

## Cecilie Basberg Neumann and [Iver B. Neumann](#) Uses of the self: two ways of thinking about scholarly situatedness and method

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# Uses of the Self: Two Ways of Thinking about Scholarly Situatedness and Method<sup>1</sup>

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## Abstract

If the scholarly self is irretrievably tied to the world, then self-situating is a fruitful source of data production. The researcher becomes a producer, as opposed to a collector, of data. This how-to paper identifies three analytical stages where such self-situating takes place. Pre-field; there is autobiographical situating; in-field, there is field situating, and post-field, there is textual situating. Each of these stages are presented in terms of the three literatures that have done the most work on them -- feminism, Gestalt, and poststructuralism – and a number of how-to examples. We illustrate with a number of how-to examples. In conclusion, we discuss how two different methodological commitments to situatedness, which Jackson (2010) dubbed reflexivist and analyticist, give rise to two analytically distinct ways of using the scholarly self for data production. 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.

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## Introduction

This is first and foremost a how-to paper on method. We ask how researchers may situate themselves for their data production to be maximally effective. *Situs* is Latin for 'site', a place where something happens. The English expression 'you must understand my situation' moves the attention away from place, towards how a certain individual is related to place. Being situated means being mindful or aware of the relationship between oneself and one's context. We argue that, since research is intersubjective, and since it is the researcher who initiates and takes charge of the research process in order to produce as reliable data as possible, the more the researcher knows about why she has chosen to attempt data production about phenomenon X rather than Y, how she goes about producing that data and how she produces her stories about X, the better data, and the better texts. We draw on extant scholarship and identify three analytical stages that demand different kinds of situatedness. Stage one is pre-data production and concerns how the scholarly self came to pick the research issues and theories that resulted in this and that research question and not others. Stage two concerns data production: how does the scholarly self shape the context (reading, interview, fieldwork) in which data are produced? Stage three concerns textual production: how does the scholar document the pre-production and production of data in the resulting scholarly work? In conclusion, we leave the how-to mode and look at how data collection or method is underpinned by what Jackson (2010) calls ontological wagers, that is, what the world is taken to consist of. Analytical and reflectivist approaches share an ontological wager on monism; they see the social world as being forever in flux, and the researcher as a part of that world. They are divided on transfactualism. A reflexivist (such as Cecilie) takes social structures as transfactually given. This means that the focus is on the reflexivist herself, understood as a product of that structure, and that the scholarly work concerns how structures create the emergent scholarly self. By contrast, an analyticist (such as Iver), while acknowledging that the researcher's value commitments are changeable, goes for an approach where an (invariably value-based) model of the social is at the heart of the research process. The analyticist's focus is on how phenomena may be captured by means of that model. This means that the focus is on what other people do, and that the work concerns how that doing may be analyzed by means of the model.

Why should we care about situatedness? If the scholarly self is producing the data, then the more knowledge about how this process proceeds the scholar has, the less random the data production and the better the data. In the social sciences, data production or method is an intersubjective process; it involves direct or indirect interaction with other people. Data means factual knowledge about a certain phenomenon, be that an event or a state. When we speak of first-hand data, what we mean is that the researcher first asks some kind of question and then reads texts by eyewitnesses and participants, interviews persons, observes interactions or distributes questionnaires with a view to answering that question. The question is typically wide at first – what happens in this location? – only to become narrower -- why is it that the Xs keep on doing Y?

Data production goes hand in hand with another kind of production, namely the production of text. Social scientists are supposed to have a number of skills, such as leading seminars, doing

presentations and put their knowledge to use in non-scientific contexts, but what makes them scientists is that they possess the knowledge to ask questions, know how to apply those questions to social sequences so that data may be produced and are able to use those data in order to tell a story about a social phenomenon. Those stories must be related to extant scientific stories about the same phenomenon, at a minimum within the same discipline, preferably within the social sciences, ideally within science in general.

