to crosslink and analyze the underlying conditions for certain types of statements later – thus to define different contextual levels for different interviews, documents or actor types (Corbin and Strauss 2008; Keller 2013).

Based on Grounded Theory, I used a few analytic strategies as entry points into analysis. On the one hand, asking questions about the content ("what is going on?") and about theory ("what is the relationship of one concept to another?"), and on the other hand making comparisons between texts in order to see similarities and differences. Using the software Atlas.ti, I developed conceptual codes to depict underlying concepts identified, such institutions, activities or ideas. Next to the broader concepts taken from the conceptual frame, in the beginning categories were developed by looking at the text itself, such as main problems, central themes, concerns. During analysis, I added further codes for recurring patterns (DeWalt and DeWalt 2011; Keller 2013). Codes were set into relation to each other, for example regarding the interaction between actors or in view of causes and effects. On this basis, I differentiated between themes or categories, elaborated properties or dimensions specific to one theme or one group and developed different subcodes which depicted nuances. The ongoing analysis was accompanied through writing memos, i.e. notes on ideas, concepts, dimensions of categories, codes, comparisons, etc. (Ritchie and Spencer 1994; Corbin and Strauss 2008; DeWalt and DeWalt 2011; Keller 2013). To exemplify coding, an extract of the list of codes is added as Appendix A-6.

4.6 Reflections on my position as a researcher

4.6.1 Objectivity and reflexivity in research

While scientific paradigms such as positivism or post-positivism stick to an image of science as objective and disinterested, scientists following constructivism or critical theory question this image and challenge the idea that scientific knowledge is truly objective and rational (Guba and Lincoln 1994). Qualitative research requires interpretation of the data – which necessarily involves the researcher who attaches meaning to data and represents results of analysis in a written form (Langer 2013). Thus, scientific knowledge is viewed as a type of knowledge among others, affected by interests, ideologies and world views of the scientists producing it (Sismondo 2008). Following, if underlying interests coin science and research, these biases should be acknowledged rather than hidden. As Cox provokingly states: If a scientific theory pretends not to have a standpoint, it is most probably an ideology (Cox 1981).

Scientific reflexivity is a concept in this vein, acknowledging biases and the own position. Coined by social thinkers such as Bourdieu or Giddens, it rose to the

agenda in the 1980s. Most reflexivity concepts share the idea that "authors should explicitly position themselves in relation to their objects of study so that one may assess researchers' knowledge claims in terms of situated aspects of their social selves and reveal their (often hidden) doxic values and assumptions" (Maton 2003: 54). There are some differences in conceptualizing reflexivity. Scholars following Bourdieu's definition continue to believe in the possibility of objective representation. For them, reflexivity depicts the reflection of the (social science) researcher on his/her own doing through sociological methods, aimed at overcoming one's own perspective, subjectivity and positionality and thereby moving towards a higher degree of objectivity. In contrast, scholars in the tradition of constructivist Science and Technology Studies stress the impossibility of objective representations by a single researcher and call for alternative perspectives on a research problem in order to contextualize and contrast different knowledges (Langenohl 2009).

Scholars also detect an inherent paradox in reflexivity: Even a reflection on the own doing is positional and partial. Potentially, there are infinite possibilities of constructing and deconstructing realities and reflecting on reflections – which bears the risk of reflections becoming a means of its own, and not leading to further insights about the research subject. Authors therefore propose to pragmatically put limits to reflections (Maton 2003; Schweder et al. 2013).

Despite all differences and paradoxes, and although little has been said about how to put reflexivity into research practice (Maton 2003; Kühner et al. 2013), reflexivity in form of problematizing the own knowledge production has turned into a norm in critical paradigms and their scientific practice and ethos (Kühner et al. 2013). Aiming at comprehensibility and transparency, researchers provide reflexive accounts on how data was collected and interpreted (Langer 2013; Hametner 2013).

In case of discourse analysis, Keller equally argues in favour of a high degree of reflexivity on the own research activity and its relation to objective truth. Discourse research is not believed to produce exclusive truths, but rather a *discourse on discourse*. In addition, the statements produced through research are part of a social science discourse, thus being enabled and constrained by current norms of research production, guided by pre-existing structures defining which type of research practice or statement production is legitimate, or who can fulfil speaker positions within discourse (Keller 2011a).

