Making Meteorology: Social Relations and Scientific Practice

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<td>AGCM</td>
<td>Atmospheric General Circulation Model</td>
</tr>
<tr>
<td>ANT</td>
<td>Actor-Network Theory</td>
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<td>AP</td>
<td>Atmospheric Physics section</td>
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<td>CCN</td>
<td>Cloud Condensation Nuclei</td>
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<td>CEM</td>
<td>Cloud Ensemble Models</td>
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<tr>
<td>CM</td>
<td>Chemical Meteorology section</td>
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<td>CPC</td>
<td>Condensation Particle Counter</td>
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<td>CVI</td>
<td>Counter Virtual Impactor</td>
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<td>DM</td>
<td>Dynamical Meteorology section</td>
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<tr>
<td>DMA</td>
<td>Differential Mobility Analyzer</td>
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<tr>
<td>FORMAS</td>
<td>The Swedish Research Council for Environment, Agricultural Sciences and Spatial Planning</td>
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<tr>
<td>FORTRAN</td>
<td>FORmula TRANslator</td>
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<tr>
<td>GCM</td>
<td>General Circulation Model</td>
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<tr>
<td>IGBP</td>
<td>International Geosphere-Biosphere Program</td>
</tr>
<tr>
<td>IMI</td>
<td>International Meteorological Institute</td>
</tr>
<tr>
<td>IPCC</td>
<td>International Panel for Climate Change</td>
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<tr>
<td>LES</td>
<td>Large Eddy Simulation</td>
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<tr>
<td>MISU</td>
<td>Department of Meteorology, Stockholm University</td>
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<tr>
<td>MIUU</td>
<td>Department of Meteorology, Uppsala University</td>
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<tr>
<td>NWP</td>
<td>Numerical Weather Prediction</td>
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<td>OPC</td>
<td>Optical Particle Counter</td>
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<tr>
<td>SCM</td>
<td>Single-Column Models</td>
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<td>SMHI</td>
<td>Swedish Meteorological and Hydrological Institute</td>
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<td>SSK</td>
<td>Sociology of Scientific Knowledge</td>
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<tr>
<td>UNEP</td>
<td>United Nation Environmental Program</td>
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<td>WCRP</td>
<td>World Climate Research Program</td>
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<td>WMO</td>
<td>World Meteorological Organization</td>
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<td>WWW</td>
<td>World Weather Watch</td>
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Introduction

This book is about science, and in particular, meteorology, as a production process. Scientific practices and their organization are seen as intimately linked to scientific knowledge – the outcome of organized activities in the world of scientists. Every scientific result or fact has a history and a production process (cf. Latour 1999a). While many studies have focused on the production of a specific fact, this study attempts to say something about the production apparatus itself, including how it is organized.

The purpose of this book is to describe the central practices used in meteorological research and to analyze how these practices are organized and interconnected in the production process. I also aim to examine the consequences of the diversity of practices for social processes and knowledge production. Meteorological research is an interesting case primarily because of its central role in the climate change question and its heavy reliance on simulation modeling, a scientific practice that has yet to be explored in depth.

Human induced climatic change has gained increasing attention within meteorological research during the past twenty to thirty years. It has also become a major question in national and international politics, the environmental movement, and the media. The United Nations Intergovernmental Panel on Climate Change (IPCC), which was formed in 1988, has played an important role in climate politics and discussions about climate change in general.¹ Meteorologists have dominated the panel, partly because the IPCC was formed under the auspices of the World Meteorological Organization (WMO), but also because of how the climate question has been defined as a research question (Elzinga & Nolin 1998: 48f.). Throughout most of the 20th century, the history of climate politics has been intimately linked with the history of simulation models of the atmosphere; models that were initially developed to forecast weather (Edwards 2001: 41). Thus, and principally because climate models are originally based on meteorological weather models, the investigation of current scientific claims about climate change necessarily brings meteorological research within the frame of analysis.

¹ For discussions about this, see e.g. Miller (2004); Miller & Edwards (2001); O’Riordan & Jäger (1996); Shackley & Wynne (1996); Elzinga & Nolin (1998); van der Sluijs et al. (1998); Skovdín (1999b).
According to Jan Nolin (1999), scientific research on this issue started to grow dramatically after 1990. On the basis of his account, however, it is unclear what type of research actually proliferated in Sweden at the time. It remains unclear what climate research is. An interesting question is therefore what defines climate research? This thesis investigates this issue from the perspective of how researchers present and define their research questions and activities in climate terms. If research funding politics contributes to the stability of disciplinary structures (Nolin 1999), this further motivates the relevance of taking the discipline of meteorology as the point of departure – rather than the climate change problem (cf. Nolin 1995) – when studying Swedish climate research.

Other studies of the connection between politics and climate research have emphasized research practices and particularly climate modeling. Many of these analyses are based on studies of the better known centers of climate research (e.g. the Hadley center), but the climate modelers working at these centers represent only a small fraction of the climate research community. As Simon Shackley (2001: 108) puts it, some of these centers are the “tip of the iceberg” of climate modeling and relatively few climate modelers actually do that type of work. Although these high profile centers are undoubtedly important (cf. Nolin 1999), the work carried out there is dependent on the contribution of the international research community (Shackley et al.1998: 204). This raises questions about the rest of the climate researchers, including what they are working with and how they contribute. If there are also climate scientists who are not working with climate models, what are they doing and how do they define themselves in relation to climate modeling?

Climate change has become a key issue in the global transformation of the world order and a detailed understanding of scientific practice is necessary if we want to fully understand the dynamics of global environmental governance (Miller & Edwards 2001). However, and as I have mentioned above, most of the interest in scientific practice has concerned climate modeling. If meteorology has played a key role in defining both the climate question and what climate research should be about, in line with the idea that scientific practice is politics pursued by other means (cf. Latour 1983), it is necessary to gain a detailed understanding of this practice and how knowledge is produced rather than only how it is used. Whereas environmental sociology tends to view scientific knowledge not so much from the perspective of its local production but from a user perspective, for instance as a basis for policy making or in terms of contributing expertise, Brian Wynne (1995, see also Yearley 1995) argues that studies of science and technology (production) have not paid enough

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2 See e.g. Edwards (2001); Shackley & Wynne (1996); Shackley et al. (1998); Shackley et al. (1999); Shackley (2001); van der Sluijs et al. (1998).
attention to the environment and environmental problems. By studying the relation between practices within meteorological research and the climate change question, as it is constructed and appropriated into research practice, this study attempts to link themes of environmental sociology to scientific practice. How can contemporary practices and the relationship between them be understood in relation to the question of climate change? How does the research focus on climate change affect the relationship between different types of research practices? These questions become especially interesting when recognizing that the content of “climate research” is not given (cf. Nolin 1999; Wynne 1993) and that the definition of a problem such as climate change is negotiated between several actors (cf. Strauss 1978b; Clarke 1991, see also Knorr Cetina 1982).

From this it can be concluded that a study of local meteorological research practices and how they are organized in relation to the climate change question is needed. The discussion above further suggests how a study of these practices per se can make an important contribution to sociological studies of climate research. In fact, a major part of this thesis does not directly tie the analysis of meteorological research practices to climate questions.

However, studies of scientific practice more generally are important because of its fundamental role in forming contemporary Western societies, which are increasingly ruled by knowledge and expertise (Bell 1973; Beck 1992; Giddens 1990, Nowotny et al. 2001). While it has been argued that understanding knowledge practices is a part of understanding knowledge societies (Knorr Cetina 2001), others assert that the study of science is fundamental for understanding society in general, since it is in science that society is partly made (e.g. Latour 1983). Science is a fundamentally important field for sociological inquiry because it creates our view of the world and forms our deepest assumptions about the taken-for-granted (cf. Star 1991b: 32). The ways in which we know and represent the world – both nature and society – are inseparable from how we choose to live in it (Jasanoff 2004b). This becomes especially evident in issues where it is difficult to make a sharp distinction between what concerns nature and what concerns society (cf. Latour 1991/1993). For instance, global warming is not only a potential danger for the future balance between different spheres of the climate system, but is also one of today’s major challenges for industrialized societies in terms of how to deal with our energy consumption, industrial production, and international environmental regulation, to mention but a few issues related to the climate change problem. Climate simulations have been of fundamental importance for understanding and conceptualizing climate change and, since people cannot experience climate change directly with their senses, our knowledge and awareness of it is completely dependent on scientific accounts (cf. Beck 1992).
Furthermore, in empirical studies of science, traditional experimental work as for example carried out in research in molecular biology, biochemistry, and endocrinology, has received most attention. At the same time, models have become increasingly important in several disciplines (Morgan & Morrison 1999) and simulation modeling offers novel ways to perform research that are lacking in theoretical modeling. Starting in the 1950’s, at the same time as the use of computers began to increase, meteorologists were among the first to use and develop the new and powerful calculative resources for weather forecasting (Nebeker 1995). Consequently, since the beginning of simulation modeling, it has been intimately linked to the development of meteorology. However, it is far from clear that the conceptualizations and interpretations used in scientific work in traditional laboratory settings can be adequately transposed to simulation modeling practice (cf. Bowker et al. 1997b: xvi). Accordingly, if simulation modeling and modeling more generally, not only in meteorology but also in for instance economics and sociology, to an increasing extent becomes a basis for our knowledge of the world – indeed becomes a substitute of the world in a sense – and political decision-making in issues ranging from environmental issues (e.g. Wynne 1996; Miller 2004) to economic policy (Evans 1999; van der Bogaard 1999) and perhaps other public issues, there is certainly a need for sociologists to take more notice of this scientific practice and to strive to reach a better understanding of it (cf. Jasanoff & Wynne 1998). This study is a step in this direction.

In order to situate my approach to scientific practice, the following section provides a brief introduction to symbolic interactionist studies and theory and also other theories that have shaped the direction of the study and my analysis. More detailed discussions of these matters wait until the following theoretical chapter. After outlining the general theoretical grounding of this study, I discuss the role of things (material objects) in a sociological analysis and briefly present the empirical work. The chapter concludes with an outline of the book.

Symbolic Interactionism and Social World Theory in Studies of Science

In order to approach scientific practice, i.e. what scientists do rather than what they produce and who is doing what, I find social worlds theory,

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3 For “classic” examples, see e.g. Latour & Woolgar (1979/1986); Knorr Cetina (1981); Lynch (1985). For more recent work, see e.g. Knorr Cetina (1999).
4 This may cause some overlapping but it is important in order to distinguish between the historical development of some important ideas, theoretical points of departure for the study, and analytical tools.
originating in the Chicago school of symbolic interactionism, to be a fruitful point of departure. Symbolic interactionism stresses how human beings interact and how meaning is created and interpreted in, and through, this interaction (Blumer 1969, cf. Mead 1934). As a form of interactionism, the Chicago School concentrated on the flow of interaction and interpretative processes, looking at the way in which meanings develop and change (Craib 1992: 88). In line with the assumption that the meanings of phenomena are to be found in their embedding into social relations, the early studies based on the Chicago School of symbolic interactionism aimed to elucidate patterns of organization of “social wholes” (Clarke 1990: 18; cf. Strauss 1990).

According to Anselm Strauss (1978b), social worlds form on the basis of a common activity that generates a shared perspective and commitment. Adele Clarke (1990:19) distinguishes between three main types of social worlds: production worlds, communal worlds, and social movements, but adds that worlds can also be mixed and perhaps more importantly, contain subworlds. Work or collective action is the business of the world and the meaning of work includes, but is not limited to, employment. Work constitutes any activity that involves the expenditure of effort in order to attain an outcome (Cuff et al. 1992: 187f.).

A number of classic interactionist studies are also good examples of studies of social worlds, such as for example Howard Becker’s (1953) study of marijuana users but especially his work on art worlds (e.g. Becker 1970a; 1974; 1982). In Art Worlds, Becker (1982) considers art as the work that some people do, and focuses on the patterns of cooperation among the people who do the work rather than the products or the artists themselves. Strauss is probably the most prominent social world theorist and has clarified and developed the social world framework in several important contributions (e.g. Strauss 1978a; 1978b; 1993).

Symbolic interactionism and social world theory have also been used in studies of science. Clarke and Gerson (1990: 180) mention a number of symbolic interactionist studies of science from the 1960’s, but note that there were no further efforts until the 1980s when studies of, for example, cancer research (Fujimura 1987), reproductive sciences (Clarke 1985), and brain research (Star 1989a) appeared. In general, studies of science based on social world theory tend to see scientific disciplines as production worlds with knowledge and its applications as as the products (Clarke 1990: 21). Intersections or arenas where two or more social worlds meet have been emphasized (e.g. Fujimura 1992b; Clarke 1990; Garrety 1998). Intersections

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5 For examples of the Chicago school and symbolic interactionism, see Park (1952); Blumer (1969); Hughes (1971a). For a discussion, see e.g. Strauss (1990).

6 However, this does not mean that products should be neglected. One can neither understand work simply by examining its products, nor without reference to its products (Star 1983: 210).
are sites where members of one social world or subworld meet members of others and transmit information, skills or resources whereas the concept of arena, or arena of concern, refers to interaction among and within social worlds around issues where actions concerning these are debated and negotiated (Strauss 1993). Consequently, these are locations where the relations between or within social worlds become visible.

Compared to the wider, richer and all-encompassing concept of culture, the social world is conceptualized as something in between a local community and a network but more closely tied to practical activities; what people do together and the shared meanings arising from this interaction (cf. Becker 1986). I find social world theory suitable for my purpose due to its empirical openness and emphasis on the activities of everyday work and the practical order of scientific production, rather than the products of science or the people involved (cf. Becker 1982). Thus, I will generally focus on working practices.

Science as Practice

Using the social world perspective in a study of science is in line with the more general trend in empirical studies of science during the last decade that has witnessed a turn from science as knowledge towards science as practice. Approaching science in terms of concrete working practices differs from the level of generality and abstraction at which most philosophers, and to some extent also sociologists, have approached science as a knowledge-producing activity, with scientific knowledge understood as a form of representation (Rouse 1996: 12, cf. Gooding 1992). If science is primarily conceived of as knowledge seeking to represent nature, it leads to questions about whether science corresponds to its object, i.e. if it is true (cf. Pickering 1992b: 5). In

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7 For instance, in *Epistemic Cultures*, Knorr Cetina (1999) points towards the general issues of knowledge production, practice, and culture and beyond the purview of single projects or central discoveries. *Epistemic Cultures* analyses material from two scientific disciplines, biology and physics, and this comparative approach is unusual in a field in which discipline-oriented case studies predominate. It is the aim of the present work to follow in these footsteps in terms of studying, and to some extent comparing, different practices within a discipline (meteorology). This will take place on the basis of an in-depth analysis of each of them. I consider this to be necessary before they can be compared, but also in order to answer to questions about their (production) relationship. Knorr Cetina’s (1999) study also represents a more ethnomethodological approach to scientific practice. For other examples, see Lynch (1985) and Livingston (1986). For an extended discussion of ethnomethodology and the study of scientific practice, see Lynch (1993).

8 However, Rouse (1996:12) suggests that it is not initially obvious that philosophical interpretations of science should focus on scientific knowledge, and claims that philosophy of science begins by considering actual practices of inquiry. Several other philosophical discussions have also emphasized practice (e.g. Hacking 1983; Giere 1988; Rouse 1987).
contrast, I will analyze what people do to produce scientific knowledge rather than the logical criteria that the end product should fulfill. Nevertheless, it may still be important to relate to these criteria, for example in terms of how the scientists themselves understand their practices. While this discussion will be pursued in one of the subsequent chapters, I will return to the question of criteria below.

Practical problems, activities, and collaborative work, in short the production process, tend to be neglected by practitioners of the science as knowledge perspective. In contrast, the science as practice perspective captures the notion of science as an activity (e.g. Pickering 1992a). In the introduction to *Science as practice and culture*, Andrew Pickering (1992b: 6f.) states,

> The attempt to understand scientific practice is interesting in its own right and also bears directly upon the development of critical and policy-oriented perspectives on science, on the concerns of cognitive science, and so on. From the latter standpoints, what scientists do is just as important as the knowledge they produce.

Scientific research is a practical activity, one that re-constructs and re-describes the world. Practice is not distinct from theory; theorizing is as much practice as any other aspect of scientific work (Rouse 1996:127, cf. Kuhn 1962/1970). Practices are centrally organized around shared practical understanding (Schatzki 2001: 2) but they are dynamic and their coherence and continuity depend on coordination among multiple participants and things (natural and human-made) and on the maintenance of that coordination over time (cf. Becker 1982).

In many ways, scientific practice is a heterogeneous enterprise, not only in terms of the differences between the natural and social sciences, but also the differences within these broad categories (cf. Galison & Stump 1996; Knorr Cetina 1999). Accordingly, it is of fundamental importance to acknowledge the diversity of meteorological research in terms of how science is practiced and not to assume that the discipline is a unified whole. Still, scientific disciplines have traditionally been conceived of as social worlds (cf. Clarke 1990; Strauss 1993). This is probably a result of the simultaneous attention to scientific work, research problems, institutional setting, and structural conditions. However, if work and problems are emphasized, disciplines (conceived of as institutional structures) are no longer the “given” units of analysis (i.e. social worlds) in studying a scientific practice.⁹ From this perspective, social world theory does not

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⁹ One may also question the role of disciplines in relation to suggestions about the new production of knowledge (Nowotny et al. 2001) and discussions about multidisciplinarity, interdisciplinarity, and transdisciplinarity. These issues are beyond the scope of this study but
preclude the researcher from focusing on a discipline as consisting of several subworlds or segments of social worlds (cf. Gerson 1983). Thus, my use of social world theory is focused on what people do together and the joint perspective this generates, rather than the explicit institutional framework they are situated in.

Regarding how perspectives on scientific practice have developed, there was an early tendency to use economic analogies in the analysis (e.g. Hagstrom 1965; Latour & Woolgar 1979/1986; Knorr Cetina 1979; Pinch 1980). Among other things, these types of models tend to imply an overemphasis on individualistic interests and to promote an internalist view on science (Knorr Cetina 1982). With this I do not mean that exchanges, resource-flows, and resource-relationships are not important, but that this economic perspective should not be seen in functional terms nor separated from the social context including both scientists and other actors. As Karin Knorr Cetina (1982) puts it, science takes place in a transepistemic arena. This notion attempts to dissolve the distinction between epistemic and non-epistemic factors, and consequently also between the inside and outside of science. Of primary importance here is how research problems are negotiated in arenas which are local, revolving around a few colleagues, but also transcend the laboratory setting and take place in a larger disciplinary field and institutional context.10

This does not imply that science is best approached from a structural or institutional viewpoint, or as governed by factors beyond local scientific practices such as science policy or institutional structures.11 Rather, a way to accommodate the notion of the transepistemic arena while retaining the focus on local scientific practice is to study how the connections with other actors, such as for example funding agencies and political actors, are negotiated from the point of view of scientific social worlds. In this book, this is of particular interest in relation to the close connection between meteorological research and the climate question. Thus, while actors such as politicians and social (environmental) movements or organizations are beyond the scope of this inquiry in the sense that they are not directly represented in the empirical material, it does not mean that they do not belong to a study of scientific production (cf. Latour 1987: 157ff.; Jasanoff 2004a). They are indeed important but become part of the context as they

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10 This can be compared to networks where a researcher with a higher rank engages in a larger community to find money, resources and attention, while lower rank collaborators stay in the laboratory or at the department (Latour 1987).

11 For examples of more policy oriented approaches, see e.g. Elzinga & Jamison (1995); Bimber & Guston (1995); Cozzens & Woodhouse (1995), all in Part VII of Jasanoff et al. (1995).
appear mainly as and when they are invoked and interpreted by my interlocutors, the researchers.

Another important issue to consider is the materiality of science, both in terms of its study objects and tools. Whereas the social world tradition includes the social dimension of science, it is less explicit about the material. Can we understand scientific practice without attending to its material objects? If not, how should we incorporate them into the analysis?

The Role of Things in (Scientific) Practice

Bruno Latour (e.g. 1992b; 1996a; 2000, see also 1992a) has repeatedly criticized sociological analysis for its lack of attention to things. According to Latour (1996b), many facets of (sociological) “society” such as durability, expansion, scale, and mobility arise from the capacity of (material) artifacts to literally construct and shape social order. Every time we find a stable social relation, it is the inclusion of material objects that creates this relative durability (Latour 1992a: 154). The social is not solid enough to make interactions last and we should therefore appeal to the innumerable material objects in our analysis of social life (see e.g. Latour 1992a; 1996a). In sociology, material objects only appear in three modes (Latour 1996a). As tools they faithfully transmit the social intention that traverses them. They cannot influence interaction, only facilitate or hinder it. As infrastructures they determine and form a continuous material base over which the social world of representations and signs subsequently flows. As screens they can but reflect social status and serve as a basis for subtle games of distinction, they become signs. Instead, Latour suggests that it should be recognized that things have the potential to act. This standpoint is closely connected to the formulation and development of the symmetry principle, briefly described below.

The Symmetry Principle – Positions

In order to understand the notion of things as potential actors, it is helpful to place it in relation to the so-called Strong Program, which is also a central contribution to sociology of science after Merton’s functionalist tradition.

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12 Things refer to all types of material objects and thus include both natural things, such as particles and droplets, and human-made things, or artifacts, such as pencils and desks. I use things and material objects interchangeably.
13 Latour (e.g. 1991/1993; 2003a) prefers to speak of a collective rather than society, since the definition of the latter traditionally does not include things.
14 I will not repeat the whole argument here but in part, Latour (1996a, cf. 1991/1993) connects the lack of objects to the split between social/natural sciences, political/objective world. This makes things untouchable for the social sciences. See also Latour (2003a).
David Bloor (1976) formulated the Strong Program as a reaction to the traditional division of labor between philosophy of science and sociology of science (Sundqvist 1991: 58). The program included four central tenets: causality, impartiality, reflexivity, and symmetry. First, social studies of science should explain beliefs or states of knowledge. Second, they should be impartial with respect to truth or falsity, rationality or irrationality, success or failure of knowledge. Third, the same explanations that apply to science should also apply to social studies of science. The fourth tenet is probably the most important in terms of the development of sociology of science from the 1970s and onwards (see e.g. Pels 1996). The symmetry principle holds that the same type of cause should explain both true and false beliefs (Bloor 1976). With the symmetry principle, Bloor rejected the ambition of finding a universal criterion for verifying knowledge that prevailed from the time of classic epistemology from the 17th century up to the philosophy of science in the 20th century (see e.g. Sundqvist 1991: 16). The quest for a universal criterion implies a unity of science that is contested in my work (cf. Dupré 1996). More importantly in this context, however, is that its aim to distinguish between true and false knowledge is exactly what a symmetrical approach to the study of science questions (at least as an analytical point of departure). Thus, Bloor (1976) also argued against the dominant sociological view formulated by Merton (1973), which held that only false scientific knowledge needed to be explained because its falsity derived from distorting social factors. In addition, Bloor further criticized Merton for considering only science as an institution as a topic for sociological investigation, rather than its content.

Following the symmetry principle, there has been a proliferation of so-called controversy studies that seek to understand the culture of science primarily by studying how the boundary between what is deemed true and
false is constructed in practice. A second major sociological approach to the study of science is the so-called Bath school, which is largely but not completely consistent with the Strong Program. The Bath school has focused on micro-sociological processes, observational methods, and scientific controversies as points of departure. Both of these traditions belong to a social constructivist approach to the study of science commonly referred to as sociology of scientific knowledge.

The symmetry principle is also important for the development of actor-network theory, or as I prefer to call it, the actor-network approach. This approach questions the idea of the human as the center of social analyses of science and instead sees entities as a mix of the social and the natural (e.g. Callon & Latour 1992). According to the actor-network approach, humans and things (non-humans) are associated with one another in networks. When a sufficient number of entities such as people, theories, and technical devices have been brought together and linked in support of a sociotechnical system, they form what is referred to as a heterogeneous network (e.g. Latour 1987; Law 1987). Since actors are the effects of networks and all entities evolve together with each other in these networks, they have to be considered simultaneously. This rejection of all dichotomies, not only between what is true and false but also between nature and society and humans and things, is referred to as generalized symmetry. The difference between what Bloor and actor-network theorists advocate can also be expressed as a difference between epistemological and ontological symmetry (Pels 1996: 297).

Furthermore, the actor-network approach rejects the social constructivism of, for example, the sociology of scientific knowledge, partly because the latter (analytically) approaches scientific truth as an outcome of negotiations between humans as the only active participants. To be universal, facts have to be spatially and temporally unlimited and therefore depend on the construction of a particularly stable network, which is only possible if this network includes things (cf. Latour 1996a). Facts are neither pure social constructions in the sense of being independent from the material world, nor predetermined by the physical world, as if scientists would have unmediated access to it. From an actor-network perspective, facts are the result of a collective process of persuasion, equipment, laboratories, paper publishing, and citations (Latour & Woolgar 1979/1986) in which people, natural things, and artifacts participate. Therefore, all these participants should also be recognized in studies of how facts are produced.