If, as both reflexivists and analyticists argue, the scholarly self is irretrievably tied to the world, there is no Archimedian point from which to size up the social. Knowledge is situated knowledge (Berger and Luckmann 1966, Smith 1974, Haraway 1988, Harding 1991, Bourdieu and Waquant 1992).<sup>2</sup> Humans are fundamentally relational and always already socially situated. What was at stake for feminists and constructivists was that objective knowledge in the positivist sense of the term is not possible to obtain. This line of argument was followed by the realization that the individual subject is not one unified entity, but consists of a fragmented, developing set of identities. We take these as givens, and concentrate on more specific questions, like what parts of the scholars previous experiences that are relevant for the choice and conduct of research projects. The reason is that so many of our colleagues treat the minutia of situatedness, or indeed of many method, as obvious, small fry, beneath their dignity. The result of such neglect may easily be suboptimal data production and non-situated books and articles..

Before we go to it, we should perhaps nonetheless take note of half a century's worth of attacks on the idea of situated knowledge for eschewing the objectivist ideals and leading to a degeneration of social science into the pit of subjectivism (see Hamati-Ataya 2014 for a generous overview). The researcher acts as a self-aware modest witness (Haraway 1988). It is helpful for the scholar to reflect on where that gaze comes from and what it does and does not catch, and it is helpful for readers who want to evaluate the findings to know something about the origins of the scholarly gaze that engendered the research.

### Three phases and three literatures

The temporality of the research process is circular. There is a recursive element to it. The data produced lead to a reformulation of the questions that initiated the production process, and the text produced forces a certain pattern in the produced data that often create new questions that have to be answered by new data production. The introduction is always the last thing to be written. Circularity notwithstanding, the very way we laid out the circularity is based on there being three more or less distinct phases to the process, that we address separately for analytical reasons. There is the planning or pre-field phase when phenomena are picked, the

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<sup>2</sup> To give but one quote, Smith's (1974: 11) response to charges of subjectivism was that 'the sociologist's investigation of our directly experienced world as a problem is a mode of discovering or rediscovering the society from within. She begins from her own original but tacit knowledge and from within the acts by which she brings it into her grasp in making it observable and in understanding how it works'.

work of reading other scientific narratives about it is being started, theoretical works that may help are being consulted and questions are honed. Then there is the data production or field phase, and then there is the writing phase. Each of these phases requires situating. In the pre-field phase, situatedness means thinking through why I have chosen this phenomenon instead of that, this theory instead of that, this field instead of that. Let's call this autobiographical situatedness. The next phase is the field phase itself, where situatedness turns on awareness about what is going on. How does my social characteristics, body language and attire shape the interaction and so the production of data? We call this field situatedness. Here, the 'field' refers to our interactions with actual persons in field sites and during interviews, as well as interactions with data such as documents and statistics (Jackson 2010). The field is also where our preparatory and ongoing work with autobiographical situatedness comes into play. Then there is the writing phase, which offer questions of an ethical nature – are there data that should be left out because leaving them in may have detrimental effects on other people? How should I flag my own status as data producer? We call this textual situatedness.

Thinking about situatedness has a long pedigree. At ritual sites in ancient Greece, we find exhortations for people to know themselves. These exhortations may be read in many ways, one of which is that you need to know yourself in order to understand what others are doing. We are talking about an exhortation to situate yourself here. David Hume famously noted that self-reflectiveness lends a quality to social life that makes it into a separate sphere. One implication, highlighted by Dilthey a long century later, is that human sciences are qualitatively different from natural sciences, and so need other methodologies and methods. The philosophical literature on these matters is huge, as are the social science shelves on the ontological and epistemic implications thereof. If we turn to the specific question of method, however, only three literatures stand out. Two of these are well known and have followers in International Relations (IR); these are feminism and poststructuralism. A third and older literature, is the Gestalt tradition, which has its philosophical origins in phenomenology.

