end. The job is not to do ourselves in, but to identify stuff that comes in the way of good research due to a lack of self situatedness on the researcher's part, and which is therefore avoidable.

References

Clarkson, Petruska, and Jennifer Mackewn. 1993. *Fritz Perls*. London: Sage.
Kuper, Adam. 2000. *Culture*. Cambridge, MA: Harvard University Press.
Lakoff, George, and Mark Johnson. 1999. *Philosophy in the Flesh. The Embodied Mind and Its Challenges to Western Thought*. New York: Basic.

Bibliography

Ash, Mitchell G. 1996. Emigré Psychologists after in 1933: The Cultural Coding of Scientific and Professional Practices. In *Forced Migration and Scientific Change: Emigré German-Speaking Scientists and Scholars after 1933*, ed. Mitchell G. Ash and Alfons Söllner, 117–138. Cambridge: Cambridge University Press.

Berger, Peter, and Thomas Luckman. 1966. *The Social Construction of Reality: A Tretise in the Sociology of Knowledge*. London: Penguin.

Bourdieu, Pierre, and Loïc J.D. Waquant. 1992. *An invitation to reflexive sociology*. Chicago: Chicago University Press.

Dauphinee, Elizabeth. 2010. The Ethics of Autoethnography. *Review of International Studies* 36 (3): 799–818.

Denzin, Norman, and Yvonna S. Lincoln (eds.). 2011. *The SAGE Handbook of Qualitative Research*. Los Angeles: Sage.

Etherington, Kim. 2004. *Becoming a Reflexive Researcher, Using Ourselves in Research*. London: Jessica Kingsley.

Jackson, Patrick. 2011. *The Conduct of Inquiry in International Relations: Philosophy of Science and Its Implications for the Study of World Politics*. London: Routledge.

Marcus, George. 1998. *Ethnography Through Thick and Thin*. Princeton, NJ: Princeton University Press.

Mauss, Marcel. 1993 [1936]. Les techniques du corps. In *Sociologie et anthropologie, 5e édition*, 363–386. Paris: Quadrige/PUF [Presses Universitaires de France].

Merleau-Ponty, Maurice. 2004 [1945]. *Phenomenology of Perception*. London: Routledge.

Mol, Annemarie. 2008. *The Logic of Care*. London: Routledge.

C.B. Neumann and I.B. Neumann, *Power, Culture and Situated Research Methodology*, DOI 10.1007/978-3-319-59217-6

Neumann, Cecilie Basberg. 2016. Children's Quest for Love and the Professional Child Protection Worker: The Case of Norway. *Scottish Journal of Residential Child Care* 15 (3): 104–123.

Neumann, Iver B. 2007. A Speech that the Entire Ministry may Stand for Or: Why Diplomats Never Produce Anything New. *International Political Sociology* 1 (2): 183–200.

Neumann, Iver B. 2010. Autobiography, Ontology, Autoethnology. *Review of International Studies* 36: 1051–1055.

Ruane, Abigail. 2011. Pursuing Inclusive Interest, Both Deep and Wide: Women's Human Rights and the United Nations. In *Feminism and International Relations. Conversations about the Past, Present and Future*, ed. J. Ann Tickner and Laura Sjoberg, 48–67. London: Routledge.

Shaw, Julie, and Andrew Kendrick. 2016. Reflecting on the Past: Children's Service Workers' Experiences of Residential Care in Scotland from 1960 to 1975. *British Journal of Social Work* 0, 1–17.

Sikes, Pat (ed.). 2013. *Autoethnography*. London: Sage.

Skeggs, Beverley. 2008. The Dirty History of Feminism and Sociology: Or the War of Conceptual Attrition. *The Sociological Review* 56 (4): 670–690.

Starobinski, Jean. 1971. The Style of Autobiography In *Literary Style, A Symposeum*, ed. S.B. Chatman, 285–296. New York, NY: Oxford University Press.

Staunes, Dorthe, and Dorthe Marie Søndergaard. 2010. Who is Ready for the Results? Reflections on the Multivoicedness of Useful Research. *International Journal of Qualitative Studies in Education* 21 (1): 3–18.

Szakolcsai, Arpad. 1998. *Max Weber and Michel Foucault: Parallel Life-Works*. London: Routledge.

Index

C.B. Neumann and I.B. Neumann, *Power, Culture and Situated Research Methodology*, DOI 10.1007/978-3-319-59217-6