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19 See e.g. Engelhart & Kaplan (1987); Pickering (1981); Collins (1981).
20 The actor-network approach started to develop in the early 1980s. For important early contributions, see e.g. Callon (1986); Latour (1987); Callon & Latour (1981); Callon et al. (1986). For discussions about new developments and the actor-network approach more generally, see Law & Hassard (eds.) (1999), especially Latour (1999b).
One debate between proponents of sociology of scientific knowledge (Collins & Yearley 1992a; b) and actor-network theorists (Callon & Latour 1992) has been of particular importance in terms of the view on symmetry, especially in the field of *Science and Technology Studies* (STS). This debate started with Harry Collins and Steven Yearley’s (1992) attack on generalized symmetry, the alternative extension of the symmetry principle of the Strong Program. Collins and Yearley (1992) maintain that the symmetry principle between what is true and false demands a human centered viewpoint and a pure conception of entities as social or natural. If we see scientists as producing accounts of material agency, in which these accounts fall into the domain of scientific knowledge, they can be analyzed sociologically as products of human agents. But if we as social scientists try to take material agency seriously, we yield up our analytic authority to the scientists because they have the apparatus to tell us what material agents are like. We end up repeating the scientific accounts of what the world is like in a manner that is independent of the social, or at least this is what Collins and Yearley (1992a) suggest. According to Callon and Latour (1992), Collins and Yearley suggest that in practice, we should switch between natural and social realism depending on whether we are natural scientists or social scientists.

In line with the sociology of scientific knowledge and sociological perspectives more generally, social world theory is symmetrical in Bloor’s (1976) original sense but asymmetrical in the generalized sense of symmetry. From the social constructivist viewpoint of social worlds, the material, natural world becomes a baseline (cf. Latour 2004: 453); a world which social world theory leaves for the scientists to explore. As mentioned above, Latour (e.g. 1992a; 1996a) finds sociology incapable of explaining the durability of society without appealing to things, particularly in terms of

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21 For extended discussions of the debate, see e.g. de Vries (1995); Harbers & Koenis (1996); Pickering (1995), to mention only a few.

22 STS is a heterogeneous interdisciplinary field including sociological, anthropological, historical, and philosophical studies of science and technology and their role in society both on micro and macro scales. I have chosen not to review it as a unitary enterprise but only address certain issues of importance for my purpose and questions. See e.g. Golinski (1998) and Cutcliffe (2000) for the history and development of STS. See Knorr Cetina & Mulkay (1983) and Pickering (1992a) for important clarifications and programmatic standpoints. See e.g. Zuckerman (1988) for an early review of the field and Hess (1998) and Yearley (2005) for more recent overviews. See Jasanoff et al. (1995) for a broad anthology, covering several theoretical and empirical themes in STS. See Biagioli (1999) for a collection of many important contributions.

23 This does not mean that there are not other differences between social world theory and, for example, the British sociology of scientific knowledge as represented by Collins and Yearley (1992a;b). In some other respects, social world theory has more similarities with the actor-network approach (see e.g. Star 1991b). Since I primarily use the two latter approaches as theoretical tools, I will focus on discussing their similarities and differences. See Chapter Two for a further discussion of this.
While social world theory is also a target of this critique, it is important to note that social world theory and the actor-network approach can be used to answer different questions regarding stability and change of society. Whereas the stabilization of heterogeneous networks (fact construction, “black-boxing”) is a major interest from an actor-network perspective, Strauss (1978b: 233, 237) suggests that studies of social worlds provide a means to better understand processes of social change, as we confront a fragmented (social) universe marked by fluidity and movement.

Social world theory does not appeal to things to explain stabilized social interaction, but it does not completely exclude things from the analysis. The technologies of social worlds or subworlds are of importance (cf. Strauss 1978b: 237) and studies based on the symbolic interactionist tradition have incorporated the tools and instruments in analyses of scientific practice.

For instance, a number of studies have examined how scientists put instruments and tools together and under what conditions these arrangements change over time (Clarke & Fujimura 1992a). Thus, social world theorists (e.g. Fujimura 1991; Star 1991b; Clarke & Gerson 1991) admit that studies of science should include things in sociological analysis but do not grant them agency (see also Garrety 1998: 775).

Thus, if we distinguish between natural things and artifacts in scientific practice, social world studies focus on the work carried out with the artifacts (used as tools), not on the natural objects under study. This is actually a prerequisite for conceiving of scientific practice as basically any other social practice – which can be seen as a symmetrical standpoint itself – and a reason for why the social world approach does not provide any specific analytical tools for understanding scientific production. Consequently, social world analysis focuses on the practices involved in the construction of facts more generally, such as for instance how research problems are constructed (Fujimura 1987) or research fields established (Clarke 1985; 1990), rather than fact construction per se.

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24 Although Latour’s (e.g. 1991/1993) a priori assumption that society is solidified primarily by the mobilization of laboratory-made objects is questionable (cf. Pels 1996), I will not discuss it further since I am more concerned with questions of change in line with the social world tradition.

25 This does not mean that social world theory is principally uninterested in questions about durability. For instance, conventions and routines are ways to achieve stability (see e.g. Becker 1982; Star 1995a).

26 However, in Clarke and Fujimura (1992a), tools are not limited to material objects such as instruments, but also include for example theories, methods, and models.

27 Paraphrasing Latour’s (1996a) discussion, these analyses incorporate things primarily as tools or infrastructure – two of three modes of objects in which the things themselves remain asocial and unable to engage in the construction of society.

28 Compare Fujimura’s (1992a) discussion about the different foci of Latour (1987) and Star and Griesemer (1989). See also Star’s (1983: 209) suggestion that the pragmatist philosophers “Peirce, Mead, Dewey and Bentley were all concerned with delineating the relationship between scientific work, the representation of scientific process, and the development of
attend briefly to the natural things that the scientists study, in this case various atmospheric matter and processes. Nevertheless, how to include artifacts in this sociological analysis and which artifacts to include still remains to be decided.

How to Include Things in Sociological Analysis

In this work, the importance of both people and artifacts is seen as an effect of how they are incorporated into the practices bound to fact construction processes. The role in practice is taken as the point of departure both for people and artifacts. The inclusion of things as components within the human order challenges the traditional understanding of social life as assemblages of relations between people (cf. Schatzki 2001:11). However, while I agree that artifacts are essential for the constitution of social relations, my analysis does not seek to cross out, or dissolve, the border between nature and society, but primarily to expand the characterization of human-made things and include them in my sociological analysis. This is further discussed below.

Although the interpretation of an artifact is flexible, artifacts are constrained by their materiality. You cannot use a car as a helicopter or as a train, but the role a car can play in practice depends on whether this practice involves collecting, manufacturing, or racing cars. Within these different practices, the car can assume different functions and participate in, shape, guide, or exclude action as an effect of the specific social and material features of the particular practice of interest. Thus, I do not claim that we should attend to all things equally, but suggest that the point of departure should be taken in the practice concerned, which means that it may become more important to characterize the role of certain types of artifacts than others. To continue with the example from above, it is not essential to focus on the car in every analysis of social life. Because scientific practice is the focus of this work, scientific instruments become of particular importance in understanding scientific social worlds and their related practices. I propose that scientific instruments should be investigated as constitutive of scientific practice, not simply as tools in the hands of scientists (cf. Clarke & Fujimura 1992a). Using the concepts of epistemic object and technical artifact (Rheinberger 1997; Knorr Cetina 2001), which I discuss further in Chapter Two, is one of the ways in which I attempt to do so.

The concepts that I have suggested above are primarily useful for understanding the role of an object in a particular practice (or subworld). Considering that scientific practices are heterogeneous (e.g. Galison & Stump 1996; Knorr Cetina 1999) and consist of a network of practices (cf. ‘facts’.” Compare also Jasanoff’s (2004a: 22ff.) discussion of constitutive and interactive co-production.
Rouse 1996), we may also ask what links these practices and the people involved in them? In order to address the question of how different groups of actors (social worlds) manage to work together in scientific production, Susan Leigh Star and James R. Griesemer (1989) introduced the concept of *boundary object*. Boundary objects can be things, people, projects, texts, and ideas that remain flexible when they are used in sites where different groups of practitioners meet while the same things, projects etc become focused when used in particular practices. As such they facilitate articulation between different groups of practitioners involved in collaborative productive work. Consequently, the inclusion of boundary objects into the analysis serves the purpose of allowing one to study the relation between social worlds or subworlds, and the aim is to develop the sensitivity of sociological analysis to things. I will return to this question in subsequent chapters.

**About the Study**

Due to my interest in local scientific practice and everyday work, I have conducted a field study inspired by ethnography. It is therefore based on participant observation and interviews. The location for my study was the Department of Meteorology, Stockholm University. It was during the early part of this study that I identified field experimentation and simulation modeling as the two most important practices to focus on in order to understand the social world of meteorology. My field study lasted for approximately a year but was not conducted on a full-time basis. The first half mostly consisted of participant observation, including participation in meetings, seminars and numerous conversations with the staff about their work. I also participated in two field expeditions and conducted several interviews. I conducted a total of 19 tape-recorded interviews and the major part during the second half of the field study. The interview transcripts, field notes and research funding proposals constitute my primary material. In my analysis of the material, I have primarily used what is best characterized as a *hybrid approach* (Boyatzis 1998). I have identified themes inductively, inspired by grounded theory, but I have also used theories to guide the analysis in the development of meaningful themes. I will use quotes and stories based on field notes both to exemplify my material and to illustrate my points. Further presentation and discussion of the field study and analytical work are provided in the chapter on methods.
Structure of the Book

In this chapter, I have emphasized the importance of studying meteorological research practices to gain a better understanding of how climate change has been defined and how the focus on climate change interplays with social processes in meteorology. I have also argued both for the relevance of studying science as practice from the perspective of social worlds and for the inclusion of material objects in the analysis of scientific and other practices. By taking the symmetry principle as the point of departure, I have also suggested how this may be achieved by focusing on the role of scientific instruments used in practice. I thereby retain the symmetry between science and other types of practice but not between people and things.

In Chapter Two I present my theoretical perspective and analytical tools. Here, I provide a more detailed discussion of the concepts related to social worlds and actor-networks and extend the discussion of some of the issues mentioned in Chapter One. I also develop the discussion of material objects and instruments of science. The primary aim of Chapter Two however, is not to provide an extensive theoretical discussion, but rather to introduce the reader to the analytical tools that are used in the following chapters.

The main methodological considerations are presented in Chapter Three. Besides introducing my general approach, the chapter contains a description of my field study and how it was prepared, how the analysis was performed as well as discussions about data quality.

Chapter Four serves as an introduction to the field and also includes a more detailed review of earlier research. In addition, the chapter presents the setting where most of the empirical work of the study has taken place. In line with the methodological choices presented in Chapter Three, this leads to a further specification and motivation of the empirical focus of my study in relation to this setting.

In Chapters Five to Eight, I present and analyze my empirical findings. The two first of these chapters consider meteorological research like any other practice in their presentation of two social subworlds. In Chapters Seven and Eight I approach the two subworlds more as scientific subworlds and the epistemic character of practice comes more to the forefront.

Chapter Five is structured around a typology of practices and presents the subworld of field experimentalists. Particular emphasis is placed on the organization of practice and articulation work. Here I argue that the organization of collaborative research projects and the mobilization of networks for this purpose are essential features of the subworld of experimental work. This subworld is also based upon the use of measuring instruments and the role of these is another important theme of the chapter.

Chapter Six emphasizes production work and examines the various practices that make up the social world of simulation modelers. The primary “tool”, simulation modeling, has a more salient role in this chapter, in which
I examine how simulation models are used and constructed and the primary concerns that guide this work. In relation to this, I also discuss how this involves an intimate bond between the theoretical modeling and computer work and the difficulties (and possibilities) that this link creates in the modelers’ work.

Chapters Five and Six introduce the reader to field experimentation and simulation modeling separately as two social subworlds, whereas Chapters Seven and Eight explore the relationship between them. They deal with where and how the production practices of the subworlds meet in order to answer the question concerning their relationship. I am interested in this relationship both as a case of relations between subworlds and as such characterized by negotiations about issues such as boundaries and legitimacy, but also as traversing scientific production practices. In line with this shift of focus, I use the actor-network approach more explicitly in these chapters.

Chapter Seven presents an analysis of stories concerning model evaluation, or “validation”, which involves the comparison of simulation results with observations and, as such, as a “place” where simulation modeling and field experimental practices meet. While attending to the different perspectives on this activity and the relation between the social subworlds, I also frame the activity in terms of representation and fact construction. I conclude that comparison is about producing reference in simulation modeling and therefore crucial in the legitimation of this type of scientific practice, but that the consequences of this conclusion also reveal differences between the theoretical concepts I use to analyze model evaluation.

What is climate research and how is it defined? What are the climate scientists who are not working with climate models doing? How do they contribute and how do they define themselves in relation to climate modeling? These are the questions I try to answer in Chapter Eight. In current meteorological research, climate change is both an “external” problem that researchers align and adapt to, as well as a problem they participate in formulating when doing so. However, the climate question is considered in terms of how it is appropriated in meteorological research, as I implied above in relation to the notion of transepistemic arena. The climate arena is used to illuminate the relationship between the practices and subworlds, at the same time as it is crucial in understanding the relationship. In the case of the climate arena, its scientific actors include several disciplines from the geosciences as well as governmental, industrial and environmental movement actors, but their activities are not emphasized due to the focus of this study.

In the final chapter, I summarize and discuss the major findings of the study. The discussion is structured around four themes closely related to social world processes. These are subworlds and segmentation, legitimation,
arena processes, and intersection. The discussions also raise problems in relation to the actor-network approach and questions for further studies. The chapter concludes with a summary of the contribution of the thesis as a whole.
2. Science Worlds, Relations and Objects in Working Practice

As the introductory chapter suggested, the focus on practice is a central point of departure for my work. The study of scientific practice captures the view of science as an *activity*, not only knowledge and representation. This practice-orientation involves the development of an account of fields of practices and sub-domains (cf. Schatzki 2001). Activities concerning scientific practices involve machines, artifacts of different kinds as well as the objects of scientific investigation. Although technologies would not exist without the people who created them, technology is both shaped by and shapes society (e.g. Bijker & Law 1992). Practices grow from interaction between individuals, technology, and society. From this perspective, the roles assumed by people and objects depend on their incorporation into practices.

This view of practice is the theoretical foundation of this thesis. The rest of this chapter is an introduction to the theoretical concepts used in my analysis of practices in meteorological research. The concepts will then be discussed further in connection to my analyses and conclusions in subsequent chapters.

The Social World Approach

From pragmatism and the Chicago school of symbolic interactionism, four assumptions of the social world approach to sociological inquiries (of science) follow (Clarke & Gerson 1990: 181f.). 29 First, scientific facts, findings, and theories are socially constructed. Second, according to the social world approach, scientific work, institutions, and knowledge are not essentially different from other kinds of work. The third assumption holds that there is no distinction between cognitive and social aspects of knowledge, in the sense that knowledge is social and it represents and

29 For social world theory, see Becker (1974; 1982); Shibutani (1955); Strauss (1978b;1993). For an excellent review of the social world approach, see Clarke (1991). For reviews with special focus on science studies, see e.g. Fujimura (1991); Clarke (1990); Clarke & Gerson (1990).
embodies work, a particular way of organizing the world through a series of commitments and alliances. Ideas are commitments and ways of allocating resources and responding to constraints. Finally, from a social world perspective, science is best approached as a matter of work, social relations, and institutions. In line with the discussion in the previous chapter, this presentation emphasizes work and social relations rather than institutions.

Social Worlds and Work

A cornerstone in social world theory is that the meanings of phenomena derive from their embeddedness in relationships called social worlds (Strauss 1978b; see also Clarke 1990). Together, all the activities that address a given set of coherent and cohesive problems constitute a social world, which is a social whole. People in a social world share common resources but also construct shared ideas about how to go about their business and conduct debates about both their own activities and those that may affect them (Becker 1982). Becker (1982) and Strauss (1978b) focused on common activities through which both a commitment and a shared perspective develop. Becker (1960) decouples commitment from consistent behavior in his definition of commitment as a complex of side bets made by the individual, involving other interests that the individual does not want to lose. The activity-bound (group) perspective includes what is taken for granted and obvious in the world in relation to practice, and is not a separate but integral part of the social world and the practical work that generates it (cf. Becker et al.1961: 34ff.; Shibutani 1955: 564, see also Mead 1932). As expressed by members of the social world, this perspective serves to orient action and interaction and is part and parcel of the social practice of the collective.

Social world theory focuses on work and related activities and tasks. According to Strauss, (1993: 94f.), work is not restricted to what actors consider as work. It is a form of action and interaction that should not be restricted to its rational aspects but is distinguished through constraints and the exertion of effort (Strauss 1993: 81ff.). Ingrid Schild (1996: 73) suggests that Strauss is not more specific about the properties of work in relation to other forms of action because creating such distinctions outside concrete situations would be to dichotomize work and non-work. This would reify certain activities and create blind spots in empirical investigations.

Production work and articulation work are suggested as the two main types of work in science (cf. Fujimura 1987: 258; Schild 1996: 82).

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30 On the focus on work, see e.g. Becker (1970b); Hughes (1971a); Strauss (1959); Strauss et al. (1985). See Star (1991a) about the importance of work in the writings of Strauss.

31 A particular task can be production and/or articulation depending on the context, for example level of work organization (Fujimura 1987).
Production work involves carrying out a relatively well-defined task and it occurs at all levels of work organization. Schild (1996, see also Star 1995) identifies two broad types of production work. Conceptual work includes for instance the development of theoretical concepts and the related interpretation of findings, whereas hands-on (gadget) work includes preparing measuring instruments and the set up of equipment. Articulation work is the pulling together of everything that is needed to carry out the totality of production tasks (see Strauss 1985; Fujimura 1987). This involves planning, organizing, monitoring, evaluating, adjusting, coordinating and integrating activities. It is a kind of “supra-work” in any division of labor, which can be distinguished in the division of labor involved in carrying out a project – an arc of work – and many projects in a line of work (Strauss 1985: 73f.).

Social world theory focuses on how people organize themselves and addresses how they do this in relation to others who are trying to organize them and/or the broader structural situation in which they find themselves (Clarke 1991). The boundaries of social worlds may overlap or be more or less the same as those of formal organizations, but the structures of social worlds are more fluid and more open in terms of participation, boundaries, structures and processes. This distinguishes social world analysis from many other organizational theories (Clarke 1991: 129ff.).

In many social worlds there is a core of highly involved people but also marginal participants. In art worlds, for example, dressers, make-up artists, or actors with non-speaking roles represent interchangeable support personnel from the point of view of the filmmaker, who instead sees the director, producer and perhaps the leading actors as central (cf. Becker 1982). In science worlds, technical staff can be said to exemplify marginal participants, but so too are scientists who participate in several scientific social worlds or to a small extent in a research project.

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32 Strauss (1985: 73) notes how traditional writings on the division of labor have not been concerned much with the work done in the division of labor, but more so with the issues pertaining to the distribution of rewards to classes of individuals, in particular various professions.

33 Symbolic interactionism and social worlds theory have been criticized for ignoring existing structures, see e.g. Hess (1998) and Kleinman (1998) (the latter also criticizes the actor-network approach on similar grounds). However, symbolic interactionism does not rule out structural factors. According to its proponents, social world theory is particularly suited for investigating the ways in which people create, maintain and use social structures as part of their efforts to impose meanings on others (Garrety 1998: 756). “Social worlds are structural units – the structural framework – within which the negotiated social order is itself constructed and reconstructed” (Clarke 1990: 20; see also Craib 1992: 90ff.).

34 See e.g. Hughes (1971b) and Star (1991b) for important discussions. See also Becker (1970a) for a related topic in terms of the division between art and (marginal) craft in art worlds.

35 However, this does not mean that the marginal participants cannot be of more central importance in another network.
Negotiation is an important aspect of social world relations and one of the possible means of accomplishing tasks; making things work or making them continue to work (Strauss 1978a). Negotiation is therefore essential in social world processes such as segmentation, intersection, and legitimation, which include delicate issues pertaining to value, status, loyalty, and commitment (Strauss 1993: 226). These processes are briefly described below.

Segmentation into subworlds is an inevitable feature of social worlds, and raises the question of how these relate to each other as well as to the larger unit. “Most [social worlds] seem to dissolve, when scrutinized, into congeries of subworlds” (Strauss 1978b: 237), in other words, to segment. Segmentation processes include, among other things, to define and build a legitimate core activity and differentiate the subworlds from each other (Strauss 1993: 215, see also Gerson 1983: 361). When scientific worlds become segmented, these activities are justified and explained in terms of the types of problems, phenomena, and techniques involved in the work (Gerson 1983).

The process of legitimation includes the sub-processes of discovering and claiming value for the social world (or subworld) and its products, distancing the world from others, setting standards, embodying them and evaluating them (Strauss 1993: 217). Every new line of work must establish itself as legitimate in its larger disciplinary world (Gerson 1983: 366f.). In science, this involves legitimizing itself as reasonable scientific work and as technically respectable in terms of its method. Disputes over the criteria to be used in evaluating research always point to the existence of legitimacy conflict. Inherent in this work is also the setting and maintenance of boundaries between disciplines and internal disputes concerning conceptions of their own boundaries are striking features of many worlds. If these are in question, their members doubt and debate whether a given activity, person or product is “really” representative of themselves and their world (Strauss 1993: 214).

Much work within social studies of science has concerned the (constructed) boundary between science and other areas of society, especially from the perspective of sociology of scientific knowledge. The concept of boundary work is useful for this type of analysis. According to Thomas Gieryn (1999: 4f.), boundary-work is the discursive attribution of selected qualities to scientists, scientific methods, and scientific claims for the purpose of drawing a rhetorical boundary between science and some less authoritative non-science, or for demarcations of disciplines, specialties or theoretical orientations within science (see Gieryn 1983: 792). It can

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36 Boundary work is not a concept developed within the social world tradition but largely consistent with the approach.
therefore be found in scientific discourses in different types of settings. In relation to social worlds’ processes, it is likely to be closely related to segmentation and legitimization.

An intersection can involve more than two social worlds or segments (subworlds) (Strauss 1993: 217) and consists of a system of negotiating contexts in which resources, skills or information flow between social worlds (Strauss 1978b; see also Strauss 1993). In the case of intersections between social worlds, these contexts include repeated negotiations that take place among many different participants in the social worlds involved (Gerson 1983: 363).

Both within and across social worlds an arena may form. An arena is a field of action and interaction among a potentially wide variety of collective entities (Clarke 1991: 128), which therefore tend to generate disagreements about directions of actions (cf. Strauss 1993: 226f.). Conceptually, an arena refers to interaction among (or within) social worlds around issues where actions concerning these are being debated and negotiated. Individuals can do the acting, but for sociological purposes they are located in some social unit and most of the time, people negotiate as representatives of groupings (Bucher & Strauss 1961; cf. Strauss 1978a). Social worlds are invaluable for understanding arenas and social worlds often become visible through their acting in arenas, but the actions of individuals that represent them can serve the same purpose as well. However, it is important to distinguish between internal arenas, where representatives of segments attempt to negotiate, and external arenas, which include all collective actors, organizations, social worlds, ideologies, and technologies committed to acting within them (Strauss 1990: 27).

In relation to the subsequent analysis where I will use these concepts, two points are important. First, activities within all social worlds and arenas include establishing and maintaining boundaries between worlds and gaining social legitimacy (Strauss 1982; cf. Clarke 1991). The processes and entities outlined above can in other words be expected to be nested within or, alternatively, encompass each other and hence, they occur at varying scales. This also means that using all of these concepts in combination is not a sign of theoretical promiscuity or eclecticism but of the ability to attend to various social processes that are all seen as related to each other from a social world perspective. It therefore contributes to a more vivid account of the social world of meteorology. Second, the concepts of segment and arena suggest that social world theory does not include a simplistic view of the harmony of people’s perspectives, or profess that achieving or maintaining

37 I refer to discourse in the narrow sense of the word as linguistic and a specific body of representational activities rather than Foucault’s (e.g. 1972) use of discourse as a whole concatenation of activities, events, and objects, which together constitute a world-view. See e.g. Woolgar (1986) for an interesting discussion of the distinction between discourse and praxis in different social studies of science.
consensus and stable relationships is easy (or even possible) (cf. Strauss 1990: 27). As I stated in Chapter One, social world theory addresses questions of social change and while it may appear to focus on consensus, “social worlds/arena theory is a conflict theory: the generic social process is to be intergroup conflict unless and until the data prove otherwise” (Clarke 1990: 129). Thus, a common perspective and commitment has to be actively maintained by the members of the collective, and this becomes visible for instance in how the relationships to other social worlds and subworlds are constructed.