We would argue that, while all three literatures at least implicitly address the question of situatedness as it pertains to all three phases of the research process, the main thrust of each tradition may be located within one of the phases. The feminist literature, be that standpoint or constructivist, has first and foremost pinpointed the pre-field phase and addressed the relevance of autobiographical situatedness. The Gestalt tradition started by looking at the psychological and psychological preconditions for cognition, which pertain to all three phases of the research process, but then came to specialize in interaction and field situatedness.<sup>3</sup> Poststructuralism has focused mostly on the process of writing and textual situatedness. Given this situation, we find it useful to lay out the major methodic challenges that pertain to each phase by using each of these three literatures as a guide. We will privilege the Gestalt tradition, since it is less known and more in need of introduction and potentially more useful due to its newness in IR than the other two, but also because the field phase that it targets is perhaps the least elaborated in terms of method.

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<sup>3</sup> Ash (1998) argues that the main reason for this was indeed their changed situatedness as they emigrated from a German academia mainly intent on philosophising about human being in the world into an American academia mainly preoccupied with getting the job of explaining human behavior done.

## Autobiographical situatedness

Langless and Frank (1981: 89-90) begin their treatment of autobiographies by stating that

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. [...] an autobiography is, by definition, an account that focusses on the inner life (Langless & Frank 1981: 90; see also Starobinski 1971).

Autobiography is about memory. 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: 184).

To say that memories are social is to say that they are malleable over time. Memories are the result of communicative social processes, and not closed psychological time capsules. They are constituted by aesthetical frameworks and narrative tropes (White 2006, Bleiker 2001). How these frameworks differ from culture to culture is relevant when we aim at situating ourselves in research, for the phenomenon of not being able to guess how one's interlocutors think about oneself is reinforced by cultural distance. Memories may also be suppressed, and so outside the immediate catchment area for situatedness. Autobiography is, therefore, an unreliable genre. 'Time is foreshortened, details selected and highlighted, action concentrated, relations simplified', historian David Lowenthal (1985: 216) writes in his widely read book *The Past Is Another Country*.

Since the 1970s, feminist epistemology has worked on the importance of memory and the social situatedness that give rise to memory for scientific endeavour.<sup>4</sup> A sprightly set of exercises like these are to be found in a recent volume edited by Naeem Inayatullah (2011).

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<sup>4</sup> It is the recognition of the situatedness of the knowledge production that has been key to the continual modification of [feminist] methodology' (Skeggs 2008: 678).

The usual starting point is a reflexive one, namely that social phenomena hail from structures. Structures cannot be observed directly, but can only be induced by tracing back from the effects of the postulated structures. Humans are amongst the phenomena produced by structure. Researchers are human. If, then, the researcher observes her memories of becoming who she is as effects of social structure, then she may tell us something about structure. Harding argued that there is a job to be done here that had not been done before:

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: 150).

Harding argues that the situated researcher does not produce relativistic knowledge. According to Harding, it is the sociological aspect 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 have the quality of what she calls string objectivity if the research process and its presumptions are clearly accounted for. While this point has a direct bearing on the need for autobiographical situatedness, the general point pertains to all humans, and so to researcher and interlocutors alike. 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: 151-152, see also Smith 1974, Haraway 1988, Harding 1993, Pohlhaus 2012:716).

Regarding the aspect of memory work that concerns autobiographical situating, it is not the production of memories itself that is of interest, 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, a question to which we will return). If the choice of phenomenon is how Roma were exterminated in Nazi concentration camps, it is directly relevant to ask oneself why this choice has been made. Is it because of some ethnic or social attachment to the Roma? If so, the upmost autobiographical question is distance to group attachment, and the memories to concentrate on are those that pertain to these aspects of one's

own identity. Is it because the many other stories, such as the state of Israel's stories about the Jewish holocaust, have repressed it? If so, the upmost autobiographical question becomes questions of subalternness and fairness, and the key autobiographical question is what it is about one's socialization (a Scandinavian inclusive national culture? A Christian universalistic upbringing?) that has inculcated such a way of looking at the world. Or is the choice due to a childhood fascination with travellers? If so, autobiographical situatedness may be a potential source of romanticizing bias in the field. We have here three very different ways into the field of research, and these ways will shape the research process. A reader alerted to autobiographical situatedness would ask at this point exactly why we choose Roma as an example. Well, one of us (Cecilie) has a Roma great grandfather (and was asked to leave a Cyprus sunglasses shop after having been asked if she was from Hungary only earlier this year; it turned out that there was a Roma village only three kilometers away).