Notions of reflexivity are often based on the idea that the interaction of researcher and research field is a part of the research process, which adds to contextual data production, if not even to impacts on reality. For example, the role of the researcher and the researched can be viewed in the context of power relations affecting the field and their effects on research. Looking at the other direction, research effects on the actors within the research field become a subject of reflexivity. Being part of a discursive field as a researcher structures interaction with actors in the researched field, who reinforce or negate subject positions, attribute differ-

ent levels of power or social standing to the researcher, while the researcher may also reinforce or deconstruct these, thereby also impacting on power structures (Hametner 2013).

For my own research, this means that while it is impossible to reduce the own subjectivity to zero, my own role shall at least be made transparent through reflecting on my own doing in the field and in data analysis. This shall not lead to narcistic diary-like accounts (Maton 2003), but rather to a better understanding of the research process and the data generated, through "explicit attention to the ways in which family background, personality, education, training, and other experience might well shape who and what the researcher is able to access, as well as the ways in which he makes sense of the generated data" (Yanow 2006: 408).

The reflections not only on biases, but also on other aspects of positioning oneself in the research setting may also lead to further insights into the discursive field or the power relations at play. In this vein, it is interesting to consider how interviewees perceived me as a researcher, or to reflect on their expectations in view of the research outcomes. The ascriptions in the interaction and communication during fieldwork this type of context-specific data was produced (Bogner and Menz 2002). While some insights into my role in the research process are detailed in this chapter, reflections will also be part of the empirical chapters.

4.6.2 Interactions in the field: My own position as a researcher

Researching on policy making and project implementation in science cooperation can be described as a situation of "studying up" (Wedel et al. 2005): Actors of the researched field were mainly highly educated, holding PhD titles or higher degrees, and occupied high-level jobs and thus had a higher social and educational status. This led to a variety of challenges ranging from interaction with interviewees to ethical considerations during data analysis. Interview partners perceived and reacted to me in different ways, some treating me as a lay person foreign to the topic; some discussing with me as a co-expert; some perceiving me as potential critic or evaluator (Bogner and Menz 2002).

As knowledgeable experts, often with natural science or engineering background, some interview partners openly voiced their scepticism regarding my research question, methods, or scientific paradigm (Nullmeier et al. 2003). A reoccurring concern of the interview partners was the lack of quantitative analysis, statistical representativeness, or evaluative indicators. In addition, my aptness to do research on a topic in such a highly political context was questioned on several occasions. In the most extreme cases respondents called me naïve to believe that I could do research on such an assumingly hot topic without negative consequences for myself or suggested to leave research on the subject to advanced researchers with a permanent position independent of BMBF influence.

In many cases, potential candidates seemed to shy away from being interviewed. Several authors have blamed fear of potential criticism for this reluctance to share information. In professional settings, such as policy making, actors would be generally uncomfortable with spreading internal information, or fear their professional routines or identities to be deconstructed (Nullmeier et al. 2003; Mosse 2011). However, I feel that in case of my research, the reasons were of a different nature. Although an evaluation of project activities was never among my research activities, speaking openly about certain issues seemed to cause fears of harmful consequences, such as not being granted future funding. Interviewees were often sceptical and reluctant to speak openly, possibly critically, about the ministry. Thus, even those who were critical about the existing power relations and policies were afraid of potential negative consequences if critique would be openly outspoken. Most interviewees therefore only agreed to be interviewed anonymously and/or under the condition to approve their interview data before publishing. In order to build trust, lengthy explanations about my background, my research interest, as well as my independence – financially and conceptually – from the BMBF were necessary to reassure that statements were not shared with the ministry.

Similar to Mosse's experience (2011), I experienced it as disturbing to be accused of wrong views, of causing harm for the institution or putting future funding at risk. More so, however, I was also surprised by the emotional responses. Being confronted with hidden to open scepticism and rejection by some potential interviewees was hard, as of course I had hoped to be faced with research participants who would appreciate the usefulness of the research project. However, the emotionality involved also revealed that I was hitting a sensitive spot in the interviewees, and that they apparently attributed importance to the research topic (Corbin and Strauss 2008). At the same time, the reluctance also made me very aware that the topic was highly sensitive and data presentation had to be done in a way that does not harm any of those who had consented to being interviewed despite of their fears.