To recapitulate, science is social practice and from the social world perspective, science can and should be analyzed on the same terms and with the same tools as other such practice. When suggesting that scientific practice should be analyzed on the same basis as any other type of practice, it follows that analysis of different types of practice share similar features. For instance, analysis of politics, art, organization, business, or even warfare can provide useful analogies, as I show below. I find it most fruitful to see scientific practice not just like art or politics, yet as sharing some, not all, features with other social practices. Consequently, a social world type of analysis cannot address adequately some epistemic features of science, such as how facts are constructed and truth “discovered” in relation to the materiality of science, as I also discussed in the previous chapter.  

The general sociological approach outlined above is therefore supplemented by additional analytical tools, which are specifically tailored to the study of science (and technology). This explains why I will also occasionally refer to science worlds, as a particular type of social (production) world.

The Actor-Network Approach

A central tenet in the actor-network approach is that collective relations are constructed as a process and result of networking, including the construction of relations we refer to as social, economic, cultural, or technical. The actor-network approach is concerned with networking as an activity, not networks

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38 This does not mean that it is not recognized that there is something unique about science. For instance Star (1991b: 32) notes that scientific facts are the most naturalized of phenomena, which form our deepest assumptions of the taken-for-granted and that science as an ideology is authority and thereby legitimates many other activities (e.g. quantification, classification, sexism) in a "metasense".

39 An additional reason is its vagueness (see e.g. Rock 1979; Craib 1992). However, as Craib (1992: 91) notes, the vagueness of the concepts in interactionism is a necessary aspect when studying social interaction as a constant flux. This is what makes them sensitizing and flexible to capture the interaction in the field of research. For critical discussions on symbolic interactionism, see e.g. Reynolds (1990); Plummer (ed.) (1991).
as structures. Despite similar names, actor-network scholars claim that their notion of a network – more precisely networking – has nothing in common with the more traditional network analysis of social relations (Law & Williams 1982: 555, n.3). Compared to traditional network theories, the actor-network approach is more actor-centered but is not tied to either social actors or social relations. Network analysis has gained increased popularity among sociologists over the past decades. One principal way in which network approaches differ from conventional quantitative social science methodologies is that they attend to relational and structural connections among units, such as organizations, rather than (simply) to the attributes of such units (Scott 1987: 130). Network analysis concentrates on the density, linkages, and quantity of relationships but network studies have sometimes taken content of communication and exchanges into consideration. For important contributions see e.g. Granovetter, (1973); White (2002). See e.g. Faust & Wasserman (1994) and Scott (1991) for general theoretical discussions.

According to Latour (1991/1993), to be modern involves two practices: translation and purification. Translation involves the construction of hybrids and networks. Purification creates two ontological zones, and therefore a division, between nature and society. The modern paradox is that if looking at hybrids, there are only mixes of nature and culture, but if looking at purification, we are confronted with a total separation between nature and culture/society. Latour (e.g. 1987; 1991/1993) is interested in the practice of making this division (cf. Callon & Latour 1992). See also de Vries (1995) for a good presentation of this, and some other related points, of the actor-network approach.

See Scott (1991) for a critique of this.

See Knorr Cetina (1992; 1995) for a discussion about the laboratory as a theoretical notion.

Heterogeneous Actor-Networks

Studying science is a way of understanding society, since science is where society is (partly) made (cf. e.g. Callon & Latour 1992). A consequence of this is that the laboratory, as a theoretical notion, achieves a pivotal role. Due to its importance, the argument is presented more in detail.

The laboratory is a place to which scientists bring elements from the “real outside” to “the inside”, isolate them, and do new things to them. There is nothing special about the cognitive and social aspects of the laboratory (cf. Knorr Cetina 1981; Fleck 1935/1979). The strength of the laboratory...
originates from the fact that inside the laboratory, one can work on a small scale that enables experimentation. By multiplying experiments at low cost in the laboratory, the scientist can concentrate forces before going “outside”. On the “outside”, one has to work on a large scale and fewer mistakes are accepted. Latour (1983) relates the illuminating example of the difference between the politician and the scientist in which he points out that the main difference is material: the scientist has a laboratory where s/he can test different options whereas the politician works at the macro scale with only one shot at a time.\footnote{For a clarification of this point, see Chapter Four in Latour (1999a).}

However, the only way for scientists to retain the strengths gained inside the laboratories is by never going outside them. In consequence, they (and others who assist them) try to project to every setting some of the conditions that make possible the reproduction of favorable laboratory practices. As a result, the laboratory seems to reproduce events that appear to happen in the outside world, when it is in fact the scientists who have extended laboratory practices and made the outside world resemble the inside laboratory. Because “scientific facts are like trains, they do not work off their rails” (Latour 1983: 266), various practices to which science is “applied” need to be adapted to laboratory-like practices, for instance by measurements of various kinds. Consequently, there is nothing outside science but long, narrow networks that make the circulation of scientific facts possible. When science extends its rails by network building, it appears to come in contact with “other parts” of society.\footnote{Compare Collins and Yearley (1992a) for an opposite conceptualization of how science comes in contact with “other parts of society”.}

Thus, “other parts of society” appears to refer to a new “outside” world, but it is no more outside than for instance the subworlds of meteorology – simulation modeling and field experimentation (cf. Latour 1999a). It has simply other properties and brings people with other qualities and competences into the analysis, participating in building universal knowledge on the basis of local networks. To look at the thermometer before going outside is not only about checking if the forecast was correct and about finding out the temperature outdoors. It is also about strengthening and following the meteorological network by associating yourself and your thermometer to it (cf. Callon 1987; Latour 1987). This socio-technical network is centralized in numerical weather prediction models at the weather service centre, but you (as part of the public) as well as the meteorological researchers and your thermometer, as well as their simulation models and advanced measurement instrumentation, are all part of this network.

Networking is a relational activity. It involves the attribution of significance to particulars by putting them next to and seeing them in relation to other elements (Law & Williams 1982: 538, see also Law 1994).
Actors are the effects of this activity (originally understood in semiotic terms as actants) and are defined as any unit of discourse with a role (Callon & Latour 1981). An actor can be either a human or a nonhuman entity and can have both material and “social” components (Callon 1986). The constitutions and identities of actors are effects of shifting the network of connections with other entities. Accordingly, it is more accurate to speak of actor-networks to highlight the relational feature, rather than to refer to actors and networks as if they were distinct phenomena.\(^{46}\) For instance, it is not until after the processes of preparing laboratory equipment, performing laboratory tests, and discussing the interpretation of these tests before publishing the results in a journal, that the existence of a new protein can be established. Therefore, the protein can be regarded as an actor, but this capacity is the result of the previous work – the networking – that was conducted in “discovering” this protein.

It is important to clarify that the last two paragraphs do not imply that science imposes a single order on others who are passive, but that everyone and everything in a network is an active entity. Science and scientific results spread “because there are actors outside the laboratory who associate themselves with it” (Mol 2002: 64).

Nevertheless, the entrepreneur is a type of actor who has been of particular importance in studies using an actor-network approach (e.g. Latour 1988a; 1987). These actors resemble Schumpeter’s (1911/2000) classic definition of entrepreneurs, according to which the pursuit of entrepreneurship involves novel combinations of resources and people and the entrepreneur’s function is to bring them together, a central function that always appears mixed up with other, more conspicuous, kinds of activity. A person is an entrepreneur only when s/he actually puts together new combinations that later become routine. This means that it is a role that rarely lasts for a longer period of time. Entrepreneurship involves leadership, but as a social function and not as a difference in rank, and while it is neither a profession nor a social class the entrepreneurial focus will lead to certain positions. In terms of scientific production, entrepreneurship involves organizing projects or programs, combining ideas, material and human resources in a new way.

Although proponents of the actor-network approach have studied actors resembling entrepreneurs, this does not mean that this approach views scientific practice primarily as entrepreneurship, since the successful entrepreneurial actor is an effect, not a cause, of the expanding network. Instead, Latour more commonly compares scientific practice to politics (e.g. 1983; 2003a) and warfare (1987), from which the associations to military action, through for instance the notions of enrollment and mobilization (see

\(^{46}\) Conventional Social Network Analysis is founded on the distinction between “nodes” and “links”.

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below), follow. Science is about convincing, proving, classifying, and creating the power to do so. In a sense, this is similar to “traditional” politics, but whereas politicians focus on gaining political allies and voters, scientists rely on not only human but also material nonhuman allies (natural things, artifacts). This makes successful scientific heterogeneous networks much stronger than political (social) alliances since the inclusion of “facts” and “nature” in a heterogeneous scientific network creates a credibility capital far more stable than any politician can mobilize.

Translation Processes

In an early paper, Callon and Latour define the concept of translation as “all the negotiations, intrigues, calculations, acts of persuasion and violence thanks to which an actor or force takes, or causes to be conferred on itself, authority to speak or act on behalf of another actor or force” (Callon & Latour 1981: 279). Translation is not the same as to substitute (Callon 1986) because the character of both what would be substituted and what substitutes is constructed in the translation process: The notion of translation emphasizes how the identity of actors and their relations is always in process. Translation involves representation and a particular strength of scientists is their ability to accumulate representations of nature and society and re-present themselves as spokespersons of both (Callon 1995: 53; see also Latour 2003a). When the scientific definition of a problem is used in political discussions this activity exemplifies another translation from one thing to another.

Translation means drift, betrayal and ambiguity, and the aim of translation is to render two in-equivalent positions of interest equivalent. However, the concept has been developed in order to fuse the notion of interest and research program (Latour 1988: 253, n.16) but the meaning of interest differs from more traditional approaches. Interests should not be imputed to actors as background causes of action, but should rather be seen as attempts to define and enforce contingent forms of social order on the part of the actors themselves. “Interests are what lie in between actors and their goals” (Latour 1987: 109) and social interests are temporarily stabilized outcomes of previous processes of enrolment (Callon & Law 1982). For instance, an entrepreneur initiates a project and convinces other researchers to participate as a way to further their own interests through the aim of the

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47 For a discussion of this, see e.g. Latour (1992a).
48 Perhaps most important to compare with in this context is the notion of interest in the sociology of science developed on the basis of the Strong Program (the so-called Edinburgh school), in which interests are used as causal explanations. These interests are not necessarily the same as the actors’ accounts of their interests. See e.g. Barnes (1974; 1977).
project (the concern of the entrepreneur), but this aim also changes through the inclusion of others.

Callon (1986) presents four “moments” of translation through which researchers attempt to impose themselves and their definitions of situations on others and build networks. These moments can be separated analytically but may overlap in practice and should be seen as different modes or aspects of the process of translation; the primary concept, rather than different forms or conceptualizations. **Problematisation** is the way in which researchers seek to become indispensible to other actors by defining the nature and the problems of the latter. A common form of problematization is found in introductions to scientific papers, where researchers align their specific problems to a more general problem, for instance the exposure of a certain gene to a disease. Funnels of interests are a series of problematizations that start with a problem or an interest of general importance, and then translate this into a specific problem-solution (Callon et al. 1986: xcvii).

Problematisations suggest how problems would be resolved if the actors involved in negotiation supported the researchers’ program of investigation. This is referred to as creating an obligatory “passage point” in terms of solving the problem. In this sense translation has a strategic meaning because it defines a stronghold established in such a way that whatever people do, their practices have to pass through the obligatory passage point *in a way* that helps the contender’s interests (Latour 1988: 253, n.16). For instance, if a disease becomes defined as genetic, the search for the cause and a cure has to go through genetic research.\(^5^0\) If it is defined as a result of pollution, different research interests come into play.

Although I primarily use problematization and obligatory passage points in the analysis, it is important to mention the other three “modes”. **Interessement** refers to a series of processes by which the researchers seek to lock the other actors into roles that have been proposed for them in the program. **Enrolment** is a set of strategies in which the researchers seek to define and relate the various roles they have allocated to others. Finally, **mobilization** is a set of methods used by the researchers to ensure that supposed spokesmen for various collectivities (i.e. scallops, locomotives) are properly able to represent them.

In the early versions of what later developed into actor-network theory, **transformation** was used to describe similar processes as translation (see e.g. Latour & Woolgar 1979/1986). However, there has been a move away from transformation to translation within actor-network inspired literature (Hess

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\(^5^0\) Fuller (1996: 179f.) offers another example by considering explanation as social practice. By providing an explanation of, for example, the behavior of individuals, individuals are treated in a more uniform way than before. In order for the explanation to succeed, the relevant individuals must exchange their voices for that of the explainer and go through the explainer to have their interests served. If the benefits of consent outweigh the costs of contest, the explainer has become an obligatory point of passage.
1998). Latour’s (1998: 424f; see also Latour 1987) characterization of scientific visualizations serves to clarify a different conception of transformation that is not equivalent to translation.

Scientific visualizations such as diagrams and data plots involve a transformation of information and generate a special form of transfer: a reference. Information is what puts something into a form. To travel over “distance”, matter has to be changed into appropriate forms, involving the processes through which data is transformed into representations of data and temporarily fixed. If there is no transformation in the sense of encoding or inscribing into a form, transportation is not possible and the only way to know is to “be there”. As soon as something is at a distance from the features one wishes to refer to, some vehicle has to be invented to carry the reference, now in a completely different state than the one it had before.

Hence, maintaining a constant through transformation is not about conveying the essence of things themselves, but rather maintaining the aspect that is relevant to scientific practice through successive transformations of the medium. Creating fixed, measurable, and standardized categories is fundamental for this purpose. This does not mean that information is simply transferred, but that it is transformed from one medium to the next (Latour 1998: 425; cf. Latour 1995). However, the intermediary steps that are taken in the construction of an image or representation gradually disappear into a shape that the scientist keeps as a reference through those re-representations (i.e. transparency). Thus, transformation implies a change from matter into form, as for example in the case of the change from a filter to a diagram, and this is facilitated by inscription-devices that are discussed below.

From an actor-network position, I propose that transformation can be viewed as a fundamental subgroup of translation, crucial for the materiality of fact construction. It is also important to emphasize that it is by producing a reference that science “enrolls” artifacts and nature itself, and thus gains (scientific) legitimacy. In order to produce a reference, translations and associations of allies are of fundamental importance. Subsequently, however, the “political” support that was required to construct a fact becomes invisible and truth appears to “speak for itself”, as opposed to through a number of allies that enabled it to do so. From an actor-network perspective, the division between epistemology and politics becomes a result of this (cf. Latour 2003a). As Latour (1983) puts it, science is politics pursued by other means.

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51 Immutable mobiles are flat (papers), optically consistent, combinable, and mobile (Latour 1990) and carry the constants through the transformation of the media. Inscriptions (see below) and money are both types of immutable mobiles. Immutable mobiles per se are not specific to scientific practice but according to Latour (1998: 426), they capture what is scientific in a line of mediation that are similar to those found in other visual activities. See also Star (1991b: 91ff.) for a discussion.
Objects in Scientific Practice

Many things must be in place for science to be done and artifacts play an important role in scientific practice, both as study objects and as tools. In the following, I discuss two conceptions of different types of objects that are important in scientific working practice. The first section concerns objects that are important in the production system of single social worlds or subworlds. These are material objects primarily used as tools of scientific inquiry, rather than being objects of scientific inquiry themselves, but they can also serve as such. The second section concerns boundary objects, a useful concept in analyzing relations between different social worlds.

Scientific Instruments

Instruments of different kinds hold a central position in scientific practice and therefore also in the main activities of scientific social worlds. They participate in the creation of roles in social worlds, in their activities and discourses. This also means that they are essential in constructing “the scientist” (as a role). The following passage from Latour’s (1999a; see also 1994) discussion about technical mediation is illustrative in this context.

You are different with a gun in your hand; the gun is different with you holding it. You are another subject because you hold the gun: the gun is another object because it has entered into a relationship with you. (…) A forsaken gun is a mere piece of matter, but what would an abandoned gunner be? A human, yes (…), but not a soldier (Latour 1999a: 179, 192).

This illustrates how the relation between human and non-humans defines them both. Of primary importance here is the implication this has for science. With a (certain type of) instrument, you become a scientist. Scientists are people with instruments. The type of scientist you are depends on the instrument you work with, it may be a measuring instrument, a detector, or a simulation model, but also what you do with it and your relation to other actors. Do the circumstances in which technicians work with measuring equipment contradict this idea? From an actor-network perspective it is questionable whether it matters who actually operates the instrument, since the ”scientist” as an actor is constituted by the whole network of human and nonhuman resources s/he is centered in, rather than by his or her immediate, direct relation to the instrument. For instance, an experimentalist is defined in relation to modelers and theoreticians. Scientific instruments are seamlessly integrated into scientific practice and

52 This also illustrates the point discussed earlier in the chapter about the relational meaning of actors-in-networks.
the social worlds of science, and my aim is to characterize the role of these objects through practices.

Latour (1987: 68) defines a scientific instrument or inscription-device as “any set up, no matter what size, nature and cost, which produces a visual display of any sort in a scientific text”. Inscriptions are things that have been transformed into paper (see Latour 1987; Latour & Woolgar 1979/1986), for instance when samples of water are put into an ion chromatograph for chemical analysis, resulting in a diagram (an inscription) that describes the ion content. Inscriptions are mobile, immutable, and flat and their scale can be modified (Latour 1990: 44ff.). They are also presentable and readable and can be reproduced, recombined and superimposed. This means that inscriptions mixed together from various disciplines may appear similar because of their form. Scientists can therefore calculate only with inscriptions in what are referred to as centers of calculation (Latour 1990: 59). Perhaps the most important feature of inscriptions is their capacity to be made part of written text. Consequently, in a wider sense, reports and articles produced further along the translation chain of scientific production can also be referred to as inscriptions (cf. Callon 1995).

Latour (e.g. 1983; 1988a) bases his analyses of scientific practice on the final products of science, i.e. published texts such as articles or books. Griesemer (1992), however, argues that if we are interested in both the process of scientific production and in the products, we should take into account the material used to support scientific claims and interpret these elements in terms of the material basis for theory construction. This involves emphasizing the work with inscription-devices per se, which complements the study of the inscriptions they produce. This is also more consistent with the social world approach (cf. Strauss 1993; Becker 1982). To develop the analysis of the role of certain inscription-devices in the practices of interest in this work (i.e. measuring instruments and simulation models), I have found the ideas of Hans-Jörg Rheinberger useful.

**Objects of Experimental Systems**

According to Rheinberger (1997), *epistemic things* and *technological objects* are two different but inseparable components of experimental systems. Experimental systems are the smallest integral working units of research. They are systems of manipulation designed to provide answers to the questions that experimentalists can only ask with their help; they are vehicles for materializing questions (Rheinberger 1997: 28).

Epistemic things, the research object or scientific object, are material entities or processes that constitute the object of inquiry. Epistemic things are at the center of research but they need to be defined since they tend to have a characteristic vagueness. This is inevitable because epistemic things embody what is in the process of becoming known. Thus, epistemic things have the precarious status of being absent in their experimental presence.
Rheinberger (1997: 29) quotes Latour (1987: 87f.) when he claims that it is characteristic for the sciences that:

The new object, at the time of its inception, is still undefined. At the time of its emergence, you cannot do better than explain what the new object is by repeating the list of its constitutive actions. The proof is that if you add an item to the list you redefine the object, that is, you give it a new shape.  

To enter such a process of redefinition, scientists need to establish the necessary experimental conditions through which the objects of investigation articulate themselves in a wider field of epistemic practices and material cultures, including instruments, inscription devices, models and associated theories (cf. Latour 1987). In contrast to epistemic things, experimental conditions tend to be determined by standards of purity and precision.

Whereas epistemic things are “question-generating machines”, technological objects are “answering machines” (Rheinberger 1999: 312). Technical objects resemble Latour’s (1987) notion of black box, but Rheinberger (1997: 30) claims that the notion of black box primarily reflects the aspect of routine in the production process. Within a particular experimental system, both types of elements are engaged in an important interplay, both in time and space. The function of an object as an epistemic or technical entity depends on its position in an experimental context and cannot be decided on beforehand. The two types of objects are distinguished by their function that defines “places” or positions within the system. They can also change places since they are engaged in a relation of exchange and mutual transformation. Technical conditions determine the realm of the possible representations of an epistemic thing, and sufficiently stabilized epistemic things are integrated into the technical repertoire of the experimental arrangement.

Knorr Cetina (1999, 2001) develops Rheinberger’s (1997: 28f.) distinction between two elements of experimental systems and introduces the notion of epistemic object, characterized by its lack of completeness of being, as opposed to the everyday conception of objects as whole and solid things (Knorr Cetina 2001: 181). Epistemic objects are central in epistemic or objectual practice, based on a relational dynamic between individuals and nonhuman objects (rather than a tie characterized by routine) (Knorr Cetina 2001). Knorr Cetina (1997) problematizes the role of (some types of) technology as merely instruments, but Martina Merz (1999: 295) points out that by including technologies such as computer programs in the category of

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53 In Pandora’s Hope, Latour (1999a) further develops this idea.
54 If epistemic things and technical objects are engaged in a relation of exchange and mutual transformation, why not cancel the distinction, asks Rheinberger (1997: 30) “Does it not simply perpetuate the traditional problematic distinction between basic and applied science, between science and technology?” Rheinberger’s (1997: 31) answer is that the division helps to assess the game of innovation. It highlights the temporal aspects of scientific work.
epistemic objects and by excluding “instruments” and “tools” from the analysis, Knorr Cetina displaces the dividing line that Rheinberger introduced between the different object types. Where Rheinberger describes a dynamic of exchange and mutual transformation between the two object types that is effective within an experimental system, Knorr Cetina (1999, 2001) introduces a different kind of dynamic, actuated by the object’s unfolding ontology and lack of completeness of being (cf. Merz 1999).

In the context of the (social) practice approach I have outlined above, it would be inconsistent to take the object as a point of departure, without situating it in practice and its social setting. The point of departure is that both the role of the human and the material object depend on how they are incorporated into practice. This means that my use of Rheinberger’s ideas shifts the dynamic in a different direction (compared to Knorr Cetina 2001), towards the position of the things/objects in practice, making it into a type of role-repertoire for things/objects, depending on their position in different scientific practices. This is of crucial importance in relation to my focus on the scientific instruments and tools that scientists make use of, rather than natural things, because it means that it is not given in measuring instruments, simulation models or in short, any type of scientific instrument, which roles they will play in practice. Thus, a computer program for simulation experiments can be used simply to produce output, but so too may it act as a stand-in for natural processes in scientific investigations.

This also has consequences for my terminology. I utilize Knorr Cetina’s (2001) notion of epistemic object and its unfolding ontology, but due to the focus on the role in practice, I do not share Knorr Cetina’s (2001, see also 1999) lack of interest in the relation to technical artifacts a priori. Furthermore, since I only use the notion of epistemic object to characterize scientific instruments, object is a more suitable label than the (more) undefined “thing”. However, technical artifact is merely a semantic change compared to Rheinberger’s (1997) original term, simply to make a clearer distinction from epistemic object.

Boundary Objects

The concept of boundary object was developed for things that exist at the junctures where social worlds meet. It was introduced in order to address the question of how actors can co-operate in scientific work in spite of the heterogeneity of perspectives among actors belonging to different social worlds. According to Star and Griesemer (1989), co-operation is necessary to create common understanding, to ensure reliability across domains, and to gather information that is able to retain integrity across time, space, and local contingencies. It does not, however, presuppose consensus. Nevertheless, the varied commitments of different actors will not result in a particular sort of
scientific practice unless they are translated and made understandable to other actors.

In order to create scientific authority – methodological or substantive – scientists (“entrepreneurs”) gradually enlist participants (allies) in the process called *interessement* (cf. Callon 1986). This indicates the translation of concerns (of the non-scientist into those of the scientist), but according to Star and Griesemer (1989: 389) a central feature of this situation is that “entrepreneurs” from more than one social world try to conduct such translations simultaneously. Unless they use coercion, each translator must maintain the integrity of the interests of the other audiences in order to retain them as allies in a way that ensures that the centrality and importance of the entrepreneur’s work is increased.