Autobiographical situatedness also pertains to choice of theories.<sup>5</sup> One frequently hears researchers say that they choose their theories out of 'personal preference' or 'pragmatic reasons'. That is fair enough as far as it goes, but that is not very far. Social scientists are supposed to specialize in explaining a social fact, such as the choice by one human of one theory, in terms of other social facts, such as socialization. The point here is not that socialization determines choice direct. There is most often, maybe always, more than one reason for action. Socialization is, however, quite often one of them. One of us is a Foucauldian. Might that be due to autobiography? I (Iver) grew up with in-house nurses. From the ripe age of three and a half to four, I had one who used me as a sexual plaything, putting my little arms to intimate use. In order to keep me from telling my parents, she would wave needles in front of my eyes and let me suck her breasts. I still cannot stand the idea of substituting my glasses for contacts. There is no doubt in my mind that I took to Foucault like a duck takes to water because there was something immediately familiar about a relational understanding of power that stressed micro-practices being inscribed on the body, the production of docile bodies and the participation of the subaltern party in his own subjection.<sup>6</sup> For psychological reasons, my gaze was training towards the kind of situation he was talking about, and so I recognised his perspective on the world as akin to my own. Colleagues have sometimes wondered out loud how a white upper-middle class male who is leading a fairly regular family life ended up as a Foucauldian. I don't.

### Field situatedness

The early Gestalt theorists were trained in both experimental psychology and philosophy, and their philosophical godfather was phenomenologist Edmund Husserl. They were interested in ways of seeing, and their great discovery was that humans see wholeness where what is physically present to see is actually fragments. One of the reasons why data are constructed and not collected is that seeing and also sensing in general is an active and creative activity

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<sup>5</sup> This paragraph draws on Neumann 2010.

<sup>6</sup> Intellectual lives are by definition shaped by reading experiences. For a lively discussion of this, see Szokolcsai 1998.



that does not record, but actually produces, phenomena. Since these wholenesses or gestalts are actively produced by individuals as a result not only out of a psychological need to categorize the world, but also out of specific social experiences that help determine which categories are actually being used, Gestalt psychologists have specialized in situatedness ever since (Ash 1997). As elaborated by the Gestalt psychologist Gordon Wheeler (1998: 28):

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.

The field sets the scene for our active cognition; we pick out and foreground what the gestalt tradition refers to as a figure, while the rest becomes (back)ground. The flavor of this kind of theorizing that highlights the psychological aspect should be clear already from a short précis of the experiments that moved the Gestalt tradition away from looking at perception of specific phenomena in laboratories and onto looking at behaviour in general.

Around the time of the First World War, Wolfgang Köhler 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 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 smack into the trough. Köhler then placed his two-year-old daughter in the cage. After a short while the trick dawned on her, too, and she was around the glass wall and into the bowl in one fell swoop. (Köhler's dog was rather less fortunate. He could smell the food through the glass wall, with the result that it 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 was a potential inherent in the mammal brain (Lakoff and Johnsen 1999).

This may seem far removed from social scientific research practice and the question of situatedness. It is not. The researcher will tend to grasp insights as wholenesses, and she will grasp them by means of categories that are part of her software. Let us give two examples of how this works.