Which is the adequate way of reacting to reproaches and scepticism? As Mosse (2006: 949) puts it: "An analysis that exists within a field of objections has to be sure of itself." Being convinced that the topic was worthwhile to be investigated, I tried to evade further criticism by being scientifically sound, methodologically as transparent as possible, and providing reflexive accounts of research. On the other hand, the reactions also triggered some questions about the origin of the objections, which I consider to be the unequal power relations inherent in the relations between ministry, project management agencies, and projects. These will be highlighted separately in the following chapters.

4.6.3 Ethical considerations

Scientific ethical codes were mainly designed in view of people studied who are less powerful than the researcher, thus in view of studying *down*. However, if "the people being studied are more powerful than the studiers, this precept […] is problematic" (Wedel et al. 2005: 42). The authors therefore argue that in a context of powerful actors or institutions, such as government agencies, researchers should be allowed to follow journalistic ethical codes instead of scientific codes of conduct regarding their sources (Wedel et al. 2005).

This research project nevertheless followed the standard principle of ethics in research to maintain the integrity of informants and do no harm (Cresswell 1998). This meant that the objectives of research were entirely disclosed to the interviewees and participants before conducting interviews and participant observation within the case study projects (Neuman 2006).

A reflexive science should acknowledge the power inherent in data interpretation: The researcher has the power to make sense of the data (Mosse 2006; Hametner 2013). In case of studying up, this poses an ethical dilemma: As a general rule, social scientists should return their writing to their interviewees for verification. But in case of public policy and other official settings, Mosse argues that representations in research often compete with official, authorized representations of the informants (Mosse 2006; 2011). He suggests

> "that the way in which professional informants respond to ethnographic description itself generates important research insights. […] But this does not mean that such 'objection' is a form of triangulation. Indeed, objections rarely concern simple matters of fact, but reveal divergent epistemologies and frames of reference, perhaps those of managerial and interpretative viewpoints or of policy professionals and ethnographers." (Mosse 2011: 51)

The author thereby points to a tight spot: Does the initial consent to being (anonymously) interviewed encompass the right of final interpretation and editing, and how can the researcher disconnect consent and "demand for interpretative consensus" (Mosse 2011: 51)? This is highly important, as powerful actors may resort to ethics codes in order "to evade social science scrutiny, resist critical analysis, gain control over research and protect reputations and public images of success" (Mosse 2011: 51). Thus, the question is where to draw the line between what is scientifically correct, and what is socially correct or desirable, what can be said without causing potential harm or being censored (Mosse 2006).

On this background, and to obtain room for my analysis while fulfilling my obligations towards the interviewees at the same time, I decided to anonymize interviews instead of sending in interview transcripts or analyzed data to the interviewees for authorisation. This enabled me to maintain the interpretive authority

of the data to myself, while I consider the loss of information through anonymisation as limited – and justifiable, as in discourse analysis, interview statements are considered as part of a larger body of typical statements and practices anyway (Keller 2011a).

Balancing of the necessary level of anonymity with the loss of information, I classified interview partners into four primary categories with a letter code attributed to each: a) external experts (EE), b) policy makers/administrative staff (PA), c) project management agency staff (PT), and d) project participants (PP). Based on the primary category, each interviewee was given an identification code, consisting of the two-lettered category and a unique number. References to specific interviews are given through referring to the identification code. All interviews are further listed in an anonymized overview of interviewees (Appendix A-2). In the list, further details about the interviews are given: The interviewees' broad field of expertise, their institutional background, as well as the date of the interview. In order to ensure anonymity, any references to gender were eliminated. Among the interviewees, 4 held positions on a working level (such as PhD students, working level staff with lower levels of responsibility); 59 worked on mid-level positions (managers, post-docs, research officers, heads of small units), and 37 were high-level staff (such as heads of ministerial (sub-)departments and above, directors, professors).