In the original model (Latour 1988; Callon 1986), translation involves reframing or mediating the concerns of several actors into a narrower point. It is necessary to pass through this point in order to achieve the aim or produce the intended product. The story about this is necessarily told from the point of view of this point of passage, which acquires a gatekeeping function in the network. In Star and Griesemer’s (1989) rendition, however, several obligatory points of passage are negotiated with several kinds of allies. The coherence of a set of translations depends on the extent to which entrepreneurial efforts from multiple worlds can coexist independently from the nature of the process that produces them. Translation is indeterminate since there is an indefinite number of ways entrepreneurs from each cooperating social world may make their own work an obligatory point of passage for the network of participants. Once the process has established one of these points as central in some sense, it has to be defended against other translations that may threaten to displace it. For example, if medication has been claimed to cure a physical disease, it is in the interest of medical companies to defend this definition of the problem from suggestions that the problem is psychological and caused by the social environment.

Although it is problematic to accomplish translation without alienating the actors through means that are perceived to be over-controlling, it can be achieved through a reliance on boundary objects, which temporarily act as anchors or bridges (Star & Griesemer 1989: 414; see also Star 1989b). Boundary objects can be things, people, projects, texts, and ideas that facilitate articulation between different groups of practitioners. For example, in Star and Griesemer’s (1989) analysis, repositories like libraries, ideal types such as diagrams, and standardized forms exemplify boundary objects. Boundary objects remain flexible when they are used in groups but more focused when used in individual sites or, rather, in particular practices. There is an interpretative flexibility, while the material form may be the same and members of different groups, or social worlds, can read different meanings
into the same object according to the particular position the object has in their practice (Star & Griesemer 1989: 393).

**Boundary Objects and Networking**

Although the idea of a boundary object originates from the social world perspective on science and technology studies (e.g. Fujimura 1986; Star & Griesemer 1989), the connection to the translation model (e.g. Latour 1988a; Callon 1986), and thus the actor-network approach, is important for two reasons. First, it shows how the social world approach can be *combined* with the ideas of the translation model (but not necessarily with the actor-network philosophy as a whole). Second, the connection to translation processes sharpens the notion of the boundary object, which otherwise appears to be quite vague. I briefly discuss the first point below and return to the second in the analysis and conclusions, where I attempt to further clarify and develop the concept.

Joan Fujimura (1992a) makes a brief analysis of boundary objects and relates this to the network-building concept of Latour (e.g. 1987). Latour’s presentation of network building, primarily in the case of Pasteur (1983; 1988a), has been criticized as being “Machiavellian” by studying scientific practice only from the perspective of the manager (or entrepreneur) trying to enroll allies (e.g. Amsterdamska 1990; Star 1991b). By seeing scientific work as collective action from the viewpoints of all the worlds or (human) actors involved, the aim is to avoid this one-sidedness. In Star and Griesemer’s understanding (1989), boundary objects emerged through a process of mutual interaction and were not engineered by one individual or group.

Boundary objects promote collective action and coherence of information from different sites because they are more easily reconstructed (re-represented) in different local situations to fit local needs, they are equally disadvantageous for establishing the networks of ‘stabilisations’ of allies.

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55 This is also a consequence of the fact that these objects are marginal to those worlds or subworlds (Star & Griesemer 1989: 412). While measuring instruments and simulation models may play different roles in different practices and among different participating groups in collaboration, they do not necessarily serve as boundary objects in the relation (collaboration) between them. In other words, instruments may function as both epistemic objects and technical artifacts in different practices, but it is the function that this double role has in the collaboration between these practices that determines whether it is properly conceived of as a boundary object.

56 Nevertheless, Fujimura (1992a: 172) also notes the limitations of Star and Griesemer’s approach and they have also pointed out that their analysis still has a managerial bias (Star & Griesemer 1989: 390). In this context it is also worth repeating Garretty’s (1998) assertion that symbolic interactionists often seek out people in lower positions and the voices of the marginal (see e.g. Star 1991b; Fujimura 1991) rather than analyze power. However, according to Garretty (1998: 756) “social world theory is particularly suited to the investigation of the ways in which people create, maintain and use social structures as part of their efforts to impose meaning on one another.”
behind ‘facts’, which Latour (1987) discusses. That is, while boundary objects can promote translation for the purpose of winning allies, they can also allow others to resist translation and to construct other facts. (Fujimura 1992a: 174f.)

Thus, the use of boundary objects leads to a focus on collective practices involved in constructing facts, *rather* than concrete fact construction *per se*. I further suggest that this is a programmatic difference between social world theory and the actor-network approach more generally. This can also be compared to the consequence of focusing on practices: if the scientists are seen as derivative of practices, the production system rather than the production of single scientific results becomes of concern (cf. Knorr Cetina 1999: 11f.) and this is of importance in developing the sociological analysis of scientific practice.

**Combining Theoretical Concepts**

Concepts are part of theoretical perspectives and successful combinations of concepts from different theoretical perspectives are therefore only attained if the concepts are epistemologically and ontologically consistent (Clarke 1991: 146). It is therefore important to discuss the differences, problems, and possibilities that arise when ideas and concepts from different theoretical schools are employed in the same analysis.

**Similarities, Differences and Problems**

Constructivism, relativism, and focus on relations are some aspects that social world analysis and actor-network analyses have in common (cf. Clarke & Montini 1993: 45). However, while the actor-network approach

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57 Fujimura (1992a) also claims that the concept of standardized package (Fujimura 1987; 1992b) facilitates both collective work by members of different worlds and fact stabilization, but this concept will not be used here.

58 As a matter of fact, actor-network “theory” is to a large extent a methodology to study science symmetrically from the “outside”, in the sense of studying (or rather learning) what scientists do, but does not use their stories to understand it. The methodology is most clearly described in *Science in Action* (Latour 1987). The methodological principles are also stated and discussed by Callon (1986). See also Law’s (1994) dialogue with more traditional sociology. Yet to call it a methodology is to neglect the more “philosophical” ideas. As Barry and Slater (2002: 178) note, “actor-network theory was always much more than an approach to the sociology of science and technology. It was offered, rather, as a way of rethinking the very idea of society as a domain distinct from nature and technical artifacts.” According to Latour (1988a), the actor-network approach is an “empirical philosophy” (see also Mol 2002). However, in my work, I will, first, use the concepts (or vocabulary) of the actor-network approach as analytical tools and thus as if it was a more traditional “theory” and second, abstain from taking the full implications of this “empirical philosophy” into account in terms of its view on actors etc. See more below.
emphasizes translations and networking and the importance of paying equal attention to humans and nonhumans, the social world approach (traditionally) concentrates on negotiations and collaboration or cooperation between humans only. As I discussed in Chapter One, scholars drawing upon the social world approach (e.g. Clarke & Gerson 1990; Star 1991b; Fujimura 1991; Clarke & Fujimura 1992) agree with actor-network theorists that objects should be included in sociological analyses of science, but they do not grant them agency and therefore incorporate them differently into the analysis.

The actor-network approach proposes a poststructuralist view of the actor as dependent on the relations it is bound into and how it is that effects are generated as a function of their location in a set of relations (cf. Law 1994: 13). As a consequence of the generalized symmetry principle, the actor-network approach does not differentiate between semiotic actants of different types – i.e. humans and nonhumans. However, humans are the only type of sociological actors capable of some type of actions. For example, in terms of the delegation of performances between humans and nonhumans (e.g. Latour 1992b), it is one thing to delegate human performances to a machine and quite another to delegate machinic functions to a human (see e.g. Pickering 1995; Pels 1996).

Olga Amsterdamska (1990: 501, see also de Vries 1995) has also questioned if it is actually the same thing to enroll, for example, electrons and industrialists, when

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59 This terminology has been criticized as simply a replacement of a more traditional language without adding any new content (see Collins & Yearley 1992). However, the new terminology is important from the point of view of the radical symmetry principle in relation to conventional social theory.

60 Law (1994: 13) suggests that like structuralism, actor-network stories describe, they do not explain. According to Fuller (1996: 179f.), actor-network analysis involves the study of the textual traces of scientific practice as the (only available) representations of the material agents and that these representations only have the meaning they obtain from the network of textual relations in which they participate. Explanation is also representation: the translation from one discourse to another. Thus, what an actor-network scholar strives for are descriptions of flat translation chains that do not reduce nor attempt to reach a more abstract level. As Law (1994: 16) asserts, “we are giving up the explanatory resources of sociology.” “The ideal of explanation (…) is not a desirable goal” (Latour 1988b: 164). See Fuller (1996) for a discussion and Amsterdamska (1990: 503) for a critique.

61 Cockburn (1993; cf. Landström 1998: 25) criticizes that by letting actors exist only in and through a particular network, actor-network theory ignores differences in attributes among (human) actors. For instance, Cockburn (1993; cf. Landström 1998: 25) points out that the chance of becoming a successful entrepreneur is likely to be affected by gender and that men are more easily accepted as entrepreneurs (cf. Star 1991b; Wajcman 2004). On the basis of this, Landström (1998: 31) suggests that actor-network theory is blind to power relations not directly connected to the ability to classify and categorize nature and society, which seems to be the only distinction of importance. However, more recent contributions in line with actor-network theory and material semiotics problematize the attributes of people to a larger extent in terms of how difference come to be made relevant in particular situations. See e.g. Moser (2004) for an example.

62 This also suggests that a generalized symmetrical analysis has limitations and this is a problem that even “pure” actor-network analysts probably cannot solve.
considering the goals, means, and results of the enrollment of such different allies. Thus, while things and people can be equated as potential actors in a semiotic analysis and in texts and verbal interaction, it is not necessarily possible in (material) practice. My analysis focuses on practices from a sociological viewpoint. It is therefore questionable to what extent agency is as easily exchanged between people and things and if it is reasonable to see things as capable of anything else than (attributed) behavior, as opposed to intentional action in a sociological sense.63

The social world approach has a more intersubjective view of the actor in line with symbolic interactionism, where the self is seen as an interactive product that reproduces itself in further interaction and performance (e.g. Blumer 1969).64 Work is a central human activity that reflects both interaction and the self (Hughes 1971a; cf. Strauss 1990). Thus, within the tradition of social world theory, the development of knowledge has been analyzed as a process that occurs within an arena of multiple human viewpoints, in which the central role of humans in allocating meanings to ambiguous scientific objects has been highlighted (Garrety 1998: 755).

My work focuses on practices. The roles of both people and objects therefore depend on their incorporation into these practices, more specifically working practices, in line with social world theory. As such, it also differs from the actor-network approach in the respect that it concerns how things are concretely used and incorporated into practice rather than what is written about them. As Steven Shapin (1988: 546) has pointed out, Latour (e.g. 1987) emphasizes the rhetorical, representational and literary practices to such an extent that he gives the impression that these are almost the only things scientists do in their everyday work.

63 However, Latour and Callon (1992: 356) claim that they “do not deny differences; we refuse to consider them a priori and to hierarchize them once and for all. One is not born a scallop; one becomes one.” It is thus not about admitting intentionality to things or seeing humans as mechanical machines, but to treat them as if they are alike and their performances (what they do) precede their competence (what they are). Nevertheless, Pickering (1995: 19) suggests that Latour (e.g. 1987) often seems to have a rather distinct notion of human intentionality, for instance in terms of translation of interests, which Pickering only finds applicable to (intentional) human agents. More importantly in this context is that while this analytical strategy might work well in a semiotic framework, it is not equally combinable with social world theory or with sociological theories more generally. As Michael Lynch (1993: 109) has noted, there is a considerable difference between dealing with the actor-network framework on its own terms, as an attempt to make a (material) semiotic analysis, or as read or used in a sociological analysis, making some of the statements absurd. Furthermore, it is questionable if this is only to be considered as an analytical strategy, taking into account that the actor-network approach claims to address ontological questions. This is implicit in the debate with Collins and Yearley (1992a; b) (cf. de Vries 1995) but much more explicit in later work (e.g. Mol 1999; Callon 1998). On the debate, see Chapter One. For a further discussion of the questions of ontology in the actor-network approach, see Chapters Seven and Nine.

64 See Strauss (1990) for a general discussion of the Chicago tradition’s theory of action and interaction. See also Strauss (1985: 78f.) for a discussion of action and actor.
This issue can also be related to another major difference between social world theory and the actor-network approach. This concerns how they approach the study of practice as their units of analysis. What analytical stance is employed and prescribed? Whereas I find that social world theory analyzes practice conceived in terms of work and activities from what Gubrium and Holstein (1997) refer to as a naturalistic and, to some extent, a constructivist stance, the actor-network approach mixes constructivism and postmodern discourse analysis in the semiotic approach to practices. I will return to this question in the next chapter, but also in relation to the concrete analyses.

Possibilities

In a statement of her general research interest, Fujimura (1991) captures the (social) area of interest from her social world perspective in relation to and inspired by the actor-network approach, particularly by stating: “while I am clearly interested in non-humans (especially since technologies are human constructions), my interest is organized by the humans who care about, fight over, and commit resources to these things” (Fujimura 1991: 222). She (1991: 222) further asserts that, “in contrast to Latour, I am still sociologically interested in understanding (…) why and how some human actors resist being enrolled.”^65 I agree with this standpoint, but Fujimura’s (1991) discussion also implies that from a sociological perspective, the social world approach and the actor-network approach can complement each other. At the same time, some actor-network ideas, not least the semiotic interpretation and generalized symmetry, disappear when they are incorporated into the framework of social worlds and the view on practice that follows from this. This means that the equal attention to people and things, proposed by the actor-network approach, is neglected. My work is guided to a greater extent by the collective commitments of people. However, I will use concepts from the two perspectives to address a wider set of issues in scientific practice than would have been possible from the point of view of only one of them. These issues are briefly described below.

As a consequence of the acknowledgment of the disunity of scientific disciplines, I will analyze the social world of meteorological research as an arena in order to study internal complexities, conflicts and subworlds (cf. Strauss 1978b). As it was noted above, social worlds tend to dissolve into subworlds when they are studied (Strauss 1978b: 237).^66 Which subworlds

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^65 As a consequence of the point of departure in scientific practice, this study does not attempt to emphasize people and their differences, such as for instance in terms of gender and class, nor for instance *gendering* or *gendered* scientific practices. For overviews of this type of research, see e.g. Wajcman (2004); Berner (2004).

^66 In practice, this means that subworlds may also segment into even smaller subworlds. However, if related to other aspects of social world theory, analytical dissection will not go on
are of importance in meteorological research? What are their working practices like and how are they organized? These are some of questions I will address. I will also pay specific attention to the role of their central technologies, conceived of as inscription-devices and epistemic objects or technical artifacts, in relation to the question of practice and organization. In addition to the internal arena of meteorological research, I will approach the special interest in the climate change question, stated in the first chapter, as an external arena (cf. Strauss 1990: 27) but retain my focus on the meteorological actors.

Practices never exist in isolation from one another and typically need other practices to provide necessary commitment and resources (Rouse 1996: 156f.). This fact is partly captured by the notion of intersection, which suggests that social worlds or subworlds sometimes depend on resources provided by other worlds. Intersections can be of varying degrees of intensity, duration and significance (Strauss 1993: 217). However, an important question is to what extent the exchange of resources at an intersection is working and the exchange accomplished? Although an intersection is important for a production network or process that traverses, or bridges, the activities of social worlds or subworlds, and a subworld depends on some resources from another subworld, this does not mean that the provision of resources is free from struggle or negotiation.

According to Strauss (1978b: 257), a major analytical task is to discover such intersections and to trace the associated processes, strategies, and consequences. The concept of boundary object is a step in this direction but it primarily relates to how various social worlds, or subworlds, involved in scientific production manage to work together in spite of their differences. It is more about articulation and how production work is organized, than about the process of production itself and its crafting of products. Therefore, I will use the concept of translation as a way to find out how association takes place in order to analyze how field experimentation and simulation modeling – as practices – are associated with each other in production networks. Thus, by combining concepts from the two approaches, I will analyze both how the practices are related in a network of fact production and how the perspectives in social subworlds, formed on the basis of their activities, affect this relation. Can we identify boundary objects? If we can, what translations do they enable?

To conclude, I suggest that the combined use of concepts from social world theory and the actor-network approach create possibilities for an analysis of scientific practice, in spite of the differences I have discussed forever. In any case, the analytical construction of such units should be guided by the fruitfulness of such units in order to understand the empirical phenomena.
above.\textsuperscript{67} However, I will seek to make visible the differences as and when these lead to inconsistencies or diverging results in the analysis. It is also important to emphasize that each of the empirical chapters should, to a certain extent, be regarded as separate analyses. These chapters form a whole but they do not add up completely (cf. Law & Mol 2002). Furthermore, the approaches and concepts outlined in this chapter are – perhaps obviously – not the only possible ones but they are definitely among the reasonable options.

\textsuperscript{67} Strauss (1990: 22) states explicitly that he does not assert that people working in the Chicago tradition should not borrow and elaborate suitable concepts from other traditions.
3. Methods: A Qualitative Study

Research is a journey lined with repeated choices, selections, delimitations, and attempted solutions to the problems generated by these procedures. In addition to an introduction to the general design of the study, this chapter contains sections about the choice of location and delimitations of my material, my fieldwork including observation and interviews, and my analysis. I have decided to present my work in a narrative form and describe it in fairly concrete and detailed terms, in particular regarding the field study, since this is the primary basis for my data collection and analysis. Thus, the aim is to give a transparent description of my work. At the same time, it is impossible to discuss all possible alternative choices. Nevertheless, this narrative includes indications of the problems encountered and what motivated my choices along the way. These are based on methodological and practical considerations, but also on considerations of fruitfulness (cf. Blumer 1969: 40). Methodological issues are also occasionally discussed in later chapters when this is deemed of relevance for evaluating specific issues, data, analysis or conclusions.

How and Where to Study Scientific Practices

On the basis of Kuhn’s work (1962/1970), it has been concluded that some of the most important values governing scientific practice are local and specific to sub-cultures that are smaller than the discipline (Golinski 1999: 22). This is a point of departure for this thesis and also one of the insights related to the so-called laboratory studies that became popular in the study of scientific practice in the late 1970s. Laboratory studies are based on the identification of the laboratory as a crucial setting for the production of knowledge and the early laboratory studies are classics among contemporary studies of scientific practice (e.g. Latour & Woolgar 1979/1986; Knorr Cetina 1981; Collins 1985; Lynch 1985). They influenced my decision to gather most of my empirical material through ethnographic work consisting of participant observation and interviewing at the Department of Meteorology, Stockholm University.

In Sweden, meteorological research takes place at three departments or sections at Stockholm University, Uppsala University, and Göteborg University. There is also a research department at the Swedish weather
service, SMHI. The Department of Meteorology at Stockholm University, MISU, is the largest university based research locality and it has also been considered the center of climate research in Sweden (Elzinga & Nolin 1998).

Because of my theoretical point of departure in social world theory and the incorporation of ideas from the actor-network approach, this is a qualitative study that is inspired by the ethnographic tradition of inquiry (see e.g. Creswell 1998). Ethnography generally involves the description and interpretation of a cultural or social group or system. However, Creswell (1998: 59) notes a distinct lack of orthodoxy in ethnography as a general approach and of the resulting need for researchers to be explicit about what “school” they espouse. I have primarily relied on procedures in the sociological approach presented by Martyn Hammersley and Paul Atkinson (1995). According to Hammersley and Atkinson (1995: 206), ethnographic research tends to have a broad focus that becomes progressively sharpened through development, clarification and delimitation of the research problem as the inquiry proceeds (cf. Blumer 1969: 38, 40). This is an important reason for a detailed description of the study, since its focus has developed during fieldwork. An ethnographic study involves the researcher’s participation in people’s daily lives for an extended period of time, watching what happens, listening to what is said and asking questions (Hammersley & Atkinson 1995: 1). This may include both observation as well as active participation in the social interactions of the field.

Interviews have been used to illuminate and deepen findings from participant observations (cf. Hammersley & Atkinson 1995: 226; Leppänen 1997). Experiences gained from participatory observation were also of great value in constructing relevant interview questions. In addition to participant observation and interviews, the collection of data material in ethnographic research can also include documents and did in this case. It is often claimed that triangulation provides a more complete picture of the area of research and thus as a reason for using a combination of techniques (Denzin 1978, cf. Fransson 1997: 52). However, different research strategies produce different data (Hammersley & Atkinson 1995: 133). It is also important to keep in mind that different techniques can also mean studying different things.

A simple factor that is often glossed over in terms of selecting field settings is geographic proximity (Punch 1986: 22). In my case, it was a great practical advantage that the distance between my own department and MISU, which is situated at the same university, was only a few hundred meters. This made the whole data collection process easier and simplified a continuous relation with people at the department. In some ways it has been a problem to study people working in the same organization but that is a different problem tackled below.

Traditional distinctions between the different roles of the observer include (complete) observer, observer as participant, participant observer and (complete) participant (see e.g. Adler & Adler 1994). In most situations, my role has been observer as participant.

For an overview of qualitative methods, see e.g. Denzin & Lincoln (1994).
Therefore it is essential to reflect on the purpose of triangulation and not take its usefulness for granted.

The use of interviews to find out about scientific practice has been criticized (e.g. Latour & Woolgar 1979/1986; Mul kay 1974, Mulkay et al. 1983). According to the actor-network approach (e.g. Latour 1987), scientists’ accounts should not be used as sources of information about what they are doing, only about how they do it. This was first stated in *Laboratory life – The (Social) Construction of Scientific Facts*, in which Latour and Woolgar (1979/1986) (primarily) studied what scientists said and did, as opposed to what they said about what they did. Thus, they see interviews as less suitable than observations, which are free from the actors’ (in this case researchers’) subjective understanding and interpretations of activities and events. However, from a social world perspective, interviews are less problematic, or even preferable, since they inform about the researchers’ views on their activities. Furthermore, it is not evident that all scientific practices are equally suitable for observations. For example, the practical work on the shop floor in a traditional “wet lab”, or discussions about how to interpret a particular diagram, are likely to be more easily observed than computer programming, as I will return to below.

This discussion can also be related to different analytical views and the different types of questions that this thesis aims to answer by using the analytical tools presented in Chapter Two. At the most general level, I am interested in questions starting with both “what” and “how.” To some extent, questions concerning what and how demand different types of analytical viewpoints. In *The New Language of Qualitative Analysis*, Jaber Gubrium and James Holstein (1997) discuss some idioms of qualitative inquiry, of which I will start by discussing two: naturalism and ethnomethodology, including constructivism more generally. These have some important differences but much in common as well, and can therefore be used in combination where this is done with care. 

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71 In part, this can be related to the generalized symmetry principle since interviews can only inform about the human perception of things.

72 “Social” is removed from the title in the second edition. This is more in line with the explicit disavowal of “social factors”, as defining a realm that excluded consideration of “scientific” content or as an attempt to define the technical content of science in opposition to *something else* (Latour & Woolgar 1979/1986: 281). To avoid connections to this understanding, “social” is deleted from the title. This also signifies the view that if “social” comes to refer to all interactions, also including things, it becomes devoid of meaning. For a development of constructivism and a critique of misleading connotations, see Latour (1999a), see also Latour (2003b).

73 These differences are related to their different views of actors, as discussed in the previous chapter.

74 See Wieder (1974/1988) in which the naturalistic and ethnomethodological approaches are intentionally contrasted.
In short, naturalism seeks rich descriptions of people and interaction.\(^{75}\) Data should emanate from real life and the researcher’s close experience of it. “It is the settings’ sheer naturalness that makes them authentic.” (Gubrium & Holstein 1997: 7) Whereas naturalism is primarily oriented towards “what” questions, ethnomethodology turns attention to “how”.\(^{76}\) Ethnomethodology is a somewhat narrower term than constructivism,\(^{77}\) but Gubrium and Holstein’s (1997) discussion still serves to illuminate some questions of importance in this context. Ethnomethodology focuses upon the construction of social reality and makes the “taken for granted” its research problem.\(^{78}\) As Gubrium and Holstein (1997: 39) put it, ethnomethodological inquiry shifts the focus “from the substance of reality to reality construction practices” and to how its members accomplish society.

From these different perspectives, different approaches to data collection and different types of material are not of equal value, yet they are not mutually exclusive. The naturalistic way of performing research opens up possibilities also from the point of view of constructivist analyses in that it enables a broader base. In any case, without knowledge based on firsthand experience, we do not know where to look for what is interesting or for that which requires extensive investigation (Becker 1998: 16; cf. Blumer 1969). Thus, asking “what” questions is to a certain extent required if one is to be able to understand (important) “how”. Constructivist analysis can therefore be seen as a logical continuation and development of a naturalist inquiry and also related to the discussion above in terms of how observation and interviews may complement each other as data collection methods.

However, the postmodern idiom should also be mentioned, which takes qualitative inquiry away from and beyond empirically grounded “what” and “how” questions in its displacement of reality with representation (Gubrium & Holstein 1997: 76). This idiom views reality as a consequence, not as the object or the cause of scientific description, and it also involves a growing recognition of the role of rhetoric in representation (Gubrium & Holstein 1997: 87, 89). At the same time, the meaning of rhetoric changes when it absorbs truth and proof, rather than conviction and seduction (Latour 1987) – when all description is rhetorical (Atkinson 1992; cf. Gubrium & Holstein 1997).