Although I (Cecilie) had been interested in the effects and workings of class for many years, I had yet to research class myself when I started my Ph.D, and I had no clear idea of how to do it. During my fieldwork I sat in a waiting room in a health visiting center in the eastern part of Oslo observing the interactions between parents and their children. The atmosphere in the waiting room was quiet, even anxious, and there was little interaction between parents and children. As I pondered the meaning of this, a mother and her two children entered the room. The mother was very well yet discreetly dressed in good-quality clothes and jewelry. She sat down to talk and play with her children, and when the health visitor came to fetch her, she did not hear the call. This mother stood out in sharp contrast to the other parents and made me suddenly realize, Köhler's ape-style, how I could study class. I recognized this mother's being-in-the-world as akin to my own. The reason why I experienced the other mothers and their practices – their dress, their on-parade comportment, their silence, their anxiety in front of the health visitors – as strange, was simply a class thing. They were overwhelmed by the middle class knowledge of the health visitor. The talkative mother and I were not, for we, too, were possessors of (other kinds of) middle-class knowledge. By implication, one way to study class would be to observe closely what kind of effects my interlocutors have on me, and to use these observations as analytical entries. Here we have a concrete example of how awareness of one's own situatedness (*in casu*, class) is analytically helpful when we want to understand the workings of hierarchies and marginalization in empirical studies. In the case of health visitors, the researcher's self-awareness on class and gender gave an analytical entry to see how marginalization of class and gender is shaped and co-constituted by specific welfare agents whose job it is to do the opposite; to secure equality (Neumann 2012: 149; see also Skeggs 2002 on working class women and care work and how they are positioned towards the state as marginalized guarantors of welfare politics).

To complement with an example from IR, my (Iver's) supervisor, John Vincent, once told me how he had set out to write his own doctorate on when military intervention would be warranted. He could not get it to come together. One day he suddenly realised that he himself was against intervention on principle. He threw out what he had, recast the thesis as a history of the principle of non-intervention, and finished the thing in a year.

Here we have another reason why autobiographical situating 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 class ( or gender, or ethnicity, or...) may open up possibilities for analysing power 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 used to result in a lot of questions (see also Dauphinee 2013).<sup>7</sup>

Finally, the epiphany-like way in which wholenesses come together should be a consolation to the field-working researcher. Field work typically involves a lot of seemingly false starts and a lot of frustrations. There are periods where we, usually falsely, feel that nothing happens. If, however, the researcher has done her pre-field job and read up on as many aspects of the phenomenon to be studied as possible, then chances are good that things will suddenly fall into place. When Nietzsche exhorted us to perspectivise a phenomenon and when Geertz told us to provide a thick description of it, it was exactly because the more angles we have on a phenomenon and the more disjointed stuff we know about it, the better the ground, and the better the ground, the better the chances that a figure will emerge. The way to insight into wholeness goes via the scrutiny of parts. Every little thing may prove to be one of the triggers for the Gestalt to emerge.<sup>7</sup>

The Gestalt tradition also offers another methodic tool that may help researchers situate while in the field, namely the so-called contact mechanisms. 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 characterisation (Clarkson and Mackewn 1992). They are, rather, modes of interaction that may be prevalent for certain people, in certain situations, such as an interview of/or? a casual chat with an informant. Since they all crop up in informants, it may be good to be aware of all six -- projection, introjection, retroflection, deflection, confluence, egotism. Since confluence and projection are the most usual ones,

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<sup>7</sup> In a research project on social workers in a home based residential care for children, my analytical entry to understand the social workers professional ethics went through reflections on my own experiences with motherhood. I would insist that my children do their homework instead of watching TV for hours. How could I make sense of their particular ways of acceptance, ways of setting limits, of service and care compared to what parents do in what we like to think of as "normal" families?). This in turn led me to investigate the impact of the institutional conditions on the social workers, and how this particular institution was situated in and affected by the larger child welfare machinery in Norway (Neumann 2010, see also Dorothy Smith 2005 on institutional ethnography).

however, we will use those as examples and relate them to the researcher, as opposed to her interlocutors.<sup>8</sup>

*Confluence* refers to a way of being with others that is characterised 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, be those perspectives (an economic gaze), identities (we are both fathers), political viewpoints (there is no such thing as a combatant child). Without the ability and desire to create confluence, there would be a great deal more conflict in human interactions and communications. 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 and the friendly atmosphere may lead to the researcher avoiding questions about things that should clearly have been asked about. Fear that the interlocutor will perceive such questions as annoying or hurtful will come in the way of data creation. Woody Allen’s persona Zelig, from the eponymously named film, is the ultimate in confluence; he literally becomes the person he is speaking to.