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\(^{75}\) Some famous contributions in this vein include Whyte (1943) and Glaser & Strauss (1965).

\(^{76}\) Garfinkel (1967) is the pioneer of ethnomethodology. In social studies of science, Lynch (1985; 1993) has made important contributions from an ethnomethodological standpoint. The work of Knorr Cetina (e.g. 1981; 1990) can also be characterized as ethnomethodological.

\(^{77}\) Within (social) studies of science, several approaches to constructivism exist. For discussions about different notions of social construction, see Sismondo (1993).

\(^{78}\) While construction rather than meaning is the focus, ethnomethodology maintains a naturalistic orientation in striving to describe everyday knowledge and interpretative procedures in detail (Heritage 1984).
A postmodern idiom is especially suited for the analysis of texts and will therefore be used in document analysis, further described below.\textsuperscript{79}

**Empirical Delimitations**

I delimited my study of MISU in several respects. I excluded the oceanographers at MISU for the reason that meteorology, dealing as it does with atmospheric processes, was a focus from the start. However, I did not restrict my study to “classic” meteorology (cf. Rossby 1956/1959) since many experimental researchers at the department collaborate with atmospheric scientists in a wider meaning and these collaborations became an interest as such. Collaboration between modelers appeared as less evident and would have been more difficult to analyze due to the way I chose to study modeling. Since the aim is not to make a comparative study of experimentalists and modelers, the different foci do not constitute major problems.

I abstained from studying the research being conducted on the upper atmosphere at MISU for two reasons. First, studies of the upper atmosphere (the stratosphere) have been (or are) connected to the problem of ozone depletion rather than climate change.\textsuperscript{80} Second, the interest in the relationship between modeling and experimental practices requires points of connection (intersections) and I have focused on experimentalists and modelers concerned with the lower atmosphere (the troposphere). Although some modelers collaborate with theoreticians, these are almost absent from the department and they were therefore excluded from the study at an early stage.

I have also visited two field measurement expeditions as well as visited the Swedish weather service. It is important not to neglect how the organizational setting affects research, but since I have come to concentrate on the work in social worlds, geography or formal membership (cf. Shibutani 1955) is of less importance. Although my data collection had its point of departure at MISU, and while the participation of affiliated scientists has been a precondition for studying particular projects, the data itself refer to a much wider context. Research activities are often local, but science worlds generally have an international scope (cf. Strauss 1993). Thus, the analysis is not limited to MISU.

\textsuperscript{79} However, in Gubrium and Holstein’s (1997) version of postmodernism, everything is defined as text.

\textsuperscript{80} For a study of this connection, see Nolin (1995). See also Martinsson (2001) and Liftin (1994) for the politics of ozone depletion.
Preparations

All sciences are highly specialized fields and how one prepares to study them depends on the approach to the study of scientific practice. For example, preparations such as trying to learn the language of the researchers are at odds with one possible “anthropological” approach to science as the study object, in which aspects of scientific activity readily taken for granted should be apprehended as strange (Latour & Woolgar 1979/1986: 29). This is the approach advocated by actor-network scholars (see e.g. Latour 1987), but Collins (1988) questions if this really results in an adequate description of scientific activities. Furthermore, learning about what scientists talk about does not mean that one accepts how they do it at face value, and in order to avoid nontrivial results, I consider it important to learn how to distinguish what from how. In some ways, this is more in line with the type of anthropological work discussed by Sharon Traweek (1992: 438), according to whom the anthropologist is supposed to learn about the values etc. of the "culture". It is therefore not an end in itself to remain alien.

Because of its character as such a specialized field, would not scientists themselves be better suited for analyzing science? Indeed, many researchers involved in science studies have a background as practicing biologists, physicists, engineers etc. Yet it is questionable if this high level of expertise in the scientific field is contributing to a better sociology of the subject (cf. Collins 2004: 104). What is required to do sociology of the scientific subject however, is some degree of what Collins and Evans (2002) have referred to as an interactional expertise: the expertise one must have in order to have an interesting argument or lively conversation with a scientist about his or her work (cf. Collins 2004: 104). Although a meteorological researcher obviously knows a lot more about doing science than I do, the question is not only about how much is “known”. The question is also about what you want to know about and the tools you have at your disposal. Sociological questions, perspectives, and methods may mean that I am able to recover features that are not “known” in the same way from the practitioners’ perspectives (cf. Lynch 1985: 19) and therefore to a more fruitful sociological analysis. The study of working practices and the perspectives and commitments arising from them are topics of sociological inquiry for which the sociological “toolbox” is suited (cf. Becker 1986: 7). Why would the tools not be suited in terms of scientific practice?

81 See also Collins & Yearley (1992a: 316ff.) and Callon & Latour (1992: 357). For a recent debate on this issue see e.g. Stolzenberg (2004a;b) and Collins (2004). See also Mulkay (1974), who suggests that the analyst should bring a participant or ex-participant to the interview with a scientist. However, there are not only practical difficulties with this method but it is also an attempt to solve a different set of problems.
82 Collins and Evans (2002) distinguish interactional expertise from contributional expertise, where the latter is what you need in order to make a contribution to the scientific field or be offered a job in it.
In order to learn the language of meteorology and be able to communicate with the researchers, I acquired some background knowledge (cf. Star 1989a: 2). I read meteorological literature, including course literature such as textbooks, journal articles, and reports, which I ordered primarily from MISU and SMHI. I also attended open lectures and seminars about climate change that were delivered to various audiences. To find out more about meteorology and the department, I interviewed a doctoral student who was a member of an environmental research association that I participated in at the time. I also made a small study of the use of images in meteorological research work. This served as a preparatory study, which was based on three interviews with researchers at the Meteorological Section of the Department of Geosciences at Uppsala University (MIUU), and took place a few months before the main field work at MISU started. However, most of my knowledge about meteorology has been acquired during fieldwork, principally in conversations with research staff and by reading their work (cf. Traweek 1992: 439).

The Field Study

In order to access a formal organization, contacts should be made with people in higher positions who function as gatekeepers (Franssén 1997; Hammersley & Atkinson 1995). I sent a letter to the prefect at MISU where I presented my study. We also had an informal meeting where I presented more details about the plan of my project but also had the opportunity to ask questions about the department. We agreed that a presentation of my project at an informal staff meeting at the department would be a good way to introduce my study to the rest of the staff there.

My main fieldwork started with a presentation of my project at an informal staff meeting held in the lunchroom at the department.\(^{83}\) In order to ensure that people who were absent from the meeting would be informed about my project, everyone at the department received a one-page-letter in English and Swedish in their mailbox the same day as I held my presentation. Sixty-one people, including scientific, technical and administrative staff, worked at the department at the time of my field study. However, those who were actually present at the department varied since some people were on extended visits elsewhere and guest researchers worked at the department for limited periods of time.

\(^{83}\) At my presentation, the prefect introduced me and told the audience that I was going to conduct interviews with the researchers. He sounded positive and said it would be interesting to hear about the study of their work. Afterwards, I gave a brief, informal presentation of my project. Although my focus was on presenting rather than observing, I noted that some of the people held my letter in their hands during my presentation. While some people seemed to look interested and curious, others looked quite neutral or even negative.
At the end of the meeting, a researcher invited me to attend a so-called field campaign the following month. I accepted gladly. Another researcher invited me to attend a day-long discussion at one of the department’s sections about projects and future plans. Both of these invitations seemed like a good way to start the field work. I felt welcomed and had a concrete way to start. The last part of my first visit consisted of a guided tour around the department with the prefect. Following my presentation, he showed me around the building and introduced me to some of the staff, including the laboratory staff. This introduction was the starting point for my visits at the department.

To summarize, my fieldwork started in February 2003 and lasted for about a year. I visited the department regularly over a period of three months (March–May), during which time I also made eight formal interviews. I also wrote a form of summary report of the preliminary results and suggestions for further research. During the summer of 2003 I carried out two intensive periods of fieldwork (lasting for a week each), but spent the rest of the time reading through my material and trying to write about my findings (cf. Becker 1986b). A second phase of fieldwork, which included several visits to the department and eight interviews, took place between September–November. At the end of this period, I started to code my material and write drafts for chapters according to a structure that has only changed slightly since. I made a brief visit at SMHI and a few visits to the department during this period, which was otherwise free from fieldwork. During this time, I also analyzed grant applications with the purpose of investigating how researchers coupled their research problems to climate. During the coding phase, I conducted three final interviews in January and February. My fieldwork was supposed to end completely after my own seminar was held at the department at the end of March, but I also visited the department for an additional seminar after my own.

Fieldwork at the Department

In order to gain a general idea of the department and to get to know the people at the department and their work, I started by “hanging out”, which primarily involved attending coffee breaks and talking to people about their work. The technical staff demonstrated the facilities in the laboratories and on one occasion I observed the work being carried out by a member of the technical staff. It was more difficult to follow the work of the research staff.

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84 The principal difference between what I refer to as formal and informal interviews relates to whether I overtly and more or less explicitly guided the interviews or conversations by asking certain questions and showing my interview guide. In most cases, this corresponded with interviews where I used a tape recorder. From the point of view of validity, I considered it to be important to have some documentation that could retain verbal accounts in a more detailed form.
Researchers and professors have their own offices but most doctoral students share with another student. Because of the physical setting and the type of work that is done at the department it was difficult to observe work without being a disturbance. To observe work being carried out with a computer, one has to ask about what the person working with the computer is doing, especially when the observer is not familiar with tasks and computer programs. During a few days I sat next to two doctoral students and a post doctoral researcher but, unfortunately, I experienced that my attendance was clearly disturbing their work. As a consequence, I decided to draw more upon interviews and on what people told me about what they were working with. Most of my visits to the department therefore resulted in chats and informal interviews with people about their work, rather than “pure” observations of work. This decision was grounded in my experience of practical problems, but also in methodological considerations, since my interest also included the perspective of the people whose working activities I studied. However, during both informal and formal interviews, my informants and respondents often demonstrated different things for me, for example by writing equations, drawing pictures, showing parts of how a computer program worked or the results of a model simulation. In this way, through the representation of the scientists, the things and tools they worked with “participated” in interviews and conversations and in a sense became present and open for (my) alternative interpretation.

During the first three months I visited the department regularly. These visits took place two to four times a week and each lasted between two and six hours. During the following two months, I visited the department more irregularly but at least once a week, in addition to visits primarily for making formal interviews. I have attended nine different meetings at the department. These have concerned various issues primarily related to the organization of work at the department, for instance about guest researchers, and regular meetings of the sections. I have attended twelve seminars and two classes of a climate course. I attended most seminars during the early phase of fieldwork in order to collect material and learn more about research and the discursive repertoire of the researchers. Later, this became a way of maintaining contact with the field site. Thus, I did not leave the field abruptly but my visits became fewer and fewer.

Following each of my visits to the department I returned to my office at the Department of Sociology and made notes. In this respect, the proximity to the setting was a great advantage. During my visits I also used any free time to make brief notes in a notebook. It has been suggested that it is beneficial for field workers to openly make notes from the start in order to establish a note-taking role, but that this also distracts one from paying close attention to talk and activities (Emerson et al. 1995: 22f.). I sometimes made notes during longer conversations, but since I had ample opportunity to make notes soon after conversations and incidents, I mostly avoided writing
in front of my informants. However, an exception was during meetings and seminars, where I often made more notes directly, as this is a less “deviant” activity at such occasions (cf. Dahlgren 1996: 93). Furthermore, it could also be argued that people being interviewed “expect” interviewers to take notes, and may judge a lack of this to mean that they are not saying anything “noteworthy”.

Field notes are by nature selective but I tried to write down as much of interest as possible about my visits, mostly about the people I met, what we were talking about and what other people around were talking about and what seemed to be “going on” in general. I wrote less about the physical environment but documented it by, for example, noting what offices and laboratory looked like. During the first weeks of fieldwork I wrote field notes in English but subsequently realized that it was better to write in Swedish. It was faster and the language in itself did not require any extra effort. This is important when as much as possible needs to be written down as soon as possible after visits to the field.

In addition to the observations of everyday life at the department, including the more formal occasions such as meetings and seminars, I also had the opportunity to make three visits to two field measurement expeditions, resulting in about ten complete days of observations. I also attended four days of a five-day intensive workshop on data interpretation. There were five main participants. Two worked at MISU and the three others came from departments in different countries. On a further occasion, I attended one day of a two-day workshop about a chapter in a climate assessment publication. Of about ten participants, two were scientists affiliated to MISU. At the end of my fieldwork, I visited SMHI and interviewed eight people from various categories of the staff who were affiliated to different sections of the research department. I describe these interviews in more detail in the section on formal interviews. The visit to SMHI also included a seminar on the development of climate modeling by incorporating models of vegetation.

Regarding my relations with the people at the department, I did not talk to everyone to the same extent. For example, I regard some of the people I had more contact with as key informants whom I turned to when I had special issues that I wished to discuss or questions to ask. These key informants were both experimentalists and modelers. At the same time as one cannot bias the fieldwork by talking only with the people one find congenial or politically sympathetic, not everyone is willing to talk (cf. Hammersley & Atkinson 1995: 79, 91). I tried to talk with all researchers and technical staff but I did not make great efforts to return to the (few) people who I felt were less willing or interested in talking to me.

When I started my fieldwork, I thought that if I was around, I would meet most of the people and I could talk to them when they had time. This did not work as I had expected for several reasons. A few researchers and professors
were absent from the department for longer periods of time and several traveled a great deal. Three of the modelers visited other universities abroad during the first months of my study and several other researchers often attended workshops, conferences, and field campaigns outside the department. Moreover, some researchers did not participate very much in the informal social interaction at the department. I hesitated to knock on their doors and ask them to talk to me without any previous contact because people seemed to be very busy. While I tried to broaden my perspective by especially establishing contacts with people in higher positions, these were some of the reasons why I came to spend more time with doctoral students, and to some extent post-doctoral researchers.

It is also important to discuss the personal characteristics of the researcher in order to understand fieldwork. As a young person, it was easier for me to establish working relationships with doctoral students and post-doctoral researchers (cf. Landström 1998). Gender is another important personal characteristic of the researcher. Common cultural stereotypes of females can be an advantage in some respects. In so far as women are seen as unthreatening, they may gain access to settings and information with relative ease (see e.g. Hammersley & Atkinson 1995: 94). This is also likely to be the case in terms of age (see e.g. Punch 1986: 24). If my attendance had been considered as threatening, I probably would not have had access to certain more sensitive or important meetings and I did not get the impression and find it unlikely that people were “on guard” when I was around or when they talked to me.

This does not mean that I assume that my presence did not affect the setting and by extension, the data, when I was attending seminars, meetings or coffee breaks. However, my impression is that other audiences were of more importance than I was on all occasions except one (cf. Hammersley & Atkinson 1995: 222). On that occasion, I had the impression that a discussion was stifled because of my presence. This happened during a modeling meeting and concerned the question of whether a simulation model could be true or simply better or worse. This question seemed to raise some disagreement but was only discussed for a very short time before the modeler leading the meeting glanced at me and said something like “perhaps we should continue with this discussion afterwards”. This probably indicates

85 It has been suggested that junior people find it easier to adopt the “incompetent” position of the “marginal” person suggested as a paradigmatic approach by Hammersley and Atkinson (1995: 97). Although I must have appeared as incompetent in the eyes of the meteorologists at some times, I found it difficult to ask all the “stupid” questions that would have made it easier for me to understand exactly because I was afraid to be considered “incompetent” (by their standards). In retrospect I realized that it would have been easier if I would have accepted that I sometimes gave this impression. Yet the good will of researchers in terms of explaining to me as a layperson was sometimes frustrating because I was offered very general descriptions of meteorological research. In these cases, I tried to drop “keywords” that made my informants aware that I was not completely ignorant of the(ir) field.
the sensitive nature of this question, and I discuss this further in Chapter Seven.

In addition, and as Becker and Geer (1960, see also Becker et al. 1961) have suggested, the difference between solicited and unsolicited accounts should be considered when assessing evidence. It cannot be assumed that unsolicited statements are also unaffected by the researcher’s presence. Informal questioning also forms part of participant observation, especially in settings with limited opportunities to gather most material on the basis of unsolicited accounts, which was the case in this study. Moreover, the naturalistic desire for unsolicited data (cf. Gubrium & Holstein 1997; Hammersley & Atkinson 1995) limits the possibilities to learn about matters that are taken for granted, and hence, direct questioning is necessary. Thus, many of the conversations I had when I visited the department are best characterized as informal interviews.86

Two particular features of fieldwork are described in more detail in the following discussion. First, it is necessary to discuss the relations I established with the experimentalists and modelers at the department because these influenced my data collection and the decision to focus on these two groups as subworlds. Second, my visits to field expeditions are a basis for my analysis of experimental collaboration and also exemplify different aspects of my fieldwork as compared to my visits to the department.

**Field relations with Experimentalists and Modelers**

Learning more about the field is a way to prepare for fieldwork, but it does not reveal how to do it. In spite of reading about how other researchers have handled situations in the field, many of the skills of fieldwork are difficult to transmit and much about the work is left unsaid (Punch 1986: 16ff). In my view, some difficult aspects concern the role as researcher and the relations in the field. I will discuss questions relating particularly to experimentalists and modelers because of the role these groups play in my analysis.

Following the social world approach, I did not choose to study, generate or in any way take my point of departure in these categories a priori. Clarke (1991: 136) suggests that a good fieldworker enters “into the situation of interest and tries to make collective sociological sense out of it, starting with the question ‘what are the patterns of collective commitment and salient social worlds operating here?’” Collective commitment is thus an empirical

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86 I often used the work of researchers or doctoral students as a point of departure in conversations. For instance, if I had attended a seminar, I often asked for the paper that had been presented, read it and came to the author to ask questions. At other times, I went to talk about the work of a researcher or doctoral student and asked for written reports or articles about the project we discussed. I often read or browsed through these and returned to ask more questions. These preparations were a basis for conversations and aimed to deepen the data collection.
question and social worlds are actor-defined in the sense that they permit identification and analysis of collectives construed as meaningful by the actors themselves (Clarke 1991: 135; cf. Hughes 1971a). In this case, field experimental practices and simulation modeling practices seemed to contain important collective commitments, which became salient during the early part of fieldwork and organized my data collection thereafter. People referred to themselves as experimentalists or modelers and experimental work and simulation modeling seem to be the most profound basis for group feeling among the researchers I have studied (cf. Galison 1997; Saari 2003; Traweek 1988). To raise one example of many, one researcher talked to me about experimentalists and modelers as two groups of researchers, working with different things and making stereotypes about each other. He concluded, “There is a tension between these camps that you will probably become aware of.” Moreover, very few researchers participate in both modeling and experimental activities and two who did referred to it as “rare” and “odd” respectively. Although several doctoral students, mainly from experimental groups, mentioned that they strive to include both modeling and measurements in their dissertations and that they would like to keep a foot in each camp, their ability to do so depend on several factors. First, having this goal and the possibilities to reach it are related to the advisor. The students who have succeeded in this have had advisors who emphasize the importance of both types of work and who also try to combine them in their own work. Second, doctoral students have time constraints that make it difficult to learn both how to use and interpret measurement technology and to set up and run a model, since both activities require a long time to master. In relation to this group division, it is important to discuss early experiences since a friendly and co-operative gatekeeper can shape the development of research (cf. Hammersley & Atkinson 1995: 75). The selection of people to whom I was introduced during my first visit exemplifies how I was channeled into existing networks. This did not influence the direction of my study for the reason that I did not specifically

87 Knorr Cetina (1982) has a similar standpoint when specifically discussing this question in relation to science studies. Discussing the difference between taxonomic collectives that exist in the mind of the sociological classifier and those locally significant groups that appear to be relevant to the participants themselves, Knorr Cetina (1982: 115) insists that the groupings proposed as relevant in regard to scientific work should be of an empirical nature. They should be meaningful in terms of the participants’ contextual involvements. Compare also the view of Schütz (1932/1976) regarding first and second order constructs.

88 I sometimes refer to these as modeling and experimentation but these are only abbreviations for simulation modeling and field experimentation, unless I clearly state otherwise.

89 For instance, one researcher who I spoke to a few months after he finished his PhD, said that it takes such long time to analyze measurements that it is not possible to start with modeling at the same time. Lack of experience in this also results in poor model attempts compared to the “pure modelers”, another reason to stick to field experiments.
turn to these people during my fieldwork. However, generalizing this problem to include any informant, it is not always easy to grasp immediately how co-operative informants can shape the conduct of research. If one is “sponsored,” it may be difficult to achieve independence later (cf. Hammersley & Atkinson 1995: 75), but the consequences and importance of being “sponsored” by a group also depends on the purpose and direction of fieldwork. When some (experimental) researchers were very welcoming at the beginning, this seemed like a good start but it also made me concentrate more on experimental practice. This almost became a “vicious circle” in the sense that my focus on experimental work created more contacts in the same group (and less contact with modelers). As I came to see the welcoming individual researchers as a group (of experimentalists), it raised my awareness of the possibility that they could become “sponsors”, especially because of my growing focus on this group division (segmentation) as such. However, I decided to concentrate on understanding the experimental field in the first phase and to focus on modelers in a later phase of the study. As I spent more time with experimentalists in the first few weeks it was possible that some modelers could have seen me as aligned with experimentalists. Alternatively, it was also possible that the experimentalists may have seen me as “one of them” or “on their side.” To establish a balance, I consciously compensated by subsequently making efforts to “get to know” the modelers better and find out more about their work. They were friendly and open when I took a clear initiative and often seemed happy, and sometimes surprised, about my interest in and knowledge of their work.

The idea behind “hanging out” at the department was to find out what was “commonly known”. This is in line with the emphasis attributed by the naturalistic approach to the importance of “being there” and being close to ”real life” (cf. Gubrium & Holstein 1997). This leads to the question about the meaning of “there” with regard to different practices. In terms of experimental practices, “being there” involved not only visiting field campaigns, the laboratories, and the workshops, but also the offices and the seminar rooms – spaces that they share with modelers. However, because modelers sat at their computers most of the time, this type of working practice was less suited to naturalist work, partly because of the difficulties in “being there” without disturbing what was the intended object of the observations – working practices. It is therefore possible to trace the reasons for differences in accessibility to the location as well as well as to the practices per se. Part of the reason that I experienced the modelers as less welcoming may have been that modeling practice is less open to observation. In line with this argument is the fact that experimentalists did not encourage or invite me to observe their computer work any more than

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90 As a matter of fact, I did not turn to several of them because their direction of research was not within my focus.
the modelers did. Consequently, it is likely that practical rather than strategic considerations affected how experimentalists and modelers invited me to participate.

**Visiting Field Campaigns**

Experimental researchers organize measurement expeditions that often involve more than one research group (sometimes including as many as twenty), at locations such as in forests, on ice, on ships or airplanes. These are chosen on the basis of the research question and these expeditions are referred to as field campaigns. My visits to two different field campaigns were very valuable not only because they offered me the possibility to observe more measurement-related work, but also because of the importance of field campaigns in the subworld of experimental research.

During the campaigns, I stayed with the researchers in the same houses, but in separate rooms. One of the campaigns was large, with over ten participating groups. I observed the work of different groups, talked to their participants, attended organizational meetings and had breakfast, lunch and dinner with the participants. I wrote regularly two or three times a day and again in the evenings to have a balance between participation and documentation. I also carried a notebook and continually jotted down keywords.

The other campaign was smaller and I stayed at the same place for most of the duration, watching the work of the researchers, talking to them about what they were doing and about other aspects related to their field of research and research work in general. Another difference compared to the first campaign was that I frequently used a digital camera to photograph the equipment and the researchers while they were working with it.\(^{91}\) This was not a deviant activity since the researchers also took photos and I was even asked to do so for them on some occasions. I wrote down keywords in a notebook during the day and wrote up everything by the end of the day. I also wrote in the evening in case of social events together with the participants.

In order to discuss questions, test hypotheses, and find out more about an area of research about which I would otherwise have only gained restricted information, I initiated e-mail correspondence with a participant from one of the field campaigns. I used this to ask questions similar to the ones I asked people at the department during formal and informal interviews.

In relation to his fieldwork concerning the detection of gravitational waves, Collins (1998: 298) writes that “perhaps most important, [I have]

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\(^{91}\) I have not analyzed these photos specifically but they have mostly served as a visual record of what was going on. The purpose was also to take photos for presentation purposes. I also took some photos to have as an addition to my fieldnotes in the first campaign, but the purpose was mostly to have a better visual memory.
joked, chatted, and argued with physicists about their work”. Visiting campaigns provided me with many opportunities to chat with participants from different backgrounds, departments, and countries. This also illustrates the collaborative organization of experimental work that is described later in the analysis. I consider the chats I have had, mainly with experimental, meteorological researchers, as invaluable resources for my study, although I draw most of my examples from interview material (see below).

To summarize, I had much better opportunities to observe experimentalists in action than was the case with the modelers and this also enabled a more relaxed form of contact. Working activities at the department differ from those during (field) campaigns, and simulation modeling only takes place inside departments. Due to the above mentioned difficulties in observing work at the department, my analysis of modeling relies more on formal and informal interviews (at the department and SMHI), seminars and documents, compared to experimental work.

Formal Interviews

Spending time at the department was a very good way of preparing before conducting more structured, formal interviews. It gave me insights into the everyday of research working activities as I also came to know my informants better. As I could draw upon my experiences from visits, I could get more information and a deeper understanding during the interviews. In total, I conducted 19 tape-recorded interviews (fifteen in Swedish and four in English). The interviews ranged in length from 45 minutes to two hours, but generally lasted around an hour and a half. They mostly took place at the respondents’ offices, except in three cases where the interviews were carried out either in a small meeting room at the department (two interviews) or in a separate part of the library at the department (one interview). With one exception I transcribed the interviews as soon as possible afterwards. Transcribing interviews gave rise to many reflections and ideas that I captured by constantly having a text document open in which I noted ideas for possible further development. These reflections were important when I started my more structured analysis. To give the reader an idea of how I selected interviewees, in what order the people were interviewed, and the questions I asked and what guided these choices, I present more details about the interviews below.

Selection and Order

The organization was too large for interviews to be conducted with all members that could have been of interest in relation to my purpose. For this reason I selected a sample. I made a preliminary list after a couple of months, which I revised as I narrowed my focus. In selecting people whom to ask for a formal interview, I generally aimed for a broad range of
researchers in terms of position, approach and specialization. A general feature is also that many informants related their research to climate. I also took other aspects into consideration, especially when choosing between people who were similar in the respects mentioned above. Hammersley and Atkinson (1995: 137; cf. Dean et al. 1967: 285) mention that a valuable type of informant includes those people who are especially sensitive to the area of concern, and I kept this in mind when selecting informants for formal interviews. Since previous contact did not direct how I selected people to ask for formal interviews, I was not equally familiar with the respondents. I had talked to most of them on previous occasions, but in a few cases the actual interview was my only lengthy form of contact. However, my previous contact did affect the order in which interviews were carried out in the sense that I tried to achieve a balance between interviewing key informants and people I had had very limited contact with before the interview.

As a means of respecting the academic hierarchy, the first interviews were conducted with the professors at the department. These took place during the first three months of fieldwork and included interviews with a few more experimentalists than modelers. Interviewing professors was also a good way of finding out more about what was going on in the organization, for example in terms of important meetings and larger projects. I also interviewed three other researchers during this time for other reasons.

As my early fieldwork was more oriented towards experimental work, I aimed to make it more balanced by interviewing several modelers in what I refer to as the second phase of my fieldwork. This began about half a year after I started my data collection. During a period of about two months I interviewed six modelers and two employees who belong to the technical personnel. After this, I started to systematize my field notes and interview transcripts. I also visited SMHI to get a broader perspective on modeling and to find out more about climate modeling in practice.

Some of my interviews at SMHI had a formal character but were not tape-recorded. I had prepared specific questions for three of the respondents and more general questions for the others. The questions concerned the work at the different sections, the major questions they worked with and how they approached them. I also asked the respondents to describe their work in relation to the forecast model, how they worked to improve it, and how this was done in practice. The conversations with the other co-workers resembled the informal interviews with the staff members of the department. I noted many keywords during all these conversations but wrote extensive field notes afterwards.

92 One was a guest researcher with a specialty I was interested in finding out more about, one was about to leave the department and the third was affiliated to the department but was working elsewhere. The last person participated in one of the campaigns that I visited and I found it useful to conduct the interview soon after the campaign since I wanted to relate the interview to this particular project.
In order to narrow the problem formulation and thereby also the themes and focus of the interviews, I found it useful to have time between my periods of interviewing. As a means of balancing the data concerning experimental work collected during the early part of the field study and the data concerning modeling work collected during the later part, and to fill what I considered to be the gaps in the material, I conducted the last formal interviews with both experimentalists and a modeler. I conducted these interviews with familiar informants whose work I knew well and who I therefore could ask more detailed and direct questions.

Questions and Interview Guides
As the last paragraph implies, I did not ask each informant the same questions. My questions were related to what each informant was doing, which was the primary reason why each informant had been selected. I wanted to interview researchers involved in different types of research as a way to find out as much as possible about different meteorological research activities (while remaining within a certain range). Consequently I prepared a new interview guide for every interview. Some interview guides where more similar than others and several themes re-appeared, but most of the guides were very different.

Generally, I have been interested in research areas, what people work with, and their use of simulation models and measurement instruments. For example, what role did this play in their research work and how did they work with this, as well as more detailed questions about what steps this work involved and how models or instruments are developed. These themes follow from the focus on work and activities in symbolic interactionism and the special attention on the things involved in this work.

I have mostly asked concretely about the work or experiences of each respective respondent. This was especially important regarding modeling, since interviews (informal and formal) were my major source of data. I often asked for concrete examples as a way to find out about what people were doing, rather than how they presented what they were doing. Of course it is practically impossible to draw a sharp line between these types of accounts and, as I will discuss in the section on analytical work, this requires a flexible approach in order to make fruitful use of the different types of accounts.

In addition to questions about the production work, I have asked about the organization of research, how resources are collected and about collaboration with other researchers and groups. Furthermore, I have sometimes asked about the respondents’ attitudes concerning related issues, for instance what they see as problematic. We also discussed what the respondent regarded to be climate research, if and how s/he related her/his research to climate, and how s/he perceived the attention that climate issues have received in relation to meteorological research.
The interviews were held in a conversational style in order to facilitate an open expression of the respective respondent’s perspective. Due to the non-directive questions, it was of crucial importance to be an active listener and to assess how answers related to my research focus. The latter naturally improved with time. An important question to consider is how the respondent views the interviewer, for example as a journalist, colleague or researcher, and whether this is done with trust or suspicion (Sundqvist 1991: 159f.). My overall impression is that the respondents were interested, informative, and trusted me. In general, the scientific staff seemed to be happy and interested in talking about their work and it was easy to encourage them to do so.

There are some notable patterns regarding the answers from my informants related to their positions. Professors and senior researchers gave long answers that sometimes seemed to be prepared or repeated. This was mostly evident during my first interviews when I opened with broad and general questions. I thought this was a good way to start but it led to long descriptions of research that were often interesting but this made it difficult for me to both direct the interviews and ask all of my questions. This was an important lesson. It is likely that many saw me as a student rather than as a researcher and accordingly approached me as if I was to be taught rather than conversed with on equal terms. Maintaining a balance between very general and very technical descriptions was something I learnt along the way and interviewing became easier, and the interviews better, over time. I also became more familiar with the field in the sense that I became better at relating what was said to my other questions and to ask follow up questions. In other words, I had passed the phase of learning about “whats” in order to distinguish relevant “hows” and during the course of the study I believe that I acquired more interactional expertise (Collins & Evans 2002). Doctoral students were also talkative and gave long answers to my questions and these seemed more spontaneous than was the case with the professors.

Documents

Documents are common sources of material in studies of science and are sometimes used as additional sources in ethnographic studies such as this one. I collected different types of documentary material in order to prepare

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93 Nigel Gilbert (1980) has also discussed this problem when interviewing scientists. This is probably a common problem for (young) sociologists of science (Nolin 1990: 32).

94 The technical staffs were briefer when they told me about their work and I had to be more active. There are several likely reasons for this, among others their subordinate position in relation to the scientific staff and normally less used to talking about their work. It is also likely that after interviewing many talkative respondents, I was less prepared for this different “type” of respondent.

95 Many studies are based on the analysis of reports, protocols, articles etc. The important point here is that the informal parts of scientific work are not “available” in these types of
myself, both before and during fieldwork. Most of these sources are not salient in the analysis but are briefly mentioned in relation to the section about preparations. I have also used documents as supplementary sources to illuminate themes developed from the other forms of data. Furthermore, I have used (meteorological) journal articles to compare my conclusions with. This is most evident in terms of the question of validating models. Discussions of model evaluation in articles and in interview accounts both exemplify the rhetoric of science but in different ways. It is also likely that articles are valuable as support for the analyst in understanding what is “common knowledge” or sufficiently acceptable, or at least not too controversial, to be published in a scientific journal of the field. 96

A small part of the study is based more explicitly on document analysis. I have analyzed grant proposals in order to study how researchers present their projects in order to relate them to climate change issues. Initially, the idea of analyzing grant proposals arose from the empirical study. Many researchers talked about how everyone was trying to get funding by relating research to climate, and this made me interested in studying how this was done. However, the primary reason for analyzing grant proposals is that they represent literary inscriptions that pass “outside” the local place of knowledge production (cf. Latour 1987; Landström 1998). Funding applications connect the activities of the social world to the wider society. Reading them is a way of recognizing the range of actors involved in scientific production (cf. Latour 1987) and the mutual elaboration about what research problems consist of (cf. Knorr Cetina 1982).

Proposals to the National Research council are likely to be the most “neutral” among the different funding alternatives, one reason being that the National Research council does not propose certain lines or directions of research. 97 However, proposals do have to include motivations both regarding important scientific reasons and relevance for society in general. Consequently, potential connections to a well-known environmental problem are likely to be clearly stated.

I have analyzed 15 grant proposals. Four proposals are from 2001, four from 2002, and seven from 2003. Five of these proposals are based on modeling work, six proposals are based on measurements, and four are based on a combination of experimental work and modeling. Of these four, measurements are emphasized in three of them. I have also analyzed two proposals from MIUU and two from the so-called air-section at the Institute of Applied Environmental Research (ITM), which is closely related to MISU accounts. Thus field experiences give access to features of scientific practice that remain hidden in scholarly presentations of the same (cf. Lynch 1985: 3).

96 This is obviously also dependent on which journal it is. Since they have different profiles, they are likely to view slightly different things as “controversial”.

97 Examples of other funding agencies in Sweden that are likely to be more oriented to certain types of research are FORMAS and Vinnova.
through education and research. In the presentation, I will refer to applications with their registration number.

Presenting Preliminary Findings in the Field

As a means of presenting my initial findings, I held a seminar at MISU a couple of months after I made my last interview. About 25 members of staff from various sections, including administrative, technical, doctoral students, professors and researchers, attended the seminar. My analysis was in its initial phase and I only presented a summary of some preliminary results. After my presentation, I discussed my findings with the audience.

The seminar had two purposes. First, I wanted to share my findings and analysis with the people that had made the study possible. Second, the seminar was like a group interview where standpoints were discussed in public that I had previously only heard from one person at a time. This was interesting because it gave me the opportunity to listen to the reactions from others. However, it was mostly professors who made comments and asked questions. The questions and reactions were not surprising. The reactions were both “positive” and “negative” at face value, which was to be expected in the context of my analysis.

How should their agreement or disagreement be considered from the point of view of validity? While people are well-placed informants about their own actions, their comments concerning the result of a study must be analyzed and treated as another source of data. It may also be in a person’s interests to counter the interpretations of the ethnographer (Hammersley & Atkinson 1995: 229). In my early presentations of the project (by letter and in a verbal presentation) as well as during my seminar, I emphasized that I did not intend to evaluate or criticize research. However, initially I did not take into account the possibility that people would expect that my research should contribute something for them, for example regarding help to deal with problems. At the seminar, I was asked how experimentalists and modelers could work better together, a question to which I had no good answer. An additional example of how my research was considered to apply to the field was when one doctoral student made use of one of my figures at a licentiate seminar that took place about a month afterwards. During this seminar, he showed one slide with a figure that was inspired by one of the figures I had presented, which he explicitly stated.

98 See also Gubrium & Holstein (1997: 44).
The Analytical Process

This section contains a review of the practical analysis, work including coding the material, and choice of quotations. At the most general level, modelers and experimentalists at MISU represent two different groups, which I have analyzed both separately and in relation to each other. Although I have to some extent compared the two kinds of practices, the analysis of simulation modeling is not structured in the same way as is the presentation of experimental field research, or vice versa. The aim is to highlight central features of the two worlds that have been studied and since they are different, the analysis of them will focus on different characteristics.

Concrete Analytical Work – A Hybrid Approach

During the first phase of my fieldwork, I wrote theoretical and methodological reflections that related to my work, but I did not explicitly try to connect the theoretical reflections to existing theories. Instead, I endeavored to take my point of departure in the impressions and experiences during fieldwork in relation to working practice as the general (sensitizing) concept.

The first step in the process of analysis is a careful reading of the data in order to become familiar with it (Hammersley & Atkinson 1995: 210). I read and coded openly (line-by-line) all my field notes and interview transcriptions and started to identify general themes and patterns. I also wrote an overview report with the intention of more clearly specifying my questions. Nonetheless, at this stage, the aim was to use the data to think with (Hammersley & Atkinson 1995: 210) and I did not carry out much formal data analysis, such as coding, before the main fieldwork was completed.

At some point in time, fieldwork ends and the analytical work has to proceed only with the existing pile of data (Becker 1998). After completing the second phase of fieldwork, systematic work with my material by coding field notes and interview transcripts began. I used the program NUD*IST to do this, and began by importing the Word-documents with field notes and interview transcripts into the program. Inspired by grounded theory (at this particular stage), I started to develop codes – or nodes as they are referred to in NUD*IST – as I read through the documents and attached these codes to the sections of the material where I found it appropriate.

First, I made free, unrelated codes in order to get to know my material better and to focus on what was important and interesting in the material (cf. Seale 1999: 154). This stage resembled open coding in grounded theory. Second, I systematized my codes into themes, categories and sub-categories. The nodes where also related to each other in a root system, including a “main” node (e.g. articulation), a sub-group (e.g. funding and motivations,
organizing projects), nodes (e.g. purchase instrument) and sometimes sub-nodes. This was done both on the basis of my first codes and on the basis of how my material could be related to my theoretical tools. This categorization was constantly developed in the early part of the coding. By adding new categories and deleting others, which I found to be unimportant or insignificant in relation to my material, I recoded some of the material (cf. Hammersley & Atkinson 1995: 208f.; Blumer 1969; Becker 1998). I also wrote reflections about the codes and coding in separate files (so-called memos).

Concerning the construction of codes more specifically, I started by trying to connect the material to broader analytical concepts such as working activities involved in production and organization of work. Thus, I followed the symbolic interactionist focus on work (Becker 1982; 1998; see also Star 1983). Second, since I tried to code all my material and thereby not lose any opportunities to develop new themes, I also constructed codes that were not directly motivated by my research questions but connected to theory and visible in the material which I thought could be of relevance at a later stage. This was for example the case with the experience of work. The purpose was to code all my material and not lose anything (cf. Becker 1968: 245). This was of great importance since the aim was to henceforth work only with the coded material. After coding all interviews and field notes, I had different “files” consisting of all the sections of text that I had given a particular code, in addition to the original “uncut” material, and I could start to study each category separately and relate them to a more theoretical frame of reasoning. I also chose quotes that I wanted to use for deeper and more developed analysis. This means that the activities involved in sorting and systematizing material was but a part of the analytical process and that analytical work proceeded as I focused more on the activity of writing.

In the analysis presented in this thesis, I have used the theoretical concepts to develop the themes that I identified in the data. While my coding was inspired by grounded theory, it was not purely inductive. According to Blumer (1969: 24), who asserts that one can see the empirical world only through some scheme or frame, this is unavoidable in any study of the empirical world. In this case, this is the focus on practice that I outlined in the previous chapters. Nevertheless, Blumer (1969: 33) also points out that the analyst should learn from the empirical world, not resort to a priori theoretical schemes and bend the empirical world to their premises. This is how I have strived to collect and analyze data. Thus, the analysis has grown

99 All of the material has not been coded in NUD*IST. During one of the field campaigns I did not have access to a computer and wrote by hand. I decided to code it by hand in a more general fashion (“coding the record”) due to the time it would have taken to re-write everything into a Word-program. I coded the email correspondence with the informant mentioned above in a similar way.
from constant dialogue between my (coded) data, the codes, and the theoretical concepts presented in Chapter Two. My development of themes and codes therefore represents a hybrid approach that combines a data-driven approach with a theory-driven approach (Boyatzis 1998: 51ff.). This involves following an inductive approach in identifying themes, but also the use of theories to guide the articulation of meaningful themes.

In terms of documentary material, I analyzed grant applications in a different way from the material I coded by using NUD*IST. With the purpose of seeing how researchers motivated their research problems in relation to climate change, and to identify the resulting relationship between modeling and experimental practices, I have deconstructed the chain of the argument to see how it is structured and have focused on how different problems and research areas are linked to each other (translated) (cf. Latour & Woolgar 1979/1986). Hence, this analysis was more directly theory-driven than the analysis of interviews and field notes.

I have not coded or deconstructed the arguments made in dissertations, reports, and journal articles in detail since this type of material have been of most importance during the field work preparation and during field work itself, to learn about research and to prepare for interviews with informants. In the present text, I use this material for information or in addition to findings in the data from observations and interviews. I only include the texts that I explicitly refer to in the list of references.

Concerning the individual chapters, Chapters Five and Six are based upon two ideal typologies of social practices in the subworlds of field experimentation and simulation modeling. Creating ideal typologies is a way to make sense of data in order to highlight typical features and also to reach a higher level of abstraction (cf. Widerberg 2002: 141f.), which may produce a systematic account that may also hold for application to data from other situations (Hammersley & Atkinson 1995: 215). In the following, I will refer to these ideal typologies simply as typologies.

The two typologies are results of this study, based on primarily a naturalistic analysis of my material, in which I have made use of the analytical tools presented in Chapter Two. The typologies – primarily the experimental typology – are inspired by Becker’s (1982) categorization of artists (or art work) in his book Art Worlds where typologies are essentially about activities, as opposed to people (cf. Becker 1998: 44). The primary unit of analysis is thus a (working) practice that constitutes a role, which is made up of a combination of activities and tasks performed by an ensemble

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100 Becker (1982) outlines a typology of artists where the types are “relational terms that do not describe people, but rather how people stand in relation to an organized art world” (Becker 1982: 228f.). This is important since it points to how typologies illuminate the organized world people live in.
of people. Accordingly, the actors behind these practices are not necessarily individuals.\textsuperscript{101}

The typologies originate from an initial differentiation of practices in an attempt to connect analytical tools and material in forming new categorizations based upon two dimensions. This results in systematic typologies, each consisting of four types, thus resulting in eight different types of practices. A general difference, as formulated in the terminology of social worlds, is that whereas the experimental typology is more about articulation work, the modeling typology is about production work, and this is reflected by the use of codes to construct the typologies.\textsuperscript{102}

However, the development of an effective typology is not purely a logical and conceptual exercise but must include constant recourse to the material one is analyzing. There is little point in developing highly systematized

\textsuperscript{101}See also Bourdieu (1990).

\textsuperscript{102}In experimental work, the typology consists of entrepreneurship, integrated experimentalism, instrument expertise, and technical entrepreneurship. I noted differences between experimental researchers already during my fieldwork, where I observed how certain researchers or groups organized campaigns with other groups, which differed in terms of how many people that participated in the campaign, how long time the instrument set-up took, how many campaigns they visited each year, what their research interests were etc. The typology developed as a way to gain a more systematic understanding of how these campaigns were organized (articulated) and the practices they included. The dimension of "organizational role" was therefore appropriate as a division between the organizing and the organized. The dimension of "commitment" was a consequence of my theoretical focus on instruments or inscription devices and based on an idea of how to distinguish the different practices performed by the groups involved in the field campaigns. The difference in the "organizational role"-dimension was then established by finding patterned differences regarding the main node of "articulation", the subgroups "funding and motivation", "ideas and design", "organizing projects", where the latter includes the nodes "organizing production tasks", "purchase instruments", "purchase data", and "purchase people". The difference in the relation to instrument dimension ("commitment") was established by patterns in the main node "production", the sub-group "experimental activities", the nodes "measurements", "interpretation", "developing instruments", and "process and assurance". In simulation work, the typology consists of exploratory use, theoretical construction, applied use, and pragmatic construction. The idea behind this typology also came during my field study, also as a consequence of the differences in how modelers worked with simulation models. I found two main differences between modeling practices. First, whether modelers constructed or developed simulation models or whether they used existing models, without major changes. This was the basis for the first dimension, "work with simulation model", which is based on differences in the main node "production", subgroup "modeling activities", where "use" refers to the node "simulations" and sub-nodes and the node "work with results" and "building" is based on the nodes "development" and "numerical activities" and their sub-nodes. Second, I found that modelers seemed to have different commitments regarding what was most important: model output data that corresponded well with measured data or the theoretical construction of the simulation model. This question seemed highly debated. The second dimension, which I refer to as "primary commitment", is therefore based on systematic differences in the main node "working tools", the subgroup "models", nodes "model capacity" and "type of model", the main node "production", subgroup "modeling activities", nodes "development", "work with results", and "numerical activities". Thus, both typologies are developed on the basis of additional interpretation of and work with the coded material, including comparison and the search for patterns, similarities, and differences.
typologies if they provide little purchase on the data at hand (Hammersley & Atkinson 1995: 218, cf. Becker 1998). In this work, the logical elaboration generates two categories that are less documented in the material compared to the others, due to where and how data has been collected. These types are technical entrepreneurship (experimental practice) and applied use (modeling practice). Although the practices are present in the experimental and modeling subworlds, they are of less importance due to the focus of this work on meteorological research.\textsuperscript{103}

The work that has resulted in Chapter Seven and Chapter Eight proceeded in a similar way through the extended comparison and search for patterns within other nodes. However, it was more focused on similarities within modeling and experimentation respectively, and the differences between modeling and experimentation. Chapters Seven and Eight also differ from the two preceding chapters in that they exemplify a more clearly constructivist analysis. Questions are asked about the purpose, function and role of different statements and “ideologies” in forming sub-worlds in relation to other subworlds. In these chapters, statements are not taken at face value to the same extent, as plain descriptions of practice, but as expressions of perspectives intrinsic to the (working) practices. The themes developed in Chapter Seven result from a data-driven analysis further developed in relation to theory. The analysis of problematizations (funding applications) in Chapter Eight is based on a more theory-driven approach from a postmodern analytical stance and consequently focuses to a greater extent on rhetoric. The remainder of Chapter Eight contains analysis that has been developed in a similar way as that presented in Chapter Seven and focuses more on perspectives.

As Becker (1986b: 90ff.) points out, writing is a lot about rewriting. During this process there is a struggle to express the argument as clearly as possible and perhaps to reorganize the text to do so, but it also includes revising and rethinking more generally. This does not mean that I have left the solid base of my coded material and the themes developed there behind, but only to say that the final shape of arguments and the chapters is the result of several rounds of rewriting.

Use of Citations and Descriptions from Field Notes

The quotes presented in the thesis are selected and included for several purposes. First, I use them to illustrate my points, thereby giving the reader an insight into my material and the opportunity to judge the validity of my interpretations presented in the discussions in connection to these quotes. A major purpose of these quotes is to support my reconstruction of the

\textsuperscript{103} See Hammersley & Atkinson (1995: 215f.) about the different ways of constructing typologies in ethnographic accounts with regard to their systematic development.
perspectives of field experimentation and simulation modeling, which is a presupposition for the structure of the thesis as a whole. Individual people constitute social worlds but they commonly act as part of or on behalf of their social worlds (cf. Bucher & Strauss 1961). This includes their verbal accounts, which can therefore be used to identify perspectives of the social world, or in this case, subworld.

Second, on some occasions I have included additional material in footnotes when it has concerned an interesting but not central issue, or when the main quotation concerns a complicated issue, for example regarding more “technical” aspects of meteorological research, or when the point I try to make through the primary quotation is of special importance. Third, I sometimes use quotes as straightforward descriptions, of, for example, a certain task. This is where I have considered it appropriate to use the voice of researchers rather than say the same thing in my own words.

In order to secure the validity of my analysis, I have read parts of the transcribed interviews from which the quotes originate so as not to misinterpret their meaning because of the lack of context that excerpts give rise to. This, for example, may regard the type of social interaction that produced the account (cf. Kvale 1997: 167). For the same reason I have retained the original language of the quotes and their vernacular character until the final version. I then translated to English and edited them slightly, but did not change their character as spoken language. In cases where contents of the quotations may have revealed the identities of the respondents, for example, reference to nationality, gender, locations of field campaigns, names of models, instruments or colleagues, I have changed these since they are generally of no importance for the analysis. In order to further protect my respondents, I do not inform the reader if a certain quotation has been translated or not. I have also endeavored to draw from different sources in order to avoid presenting only the view of a certain person and basing my own arguments on this.