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 particularly 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.

If confluence is about over-identifying with the interlocutor, then *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 of my mother I will perceive what you say and do as a continuation of my expectations of her, not of

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<sup>8</sup> *Egotism* is the opposite contact mechanisms of confluence. Persons characterised 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 *retroreflecting* does to others what he wants them to do to him. If I like to be supervised in a certain way, I will assume that my students also prefer to receive the same type of supervision. 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. When you ask her opinion it becomes apparent that she has not heard a word of what has been said or done.

you. 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. We assume that we share something, and then we go about finding out (Frykman and Løfgren 2003). This has also been a concern among feminists. Drawing on Harding and others, Ruane (2013: 53) 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.

In other words, to project is also an attempt to include the other and to 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 recognise 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 generalised 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 seven 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 IR 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.

### Textual situatedness

We have discussed situatedness pre-field. We have discussed situatedness in-field. Then there is situatedness post-field. We both remember our flabbergasted student reactions to some of our own professors when they said we should ‘just write up the material’. There is no ‘just’ about it. It is demanding and exhausting work. Stuff must come together. And then there is the question of situatedness – how are the researcher and her interlocutors positioned in the text?

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 field work (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 means that changes are called for. It is

illuminating to consider how the positivist translator into English of a minor classic on the Carolingian 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 rading 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 rering 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: 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. The first seeds were sown in the late 1960s, when poststructuralists like Roland Barthes and Michel Foucault problematized the death of the author.<sup>9</sup> 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* ([1977] 2007), 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* (1986). As pioneer political scientist Michael Shapiro formulated the main issue at stake around the same time,

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 (Shapiro 1988: 72).<sup>10</sup>

There is 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

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<sup>9</sup> The key philosophical source on which they drew was Martin Heidegger, as did Hanna Arendt in the quote on thrownness given above.

<sup>10</sup> 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.

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 Brazil in Brazilian, lots of stuff may be taken for granted. When the same material is to be pitched to an English-speaking audience, however, more general contextualisation is needed. Such situating 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 situatedness, targeting the future.<sup>11</sup>

This was much on my (Iver's) mind when I wrote my second doctoral thesis, on diplomats. Ian Hacking has discussed how ontological, epistemological and methodological concerns colour what he calls our styles of reasoning (Hacking, 2002: 4). My problem was that I came to anthropology as from political science and IR, and political scientists and IR scholars have rather different concerns than political science. 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. Although it was surprisingly hard to kick old habits, the major challenge was not one of 'style of reasoning', as specified by Hacking, but rather of style of writing (see Derrida 1985). To write anthropology is very different from writing political science. Karsten, one of my 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. Such a way of writing has little space for active verb forms and pronouns such as "I", "you" and "me", and is more inclined to passive verb forms, "we" and "one". The "view from nowhere" is the antithesis of the situated view. For the anthropologist, it is also an impossibility (see for example Ardener 1989: 213-214). Anthropologists have a running debate on situatedness (a particularly commendable volume is Okely & Callaway 2002). The textual situatedness of interlocutors is informed by the adage 'show it, don't say it', which takes some doing to learn and learning to do, even for those, like myself, who had already paid heed to these things for decades. There are considerable differences in national academic tradition that may exacerbate these problems. In British academic texts one may simply imply a theoretical concept (as we have implied the phenomenological concept of thrownness in this text), and in a French academic text, anything more than an allusion might 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. An American mainstream political scientist with the unlikely wish to pick up an extra competence in anthropology would probably be doubly (disciplinarily and nationally) challenged.

Two ways of going about situatedness

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<sup>11</sup> 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 play (or will not) be played out (Sokolowski 2000).