I often state whether quotations derive from interviews with modelers or experimentalists. But how have I decided whether the person is speaking as an experimentalist or as a modeler? The social world approach suggests that people tend to act as representatives of their social world in relation to

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104 For example, I removed small words that do not appear to be of importance in the context (e.g. “hm”), reducing repetitions (e.g. ”I mean, I mean”) and corrected language mistakes (e.g. ”they is”) to facilitate for the reader. When I have excluded some part of the quote, I have marked it with (...). When I have included a word that the respondent referred to but did not use (e.g. when saying “it” and referring to a campaign), I have bracketed the word I have included [ ].

105 A list of all Swedish quotes and their English translations is available upon request.

106 An overview over the citations used in the different chapters (four to eight) is available. This includes assumed initials of the respondents and how many times they have been cited in each chapter. However, it is self-evident that only experimentalists are cited in Chapter Five and only modelers in Chapter Six.
outsiders (Clarke 1990). Although this does not mean it is possible to determine from which perspective a person is talking (e.g. as a mother, researcher, modeler), it is likely that the researchers I talked to who, for example, modeled, spoke to me as practicing modelers, since modeling was the topic of the conversations and interviews.

I have also presented excerpts from my field notes primarily to illustrate points. These include verbal accounts, which I have documented as soon as possible after the event. I indicate in the text when citations originate from researchers who I have formally interviewed without a tape recorder by using quotation marks. When they derive from conversations, I refer to what people have said more indirectly and make it clear that these are based on field notes rather than transcripts. While observation notes are not salient in the presentation, my experiences as a participant observer are likely to strengthen the validity of my interpretations since they have given me a better understanding of the situation and context of the accounts.

In addition to quotes from interview transcripts and field notes, I also quote from articles and reports (not applications). Since I have not analyzed these as thoroughly as the other types of material, these are supportive and illustrative rather than used as empirical evidence to uphold the analysis (compare the second and third way to quote).

Finally, throughout this chapter I have presented the study in a chronological order to provide a good understanding of how all my research work has proceeded. I have done so in line with the view that sees methodology as integral to a study and not as something separate (cf. Blumer 1969). This means that some of the methodological considerations also appear, and should be evaluated, in direct connection to the presentation of data, analysis, and results.
4. Overview of Meteorological Research – A Background to the Study

People in industrialized countries tend to be familiar with meteorology through the widely spread weather forecasts, presented in various media on a regular basis. These forecasts originate from complex computer models, developed on the basis of meteorological research. It is this “backstage” of meteorology – in which people devote their time to understand and study the weather and other atmospheric processes – that the chapter aims to introduce to the reader. It therefore serves as a background to the study.

Social worlds have many properties that may be of significance, such as history, origin, duration, amount of resources, and relationships to technology (Strauss 1993: 213, see also Strauss 1978b). Thus, a major part of this chapter is devoted to brief reviews of the history of meteorology, the history of the Department of Meteorology at Stockholm University, the current situation at the department, and the primary (material) technologies researchers use. I will also draw some tentative conclusions about the development of research, particularly at the department, which further motivates the direction of my research. In between these sections, I include summaries of relevant earlier research in terms of the climate theme of the study. This is an extension of the discussion in Chapter One and a further motivation for the focus of this study. Firstly, however, I provide a conventional description of what meteorological researchers study – processes in the atmosphere, which produce clouds, weather, and climate. This will facilitate the reading of the rest of the chapter and the thesis as a whole.

The Field of Meteorology – Weather and Climate

The atmosphere is the layer of gas surrounding the Earth and weather is the fluctuating state in the lowest 10–15 km of the atmosphere that results from

107 An historical overview is of special importance for study inspired by social world theory such as this since I am not using historical documentation as primary material (cf. Strauss 1978b: 242). See also Chapter Three.

108 For most of the general descriptions, this chapter draws upon secondary literature, but concerning the local development at MISU, primary material has also been used.
many processes working at different temporal and spatial scales, of which some partly mix with each other (see e.g. Lejenäs 2001). The engine of all atmospheric movements – the reason why the weather changes – is the solar radiation that unevenly heats different regions of the Earth, making it much warmer at the equator than at the poles (see e.g. Salby 1992). The part of the atmosphere where weather occurs is called the troposphere and the boundary layer is the part of the troposphere that is closest to the surface of Earth. The atmosphere extends over 100 km above this surface and is divided into different layers according to characteristic properties, of which the most common is temperature variation. These layers are referred to as the troposphere, stratosphere, mesosphere, and thermosphere.

Climate is more difficult to grasp than weather and while weather occurs in the troposphere, all processes held as relevant for climate occur below the thermosphere (see for example Salby 1992: 59). Whereas weather refers to particular events and conditions occurring over hours, days, or weeks, the Swedish weather service defines climate as the long-term properties of the weather in statistical measures. Climate, as opposed to weather, can therefore only be “observed” indirectly through the collection of observations over a longer period of time. Common measures used to describe climate are mean, variance, maximum and minimum values and frequencies of specified events. Climate varies over time and space and “classic climatology” classifies and describes different climate regimes on Earth (see e.g. Liljequist 1970). A typical duration of a climate regime in meteorology is 30 years and statistically significant variations in the mean state of the climate or its variability, which typically last for a decade or longer, are referred to as climate change.

The composition of the atmosphere is important for climate (and the climate system). The atmosphere consists of a mix of gases consisting of 78% nitrogen, 21% oxygen, less than one percent argon and also a number of trace gases including carbon dioxide, methane, and ozone (see for example www.ipcc.ch 12/5/2003). These gases are known as greenhouse gases and in spite of the small concentrations in which they occur in the atmosphere, they play a crucial role in the energy balance of the Earth. By absorbing infra-red radiation from the surface and then emitting the radiation in all directions, heat is retained within the atmosphere. This is what is referred to as the natural greenhouse effect.\footnote{However, the most important greenhouse gas is water vapor, which is responsible for 60% of the natural greenhouse effect. As opposed to the other greenhouse gases, humans cannot directly affect the amount of water vapor. However, according to climate model simulations, the enhanced greenhouse effect – that carbon dioxide among others contributes to – also increases the amounts of water vapor, which is another enhancement (www.smhi.se/sgn0106/rossby/fragorsvar/htm 1/7/2002).}

The atmosphere also contains aerosols, which have both natural and anthropogenic sources. Aerosols are particles suspended in a gas and thus consist both of particles and gas and strictly
speaking, the aerosol and aerosol particles are two different things. Because they are active in radiation processes, aerosols figure in the energy balance of the Earth but their relevance for climate is also an effect of the fact that they promote the formation of clouds when acting as cloud condensation nuclei (e.g. Salby 1992: 63). Clouds are salient features of what we know as weather and because of their radiation properties they are also key components of the energy balance and the formation of clouds.\textsuperscript{110} How they function in the current climate is a major issue in meteorological research.

History of Meteorology as Weather Forecasting

In his historical description of the development of meteorology, Nebeker (1995) focuses on the 20\textsuperscript{th} century but also gives a background of earlier centuries and describes three transformations. The first transformation occurred at the end of the 17\textsuperscript{th} century and resulted in meteorological observations changing from qualitative descriptions to become largely quantitative. This transformation was enabled by the development of instruments for measuring wind, precipitation, and temperature (Nebeker 1995: 4). Although the explanations remained qualitative, this caused descriptive meteorology to become less of a natural philosophy and more of an empirical, quantitative science. The second transformation occurred during the second half of the 19\textsuperscript{th} century and was related to the development of the weather map as a basic instrument for meteorological description, analysis and forecast.\textsuperscript{111} This change also involved the establishment of a weather service, networks of observers, and international cooperation between meteorologists. Governments contributed to the institutional basis mostly because of the meteorologists’ ability to predict weather, and while technological development was important, Nebeker (1995) considers the change during the 19\textsuperscript{th} century as mainly organizational.

By the end of the 19th century, meteorology was established as a discipline and its theoretical foundations began to take form (see for example Friedman 1989; Edwards 2001). Although different traditions – theoretical, empirical, and practical – remained relatively independent, according to Nebeker (1995) they all contributed vital elements to the new discipline. Within the theoretical tradition, the laws of physics constituted the point of departure and the status of dynamic meteorology as a theoretical science exclusively originated in researchers’ application of physics to atmospheric

\textsuperscript{110} Clouds are generally classified into the three major categories of stratus, cumulus, and cirrus (see e.g. Salby 1992: 94).
\textsuperscript{111} See Monmonier (1998) for the history of the weather map.
phenomena (Nebeker 1995: 44). Many of the people anchored in the empirical tradition concentrated on climatology based on weather statistics, and the status of meteorology as an empirical science mostly resulted from the work of climatologists (Nebeker 1995). At this point, and indeed during most of the 20th century, the development of climatology as a scientific field took place as a part of the broader field of meteorology, based on the interest in understanding long term weather patterns (Miller 2004: 51). Climate and weather were thus basically identical and “the distinction (...) more or less artificial” (Hambridge 1941: 4; cf. Miller 2004: 51). As will be discussed below, this conception has changed over time. Nebeker (1995) also mentions a practical tradition, involving weather prediction based on only few observations and almost no theoretical foundation.

Weather prediction has always been a question of fundamental interest in meteorology. The first person to develop a numerical weather prediction system was Lewis F. Richardson in 1922. Richardson elaborated a mathematical theory about how equations should be used to calculate changing weather patterns. He also constructed a concrete example by using observations from three places in Europe (see Dahl 1991: 138). Partly because of the complexity of the calculations, it took Richardson five years to produce a result. Not only did it take a long time to produce, but the result also failed to produce reliable weather forecasts (see e.g. Holton 1992: 434). In his book Weather Prediction by Numerical Process, Richardson wrote that part of his failure was related to limited data but also to the limited capacity of one man to produce a forecast on time (Richardson 1922; cf. Dahl 1991: 138). From his perspective, computers as well as observations were preconditions for the development of weather forecasting, something that meteorologists also seem to agree on today.

Meteorology developed considerably during and following the Second World War. One of the reasons was that air forces were dependent on weather forecasts at the same time as airplanes enabled better observations. Radio probes attached to weather balloons, developed during the 1930s, also added to the observational resources (see for example Friedman 1989). In 1951, The World Meteorological Organization succeeded the International Meteorological Organization as a specialized agency of the UN. The roles of the WMO are to facilitate international cooperation in the establishment of networks of stations for making meteorological, hydrological and other observations, and to promote the rapid exchange of meteorological information, the standardization of meteorological observations and the uniform publication of observations and statistics (www.wmo.ch 2/18/2003).

112 The Norwegian Vilhelm Bjerknes was important in this endeavor. See Friedman (1989) for a detailed account of Bjerknes’ work and how it contributed to the “appropriation of the weather”.

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However, according to Nebeker (1995), the most important change – the third transformation of meteorology – was the development of computers. Although meteorologists did not participate in the development of computer technology in the pre-World War II era, the history of computers on the one hand, and meteorology on the other, largely overlapped after the 1940s (Nebeker 1995: 123). Meteorologists were among the first users of the earliest generation of computers. Meteorology has also affected the further development of computers because of the need for powerful computer resources. From 1940s until today, meteorologists have been eager consumers of computer products, especially the most advanced machines. Meteorologists have also been active in the development of sophisticated software (Nebeker 1995: 165).

In 1946, John von Neumann and Carl-Gustav Rossby, who came to play an important role in the development of meteorology both internationally and in Sweden, arranged a meeting of key theoretical meteorologists in the USA at that time. This resulted in the establishment of a meteorology project at the Institute for Advanced Study at Princeton, USA (Bolin 1997: 3). Rossby was a professor in meteorology whereas von Neumann was a leading mathematician who also had an interest in the practical applications of future computing capabilities. At the end of the 1940s, von Neumann and his collaborators built the first computer machine, the ENIAC. Because it was both an unresolved scientific problem and a practical problem of public interests that would secure funding, von Neumann considered weather forecasting to be the ideal challenge for the development of computers (Nebeker 1995: 136ff.).

For some time, elements of the meteorological community were overly optimistic about the development of forecasting and believed that enough data and computer resources would enable forecasts for whole seasons or years ahead (e.g. Nebeker 1995). The development of chaos theory put an end to this optimism. Edward Lorenz coined the expression *butterfly effect*, technically defined as sensitivity dependent on initial conditions (e.g. Lorenz 1963, see also 1993). The mathematical descriptions of weather suggest how it is a dynamic system to which chaos theory and the butterfly effect applies. Because the calculations are so sensitive to initial values, forecasts for longer time-periods are impossible to produce. The limit is considered to be about two weeks (see e.g. Lejenäs 2001).

However, in order to produce numerical weather predictions, as many observations as possible are required. Communication improved because of

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113 The Swedish researcher Bert Bolin, who later became well known for his role in the IPCC during the 1990s, participated in the development of the models. In 1953 at the Royal Institute of Technology in Stockholm, Bolin and another researcher in collaboration with the Swedish Air force were the first to make and use forecasts with a computer as well as to begin with routine real time numerical weather forecasting (see Bolin 1997; Bergthorsson et al. 1955). See also *Svenska Dagbladet* 8/12/2004 for a popular article on this topic.
radio and radar and this led to an expansion in the use of weather balloons, marine buoys and satellites, which enable short-term weather forecasts. In 1968, the World Weather Watch (WWW) was instituted and “by means of the WWW, the atmosphere is now kept under constant surveillance” (Davies 1972: 329).  

To return to the development of the models, the weather models used by forecasters in the mid-1950s were regional or continental. However, for theoretical meteorologists who were unconcerned with real-time forecasting, general circulation modeling became an important goal. The intention of general circulation modeling is to simulate time-averaged properties of atmospheric motion and, accordingly, initial values do not pose the same problems as they do in the context of weather forecasting (see e.g. Holton 1992; Smagorinsky 1983). The first protracted effort to construct an atmospheric general circulation model originated at the general Circulation Research Section of the U.S. Weather Bureau in 1955. It is important to point out that general circulation modeling is a consequence of the conception of the atmosphere as a single entity, which is a crucial aspect in terms of how the focus has turned to the climate system from the climate as another way of describing the weather.

114 Every country receives, on a routine daily basis, meteorological data to meet national requirements. Each country also contributes to the system by establishing and operating specified facilities and services within their own territories. However, monitoring and services are expensive and developing countries have been assisted in this task. Thus, observation facilities are sparse in some parts of the world. From that point of view, satellites have improved meteorologists’ access to observations even more, and have been considered a “turning point” in meteorology. It was largely a result of this development that the WMO planned and introduced the worldwide observation network (Davies 1972: 330). The WWW is the backbone of WMO’s activities, which offers worldwide weather information through observation systems, operated by member states. Telecommunication links four polar-orbiting and five geostationary satellites, about 10 000 land stations, 7 000 ship stations, and 300 moored and drifting buoys carrying automatic weather stations (www.wmo.ch 2/18/2003).

115 Most of the proliferation of GCM modeling groups in the late 1960s and early 1970s occurred in the United States but new groups were also established in Europe. During the early spread of GCM’s, new groups initially borrowed existing models and subsequently made significant modifications. The European Center for Medium Range Weather Forecasts is notable in this respect, since this center built a new model from scratch. Today, it is generally considered to be the best weather forecast model worldwide. This appears to be “common knowledge” among meteorological researchers at MISU and SMHI. See also e.g. Dahl (1991) and www.aip.org/history/gcm/1975_85.html 31/8/2004. For a brief history of the development of atmospheric general circulation models from the 1940s to the early 1990s, see the web site Atmospheric General Circulation Modeling – A participant history (www.aip.org/history/sloan/gcm 12/27/2004). More details about modeling are also provided below.

116 Richardson’s calculating techniques – i.e. division of space into grid cells, finite difference solutions of differential equations – were also employed by the first builders of general circulation models.
Meteorology and Climate Change – Global and Local Perspectives

At the end of the 19th century, the Swedish scientist Svante Arrhenius (1896) was the first to recognize the important role of carbon dioxide, water vapor and other greenhouse gases in effecting the atmosphere’s heat retention capacity. He also speculated on the possibility of anthropogenic climate change from the combustion of fossil fuels. In the mid-1950s Rossby (1956/1959: 14) wrote:

> It has been pointed out frequently that mankind is now performing a unique experiment of impressive planetary dimensions by now consuming during a few hundred years all the fossil fuel deposited during millions of years. The meteorological consequences of this experiment are as yet by no means clarified, but there is no doubt that an increase of carbon-dioxide content in the atmosphere would lead to an increased absorption of the outgoing infrared radiation from the earth’s surface thus causing an increase of the mean temperature of the atmosphere.

In the early 1950s, a group of Scandinavian scientists started a network of 15 stations where regular air samples of carbon dioxide were taken (Rossby 1959/1956: 15). Furthermore, monitoring of the carbon dioxide content of the atmosphere established was a result of the International Geophysical Year (1957–58). Around the beginning of the 1970s, studies began to be carried out on the effects of changing carbon dioxide concentrations for the Earth’s radiation balance. The first studies used simpler models and responses to CO₂ doubling became the standard form of this experiment (Manabe & Wetherald 1967). The first use of a general circulation model to study the effects came in 1975 (Manabe & Wetherald 1975).

In terms of the current understanding of the climate system, it is of fundamental importance that the representation of the Earth’s climate changed between the mid-1960s and the late 1980s (Miller 2004: 53ff.). Based on computer models of the general circulation of the atmosphere, climate scientists increasingly represented the climate as an integrated, global system. Thus, today the climate system is regarded as an interactive system consisting of five main components: the atmosphere, the hydrosphere (the oceans, seas, lakes), the cryosphere (ice), the landmass and the biosphere (e.g. www.ipcc.ch 5/14/2003). External forces, of which the sun is

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117 See e.g. www.aip.org/history/gcm/1955_65.html 31/8/2004. David Keeling, who established the first station, was an early guest researcher at MISU and worked with Bolin on simulation modeling (Elzinga & Nolin 1998: 20; see also Bolin & Keeling 1963), who was also engaged in studying carbon dioxide. Examples of Bolin’s importance in the early phases of the climate change question are his early simulations of the role of increased amounts of carbon dioxide in the atmosphere in the 1950s and pioneering article on the lifecycle of carbon (see Bolin & Eriksson 1959).
the most important, affect these components. In spite of differences in terms of composition, physical and chemical features, and structure and behavior, these components are all linked through changes of heat, mass and momentum. Furthermore, there is an inexhaustible amount of complex interactions between them. The atmosphere is conceived of as a single, and the most volatile, entity of this system.

The Political Situation and the Establishment of the IPCC

With the rise of the environmental movement in the early 1970s, there was also increasing interest in global environmental problems. The rise of the environmental movement provided better opportunities to secure funding for carbon cycle and modeling research and to link the two into a common research program on the greenhouse effect (Hart & Victor 1993: 667). Two studies in the report series *Studies of Critical Environmental Problems*, which was prepared for the UN Conference on The Human Environment in 1972, noted the possibility of “inadvertent climate modification” and these reports are considered as the origin of public policy interest in anthropogenic climate change.118 In 1979, the US National Academy of Sciences’ Ad Hoc Study Group on carbon dioxide and climate, produced the first assessment of the CO$_2$ doubling temperature (see van der Sluijs et al. 1998: 298).119 Reports *summarizing* the state-of-the-art in terms of scientific results, such as assessments that represent presumed consensus, can also be more important political tools for scientists than publications used to *build* the state-of-the-art in scientific research (cf. Hart & Victor 1993: 668).

In the early 1980s, the possibility of global warming became a policy issue within scientific agencies and in 1985, an influential climate science conference in Austria recommended policy studies of climate change. However, the formation of the United Nations International Panel of Climate Change (IPCC) is perhaps the most important event in the establishment of climate change as a global problem.120 It is generally agreed that this has played a tremendously important role in climate politics and the climate change debate more generally (see e.g. Miller 2004; Miller & Edwards 2001; O’Riordan & Jäger 1996; Shackley & Wynne 1996; Elzinga & Nolin 1998; van der Sluijs et al. 1998).

In 1988, the United Nations Environmental Program (UNEP) and WMO established the IPCC. This was chaired by Bert Bolin and comprised three

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118 The reports are”Man’s Impact on the Climate Environment” (1970) and “Indadverted Climate Modification” (1971). See e.g. [www.aip.org/history/gcm/1965-75.html](http://www.aip.org/history/gcm/1965-75.html) 31/8/2004.
119 Assessment is the analysis and review of information derived from research in order to help someone in a position of responsibility to evaluate possible actions. It means assembling, summarizing, organizing, interpreting and possibly reconciling pieces of existing knowledge (van der Sluijs et al. 1998: 291).
120 On the early origins of the IPCC, see Agrawala (1998).
working groups. The IPCC provides an ideal site for boundary work (cf. Miller 2004: 60) since it was considered crucially important that a line be drawn between science and politics (e.g. Bolin 1994). This was operationalised by separating Working Group I (the science of climate change) from Working Groups II and III (the impacts of climate change and response strategies). Since 1988, the IPCC has produced three assessment reports, each of which comprises three volumes, which correspond to each of the three IPCC working groups. These reports are “The Scientific Basis”, “Impacts, Adaptation and Vulnerability” and “Mitigation”. In “The Scientific Basis”, researchers appointed by the IPCC (i.e. Working Group I) present the current state of knowledge as well as what they consider to be the most important areas of lacunae in our understanding of human induced climate change, based on a summary of research in the field.121 Many scientists are also involved in reviewing the reports in order to make them acceptable to the scientific community. According to one researcher, “The composition is the great strength of IPCC, (…) [the reports] should be written for the scientific community and in that sense achieve the acceptance of the scientific community and in that way give the IPCC a scientific integrity.” The scientists involved in these processes are primarily from the atmospheric sciences and the majority of them have backgrounds in meteorology. An organizational reason for this is that the IPCC was created under the auspices of the WMO and an epistemic reason for the dominance of atmospheric scientists is how the climate question has been defined as a research question (Elzinga & Nolin 1998: 48f.): When formed, IPCC derived its understanding of climate from climate models (Miller 2004: 54).

Because of their character as assessment reports, the IPCC reports do not include the latest results. For example, researchers began to acknowledge the influence of particles on climate during the mid-1980s, and the first estimation of the aerosol effect came at around the same time as the first IPCC report in 1990. However, more extensive discussions about aerosol effects were largely lacking until the third IPCC report was published in 2001 (interview with NN). Researchers have acknowledged that aerosols influence climate in two ways (see for example www.ipcc.ch 5/14/2003). By spreading and absorbing radiation, aerosols have a cooling effect that is called the primary (or direct) aerosol effect. By acting as cloud condensation nuclei or by modifying the optical properties and lifetime of clouds, aerosols contribute to cooling in another way, which is referred to as the secondary (or indirect) aerosol effect. The effects of larger amounts of CO₂ in the atmosphere have been the most debated aspect of the climate change question. However, during the last decade, the effects of aerosols have

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121 About the panel, its structure and effect on science-policy interactions, see e.g. Siebenhüner (2003); Skovdín (2000); Miller (2004). For an analysis of how uncertainties are presented, see Mattsson (2005).
gained increased attention, perhaps partly because the IPCC reports present this effect as much more uncertain.

A paper about the consensus around the estimated changes in the global mean temperature due to climate change (van der Sluijs et al. 1998) illustrates some of the ways in which IPCC has established and maintained its credibility. Van der Sluijs et al. (1998) show how the consensus estimate of the magnitude of increased global temperature has remained unchanged for two decades, although the scientific knowledge and analyses have changed dramatically during this time (cf. Wynne 1996: 373). The IPCC report from 1990 presented the latest results of climate scenarios and demarcated a dividing-line for the delineation of what could be defined as “recent” results. In doing so, IPCC’s Working Group 1 excluded results from climate simulations (with GCM) that fell outside the estimate of global temperature increase (1.5–4.5 C), while at the same time failing to provide any scientific arguments or clear justification as to why recent results are automatically better or why results not in accordance with the previously accepted estimate were excluded (van der Sluijs et al. 1998: 300f.).

A change in the temperature range would focus attention on the methods originally used and their inadequacies, and this could be potentially embarrassing for the climate-modeling community (van der Sluijs et al. 1998: 302, 312). While the experts were not necessarily consciously engaged in these processes, they responded to a wider set of contingencies other than “purely” scientific considerations (van der Sluijs et al. 1998: 303). It can be suggested that this was important in order to create a credible basis for the climate change issue so that they could influence politicians, not least in making a stand against skeptical scientists and lobby groups from the energy industry.