The point of departure was that data production is intersubjective. We also stressed how it is necessarily the researcher who is in charge of the research process, for she is the one with the fullest information on where it is going, for the very simple reason that it happens in her. This means that the researcher makes things (among them, texts) happen that would not otherwise have happened. In other words, she is, *ceteris paribus*, at the empowered side of the power-laden relation with her informants where scientific discourse is concerned.<sup>12</sup> One response to this fact is to 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 (2010) 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. ‘[R]eflexivist scholars -- chief among them, feminists and critical theorists, including postcolonial scholars -- would say that knowledge of social arrangements begins not with the world, but with the self. Explicit, if necessarily incomplete, self-awareness marks the distinctive methodological strategy employed by reflexivist scholars’ (Jackson 2010: 159).

The way to produce, that is, the method to use, is to trace changes in the self, be that by memory work or by tracing how research experiences change us pre-field, in-field and post-field. In IR, we are blessed with an exemplary example of such work, in the shape of Elizabeth Dauphinée’s (2013) account of how research on the Bosnian War changed her entire way of thinking about research and of being a researcher, but also and crucially about Balkan wars in the 1990s and conflict in general. As to textual situatedness, she recounts how she tore up one manuscript because she found it to be inadequate in this regard, and wrote a book on how she was changed instead.<sup>13</sup>

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.<sup>14</sup> 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.

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<sup>12</sup> 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.

<sup>13</sup> While that new book was successful, we note that the procedure cannot be recommended across the board; the primary interest of social science remains the social patterns formed by the people we study, and not our own angst. Dauphinée would definitely not be a standpoint feminist in Harding’s sense, for there does not seem to be a ‘strong objectivist’ programme of objectifying social positions as a scholar and a multiplexed positioned human being (class, race, gender, etc) here. Still, Dauphinée does come close to Harding (1991) and also Haraway (1988) in thinking about knowledge as partial and fragmented. In stepping down from the position as ‘knowledgeable researcher’ to the position as ‘modest witness’ and in critically scrutinizing the clash between her own scholarly knowledge and the voice of a war criminal, her project has some of the critical effects on the social order as well as on IR as scholarly discipline that Harding calls for. All share a reflexivist approach.

<sup>14</sup> A number of texts on autobiography in IR, such as Brigg and Bleiker 2010 and Vradi 2008, imply that reflexivity is the only scholarly way of situating oneself. This monological view is criticized not only by Jackson 2010, but also by Racantore 2010 and Sande Lie 2011.

Another way is what Jackson (2010) calls the analyticist way. The analyticist, who may have various specific (and internally conflicting) theoretical attachments such as Weberian, pragmatic or Foucauldian ones, does not focus on herself in order to induce the nature of structures existing out there, such as patriarchy, for she believes that a timeless structure such as patriarchy simply do not exist. She instead focusses on coming to terms with herself as an instrument of data production relative to the social world, be that in terms of forging an ideal type based on a value judgment, in terms of why she 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 manifest structures, with the intention of identifying the 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. The entire reason why Foucault hatched concepts like *episteme*, *discourse* and *dispositif* was to find alternative ways of thinking about how phenomena like socially situated humans emerge. So, Foucault would agree with Harding or Dauphinée 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.

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 (2010: X) 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.

In Foucault (e.g. 2011), 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 18<sup>th</sup> century French hermaphrodites, 19<sup>th</sup> century French murderers or 20<sup>th</sup> 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: 785) 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?

I (Iver) have 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 Dauphinée's work, the accent is on how her main informant, Stojan Sokolovi'c, 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 reflectivist/analyticist distinction is no exception. We are particularly well placed to do this, since Cecile is more of a reflexivist and Iver is more of an analyticist, which means that we have been quarreling about how to frame a number of examples given here throughout the gestation of this article.<sup>15</sup> Note that Jackson's reflexivist has placed her ontological wager on there existing structures beyond the social. Note also that post-structuralism's break with structuralism fastened exactly on the ontic status of structure. Post-structuralists 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 categorised 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.

## Conclusion

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<sup>15</sup> In a similar vein, Hamati-Ataya (2012: 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.

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 are ethical questions 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 find 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 changes 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.

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