While there was a close connection between science and policy during the years 1990 to 1992, science became of less importance to policy-makers as

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122 See also Skovdin (1994) on the negotiation about the global warming potential index.
123 The IPCC experts felt a great need for unambiguous scientific evidence before changing the range, but there was no equally great need for such evidence to maintain the range. The need for scientific rigor applied more strongly to changing the climate sensitivity range than it did for its maintenance. This also suggests that the specific status of the temperature range is much lower than is generally perceived by the public and this is not solely due to necessary simplification for public and policy comprehension (van der Sluijs et al. 1998: 302). To argue that changing the values would require weighty scientific justification is also to implicitly acknowledge that the public view of the range of temperature change is that they have been rigorously and precisely justified scientifically, when in fact they have not.
124 Van der Sluijs et al. (1998: 317) suggest in their conclusion that “conventional normative notions of ‘good science’ in research cultures may not necessarily be appropriate for science in policy arenas, even though this is typically how these notions have been institutionalized in policy advisory processes”.
125 See Edwards & Schneider (2001) for an exploration of the charges against IPCC after the second assessment report from 1995. See e.g. McCright & Dunlap (2003) for an interesting discussion about lobbying in the case of the climate change problem.
policy became more committed (Nolin 1999: 133f.). A backlash, which threatened the connection between science and policy, began to transpire in 1992. Within a context of scientific evaluation, the IPCC was still of prime importance in maintaining pressure and as an entity that displayed a front of scientific consensus. Yet policy-makers now had less need for the knowledge supplied by IPCC’s Working group I, compared to earlier Working group I scientists who had considerable influence on policy. They evaluated scientific results and encouraged policy makers to accept the threat of climate change as credible, but after the IPCC’s second report in 1995, policy makers became seekers of knowledge that could help them in the new context and it became important to relate policy to knowledge about burden sharing, economic modeling and energy research (Nolin 1999: 136). Consequently, atmospheric research became less important in the formation of policy after climate change had been established as an environmental and political problem. However, Shackley et al. (1999) find evidence that scientists’ perceptions of the policy process play a role in shaping their scientific practices (cf. van der Sluijs et al. 1998). Institutions like the IPCC constitute an important arena for negotiation about status, credibility and influence, and as perceptions of policy needs are built seamlessly into scientific interactions, the distinctions between internal and external audiences for scientific research become blurred (Shackley et al. 1999: 435).

In the following, I will adopt a more local perspective and focus on the Swedish development of meteorology at MISU, also known as the International Meteorological Institute (IMI), but also re-connect to the climate question and climate research.

The History of the Department of Meteorology

As the brief summary of the history of meteorology during the 20th century has shown, a number of researchers with close ties to Sweden and meteorology in Stockholm have held prominent positions in the early

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126 In specific cases, such as the use of certain adjustments, it is suggested that the concerns of policy makers could have influenced the use through indirect influences via research funding agencies and indirect pressures from scientists’ participation in policy oriented scientific assessment organizations, especially IPCC (Shackley et al. 1999).

127 Most of the scientists whom I have met who have been involved in assessment work draw a sharp line between science and politics and between internal and external audiences (cf. Lövbrand 2005). They also refuse to risk their scientific credibility in order to win battles on the arena of environmental politics. Discussions at assessment meetings demonstrate this but of course these do not reveal what happens in other arenas. However, to claim scientific integrity is also a matter of strengthening the credibility of scientific accounts (cf. Gieryn 1999) and that scientists distinguish between science and politics is hardly surprising considering that drawing this distinction is part of what science is about (cf. Latour 1999a; 2003a).
developments of numerical weather prediction. The core of an institute of meteorology was established at *Stockholm’s högskola* in 1947. Rossby came to Sweden in the same year with the vision of creating an international meeting place for scientists and this dream was realized when he founded the International Meteorological Institute (IMI) in 1955 (Bolin 1997: 4).

The IMI publishes a research report every second year, which includes short summaries of all projects. Every biennial report begins with a short historical summary about the founding of the institute.

IMI was created in 1955 by a decision of the Swedish parliament with the objective ‘to conduct research in meteorology and associated fields and to promote international scientific co-operation within meteorology’. (…) The most important function of the institute is to provide opportunities for foreign scientists to work in Sweden for varying periods of time in close collaboration with their Swedish colleagues.” (*IMI Biennial Report 2001–2002*: 3)

MISU is an integral part of IMI but the institute has a different board, appointed by the Swedish government, but with a director from MISU. Although the most important function of the institute is to promote collaboration, the most important function of MISU is related to education and research. To begin with, organized undergraduate courses in meteorology were not given at MISU and the training of meteorologists was organized by SMHI (Bolin 1997: 8). In the late 1960s, university courses replaced special courses that had previously been arranged by SMHI. Since then, MISU has been responsible for educating both meteorologists and graduate students.

The History of Research and Environmental Issues

During the first four years of its existence, MISU was located in the same houses as the weather bureau and the ties between MISU and the weather service have been very strong over the years. Dynamic meteorology and model development in general, and numerical weather forecasting in particular, were fundamental during the first ten years of the existence of the department (Bolin 1997: 4). Stockholm became a focal point for such

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128 In order to realize this goal, IMI receives direct financial support from the government. In 1955, this sum was 130 000 SEK. This grant originates from when Rossby convinced the government of the importance of international collaboration in meteorology at a time when World War II had proved the importance of good forecasts. Today the annual grant is about 2 million SEK (2002). Due to this direct support, researchers at MISU have an advantageous position when it comes to international cooperation. The money is not used for the trips of MISU-researchers but finances the many visits by scientists from abroad each year. Most of these visits are brief but some are of a more extended duration, sometimes lasting for several months. Visiting scientists who stay for longer periods often have continuous collaboration with scientists at MISU.
research, with Bolin in charge of the numerical forecasting. Most of the doctoral dissertations during the first 25 years concerned this topic and research did not start to develop in other directions until the 1960s.

During the 1930s and 40s, what was called “air pollution meteorology” existed in Sweden. According to one of the senior researchers, this field of research was mostly a study of elements and simple matters such as salts. This was before the 1950s, when researchers realized the importance of the interaction between gas and solid matter (interview with NN). Chemical meteorology started to develop as a field in the mid-1950s, and acid rain and trans-boundary air pollution became central research issues at the end of the 1960s. The atmospheric chemistry section was formed in 1969, and in the mid 1970s the initiative was taken to appoint a chair in this field. However, the chair in chemical meteorology was not instituted until 1980. Previously, there had only been one chair in meteorology at the department. The chair in chemical meteorology therefore represented a stabilization of a broader research basis at MISU, compared to the (narrow) focus on modeling related to numerical weather prediction.

Regarding environmental issues, ozone research is also worthy of comment. In the 1960s, researchers at MISU used numerical models in ozone research but more experimental research of the stratosphere using rockets also started (Nolin 1995: 79). Ozone research can be seen as a part of chemical meteorology, but because the ozone layer is located in the stratosphere, it is separated from the rest of chemically related research at MISU. This research focuses on processes in the troposphere (similar to dynamic meteorology) as dependent on meteorological conditions.

Over the last two decades, additional environmental effects of pollution, such as those affecting human health and in particular climate change, have gained increased attention. One researcher noted,

The climate question is and has become central. Weather forecasting was the big question earlier, form the 1850s to 1950 or perhaps 1960. Then air pollution came in and transports and pollution and that became a new main question within this area. Then came the environmentally related questions about depletion of the ozone layer. That became an additional question that does not have to do with weather but with atmospheric chemistry. Then, in the end, over the last 20 years, the climate question came concurrently with our influence on the climate. It is a strong driving force, a large part of the research.

Because of the special interest of the thesis, I will discuss this further in the next section.

\[129\] See Letell (2002) for an analysis of the emergence, stabilization, and institutionalization of a discourse on transboundary air pollution. For more policy oriented studies of air pollution, see e.g. Sundqvist et al. (2002); Sundqvist & Lidskog (2002).
Climate Research in Sweden and at MISU – Local and International Work

In an historical analysis of the development of climate research and politics in Sweden, Nolin (1999: 132f.) claims that the scientific research on the issue started to grow dramatically after 1990. In the following, I present his arguments in some detail since it is an extensive study of the development of Swedish climate research and policy.

According to Nolin (1999), disciplines were challenged by climate change studies and this was a problem for the Swedish research system, not least in terms of the strict disciplinary boundaries and the established system of university chairs. Swedish policy-makers also chose not to separately fund a global climate change program and disciplines therefore seized new money in the “old way” (Nolin 1999: 129). With the expansion of the field of research, MISU appeared as an obligatory point of passage to which all other national actors needed to have some link (Nolin 1999: 132). However, Nolin does not explain how or why MISU became an obligatory point of passage aside from noting that it was primarily here that climate research proliferated.

It is also unclear what happened with research activities when “scientific research on the [climate] issue started to grow dramatically” (Nolin 1999: 132). For example, was this was just a matter of “climate” starting to turn up as a keyword in the description of projects or were research questions actually turning in a new direction? There was very little “climate research” in Sweden until 1990, but to what extent does this reflect a change of labels rather than actual problems and activities? Concerning the research being carried out at some institutions (in Germany and the Netherlands), Nolin (1999: 128f.) writes that “they were in fact already involved in ‘climate-related research’, but many scientists did not then think of themselves as doing climate research, whereas today, two decades later, more or less similar studies are clearly labeled as climate-related research.” Consequently, it is unclear what types of research questions and activities began to gain in importance at this time, and how these were connected to climate.

Independent of what happened in terms of questions and activities, several researchers from MISU have indeed played a salient role from the perspective of international research organizations such as the IPCC and the International Geosphere-Biosphere Program (IGBP).130 However, existing studies of these researchers, international organization, and of politics

130 At least two researchers have also been explicitly interested in the relations between science and politics and they have been involved in establishing committees where scientific and political questions obviously have mixed. This type of role has been referred to as policy entrepreneurs, i.e. members of scientific elites who invest resources to advocate the incorporation of their ideas into policies (Hart & Victor 1993).
relating to science and environmental problems (see e.g. Elzinga & Nolin 1998; Nolin 1999; Sundqvist et al. 2002; Sundqvist & Lidskog 2002) more generally tend to lack information about the local practices involved in producing scientific results.¹³¹ Research work requires a division of labor (cf. Becker 1982). For people to work inside a research department, there have to be people outside recruiting resources (Latour 1987). In studies of scientific practice, completely different pictures of research emerge depending on which of these scientists one chooses “to follow”, for example, the scientists involved in public relations on “the outside” or the doctoral students “inside” who “just” do research. All of these people are “doing science”, but in different ways (Latour 1987: 155f.).

However, Latour (1987) does not consider how local research is affected by the fact that responsible scientists act as spokespersons for, and thereby represent, a wider network. For example, people working in international committees or spending time in the international political arena are more distant from local production work than are those who write funding applications. International work may therefore involve a further detachment from local research work. The job of applying for project funding is likely to also involve coordination of the project, and thus, direct influence and participation in local work, whereas work in international committees and panels is less likely to involve control of local research work. The fact that Bolin’s engagement in local research activities decreased (interview with NN) is in line with this idea.¹³² Supporting the idea is also the fact that “despite all this early, and, as it turned out, potentially very important work on CO₂, this [Bolin’s simulations of the enhanced greenhouse effect] never became a mainline strand of research” at MISU (Elzinga & Nolin 1998: 20).

Nonetheless, research work located far away from the local production work may still involve bringing in new resources and support to meteorology on a more general level, i.e. for the department and perhaps also for meteorological research at large rather than for specific research groups. For example, the special grant to IMI/MISU from the Swedish government exemplifies a type of general support. In the case of Bolin’s involvement in influential international research committees and programs such as the IGBP, World Climate Research program (WCRP), and perhaps most importantly the IPCC, this may have augmented the view of MISU as a

¹³¹ In most studies this is likely to be both a theoretical and an empirical question, restricted by practical concerns such as time constraints. However, it is worth pointing at the possibility of studying both "sides" from an actor-network perspective and seeing the difference between micro and macro as a matter of network size and scale rather than levels.

¹³² This is not a step away from research in general. First, the work “outside” is also research, as pointed out above. Second, in his commissions “outside” the laboratory, Bolin was appointed as a scientist, speaking on the basis of, if not his own research results, then on the basis of other scientists’ findings.
central place for climate research – independently of whether the internal research work was coordinated to that end.

However, Bolin can be seen not only as a spokesperson for MISU – in that it was there that he held his professor’s chair and where he produced the research results that later gave him the status and possibility to achieve such a prestigious position as president of IPCC – but also as a spokesperson for meteorology in the construction of climate change as an environmental problem. With his background as meteorologist, Bolin participated in establishing climate change as an environmental problem as well as meteorological research, with climate models leading the way, as a core in its solution. The IPCC reports have focused on climate models as the primary sources of knowledge in terms of climate change and throughout most of the 20th century, the history of climate politics has been intimately linked with the history of numerical models of the atmosphere (Edwards 2001: 41). As president for the IPCC, Bolin was one of the people involved in this.

It is also important to consider that several descriptions – both by Bolin and other writers – of the development of MISU, and also the climate question and the development of atmospheric science in Sweden, are based mainly on Bolin’s descriptions. Historical accounts should be treated as another source of data. In addition, it is important to read them as possible stabilizations of perspectives and positions. While meteorology appears crucial on the basis of the limited sources I have used to illustrate this historical background to my study of contemporary meteorological research, it is not self-evident what role meteorology actually played in raising the interest for possible anthropogenic climate change. However, to answer this question would require historical studies that are beyond the scope of this thesis.

The Current Situation at MISU

The sections at MISU are structured according to fields of research and include dynamical meteorology (DM), chemical meteorology (CM), and atmospheric physics (AP), of which the profiles of the first two have been mentioned above. While the sections are sources of identity and group feeling for people at MISU, at the same time, they also create a division between people who belong to different groups. During the first years of the 21st century, changes in members and research focus at the AP and CM

133 For instance, Wynne (1993: 8) notes that during the 1970s, researchers at the British Meteorological Office expressed how the threat of climate change was exaggerated and suggested by sciences less accountable than reductionist meteorology. The same Meteorological Office later came to dominate British climate research.
sections at MISU were salient. An important researcher in the AP-section left MISU in 2000 and this created some instability among the remaining group members. It also led to a decrease in the size of the technical workforce. In 2001, a previous member of the CM-section became the head of the AP-section. At the beginning of 2003, another group from CM also joined AP. AP used to be more of a big research group but because of the changes and the arrival of new members, AP became a more heterogeneous section. In addition to the traditional AP-studies of the upper and middle parts of the atmosphere involving measurements using rockets and satellites, research expanded to include the physics and chemistry of gases, aerosol particles, and clouds in the lower parts of the atmosphere.

Social worlds are negotiated orders, constituted by activities and processes (e.g. Strauss 1978a; Bucher & Strauss 1961) and disciplines are therefore not stable entities (cf. Fujimura 1992a). In one meteorological textbook, meteorology is described as a science where both physics and chemistry are applied in order to understand atmospheric processes (Lejenäs 2001). The laws and principles of thermodynamics, radiation, and mechanics are important for meteorological concepts, theories, and methods, and both physical and chemical processes are studied to understand the circulation of chemical elements, such as for example sulphate and carbon, in the atmosphere. However, some processes and theoretical tools are considered as more central than others. As was noted above, traditional weather oriented, dynamic meteorology was the basis for research when MISU was founded. From the point of view of current debates, it seems as though the expansion of research areas at MISU has caused some friction. Indeed, it appears that there are still tensions concerning what Rossby (1959/1956: 9, my emphasis) referred to as the “classical problem of meteorology, the quantitative analysis of the state and motion of the atmosphere based upon the laws of physics” and “some recent problems concerning the role played by the atmosphere as a carrier of insoluble minerals, soluble condensation nuclei and industrial pollutants” which “in the past have been considered to be of peripheral interest to meteorology” (Rossby 1959/1956: 9).

There were new reorganizations after my fieldwork completed. Two researchers left AP and MISU in the end of 2004 and AP became a more homogenous section again.

Studies of the chemistry and physics of the atmosphere are not only developed on the basis of meteorology. The study of atmospheric physics and chemistry as a scientific discipline dates back to the 18th century, when the principal issue concerned identifying the major chemical components of the atmosphere. In the late 19th and 20th centuries, attention turned to trace gases and aerosol particles (see e.g. www.atm.helsinki.fi/BACCI/index.php?action=prop 06/15/2003). At MISU, research referred to as atmospheric physics and chemistry, as opposed to (dynamic) meteorology, relates to classic meteorology by linking meteorological conditions to the study of, for instance, particle growth or cloud formation.

One researcher even told me that introducing chemical meteorology was considered a betrayal.
A relatively recent discussion about the official title attributed to PhDs at MISU illuminates how this controversy is still alive. When fulfilling a PhD at MISU, doctoral students receive the title of PhD in Meteorology, with an emphasis on either chemical meteorology, atmospheric physics, or dynamic meteorology. Thus, when looking at the graduate program at the department, there is still a strong emphasis on meteorology, as distinct from atmospheric chemistry or physics. There have been suggestions about changing the name of the program and when I attended a seminar about these questions, two sides were evident. The proponents of a new terminology suggest that this is necessary in order to make the name better reflect the contents of the program. According to the opposite side, the term “meteorology” in itself is not a problem because the other sections could also be seen as doing meteorological research. Yet, those who want a new title complain that public understanding does not correspond to this wider definition of meteorology and that this is harmful to their reputation, for example regarding applicants to the graduate program. During an interview, one researcher commented,

The debate [about the public and one’s own understanding of what meteorology is] is an internal debate here at the department since a long time. I think that one of the questions concerns meteorology being perceived as something narrower than our work. The other is that internally, at the department, there are slight differences in meaning, that real meteorology should be directed towards the weather, the dynamical, whereas others think that this is not the only thing that is important. Chemical and physical processes in the air, irrespective of whether it is weather or not, are important parts of the system.

From the point of view of the dynamically oriented researchers, as expressed during the seminar, it is the others’ problem to change the narrow, public understanding. The more sensitive question about changing the name of the department is related to this discussion. One researcher said, “Indeed our department is called the Meteorological Department but many people here think that we should change the name and call it the Department of Atmospheric Science instead, which is a wider concept”. This question has been discussed on a number of occasions, but according to another researcher it has always been silenced due to “massive resistance”. Dynamic meteorology still has a special position, while the part of the research staff that works in this area has decreased and now represents a minority.137 That the dynamic meteorological section is completely model oriented, whereas about half of the scientific staff at CM and a majority at AP are experimentally oriented, is likely to reinforce the tension. Undergraduate

137 There are also still close ties between the department and SMHI. For example, some people have positions in both organizations and the numerical weather prediction model used at SMHI is also used for projects and courses at MISU.
education is also likely to support the continued status of dynamic meteorology as the “pure” and original form of meteorology at the department.

From a social world perspective, this illustrates a debate about what types of research activities and research problems are “really” representative of meteorology (cf. Strauss 1993: 214). It is also an example of boundary work (cf. Gieryn 1983). Although differences between approaches to atmospheric processes from, for example, dynamic or chemical perspective are important, the difference between the work of simulation modelers and field experimentalists is most salient, as I have discussed in Chapter Three.138

There seems to be a strong sense of group feeling among modelers and experimentalists respectively, but there are also many similarities between the groups. Both modelers and experimentalists are preoccupied with producing scientific articles, but due to the interest in practical work with instruments rather than literary practices, the chapters where I deal with each subworld separately (Chapters Five and Six) do not focus much on this.139 Another similarity concerns the activity of visual interpretation, e.g. diagrams, “simulation movies”, etc. I will address visual interpretation occasionally, but this is not a major interest of the study due to my primary focus on the work involving inscription devices rather than inscriptions.140 Furthermore, there is no difference in the formal status of these two groups. For example, there are professors who are specialized in modeling as well as professors who work with experimentation.

The Tools of the Trade(s) – An Introduction

Similar to the introduction to weather and climate, the following section gives a brief conventional description of measuring instruments and simulation models.141 These are fundamental for the characterization of

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138 The terminology deserves a comment. The essence of experiments, in a laboratory as well as in a computer, is to put an external system into a chosen state and observe its subsequent behavior. Numerical modeling enables studies of the behavior of the atmosphere under controlled conditions in the same way as experimental models can be used in the laboratory (Nebeker 1995: 181). Thus, numerical modeling is more similar to a classic understanding of experiments than field experimentation. Numerical experimentation is a way of handling the fact that the atmosphere, as a material object, cannot be brought into a physical laboratory. As a substitute, experiments are performed inside “numerical laboratories”. Despite possible confusions, I will retain the terminology used by the researchers.

139 Compare the section on objects in scientific practice in Chapter Two.

140 Several laboratory ethnographers have studied and analyzed the interpretative activity of scientists in general and of visual representations in particular. The interpretational flexibility of scientific representations is one of the major findings of these studies. See e.g. Latour (1998); Knorr Cetina (1991); Henderson (1999); and especially Lynch & Woolgar (1990).

141 This section has been checked by a doctoral student in meteorology. See also the discussion on interactional expertise in Chapter Three.
research practices and the researchers involved in these practices. An experimentalist works with measuring instruments, a modeler with a model, and it is by combining (the) human and the artifacts s/he works with that “the scientist” (experimentalist or modeler) emerges (cf. Latour 1999: 192) as an effect of the rest of the situation s/he is situated in.

Measurement Equipment

Before summarizing some features of measurement equipment, it is important to note that measurements often take place during field campaigns. Campaigns are organized at various types of locations depending on the objectives of the project. Locations range from ships on the oceans, flights over land or oceans, baseline stations, cities, or remote places in forests or along coasts. Field campaigns expose a wide variety of instrument systems, which generally consist of different parts such as samplers, computers, electronic components, boxes, cables, tubes etc. They often comprise a combination of parts that are purchased from different manufacturers and then built in the laboratories. In addition to the main measurement instruments, additional “housekeeping” measurements serve the purpose of controlling the primary measurements and to assure that they will be correct and made under the right conditions.

Thus, experimental fieldwork in the atmospheric sciences involves a large variety of measuring instruments. Techniques are generally complex and absolute measurements are almost impossible. For example, “measuring” the mass of a particle is not like using a scale to measure our body mass, but requires that particles are, for example, evaporated, split by a laser beam, or ionized. There is a broad range of experimental research at MISU, based on for example mechanical, chemical, or electronic principles to measure wind, temperature, particle concentration, particle mass, particle flux, chemical composition of particles, cloud water, cloud condensation nuclei, etc.142 The variation becomes even more pronounced if one considers the different special adjustments that are required to mount an instrument on the ground, on a mast, under a balloon, in an airplane, or on a rocket. All of these various types of measurements are represented in the work of researchers at the department, and the range is even wider if one also considers their collaborators. Partly because my study concerns experimental work in general rather than specific projects or types of work, it is beyond the scope of this introduction to provide a detailed account of different measurement techniques. In the following, some common measurement instruments (used

142 Depending on the technique, different types of engineering expertise are required in order to build or modify measurement instruments. For instance, several instruments related to the chemistry of the particles require electrical engineering while some other instruments more related to physical characteristics require mechanical engineering.
by the experimentalists that I have met) are only mentioned and presented briefly.

Different types of radiometers are used for measuring radiation and various anemometers are used for measuring wind velocity and fluxes. For aerosol measurements, there are a variety of commonly used instruments such as the condensation particle counter (CPC), which is used to measure the density of selected aerosol particles. Optical particle counters (OPC) are often used for the same purpose but for larger particles. Instruments are also used in combination. For example, a differential mobility particle sizer typically consists of a differential mobility analyzer (for selecting aerosol particles according to their electrical mobility) and a CPC.\textsuperscript{143} A further example of a combined system is the aerosol eddy covariance system, which can be used to measure fluxes.\textsuperscript{144} This consists of an ultrasonic anemometer to measure vertical and horizontal wind and a CPC to measure the aerosol concentration.\textsuperscript{145} The set-ups can also change according to specific research questions. For example, the eddy correlation system can include an infra-red gas analyzer for measuring carbon dioxide and water content.

Researchers use impactors of different types for more chemical characterization of size distributions. For instance, in the Berner low-pressure impactor, the aerosol passes through several impactor stages and is classified into different size groups. Films and filters are used for collection, from which substrates are extracted. These are chemically analyzed at a later stage in the data processing procedure. Individual particles can also be examined with a transmission electron microscope.

Interaction between aerosols and clouds is a large area of research. This includes studies of the microphysical properties of clouds and uses for example nephelometers to measure scattering, and hygrometers to measure water vapor. The counterflow virtual impactor (CVI) is a device that separates different sized ice-crystals and droplets from the surrounding air into a warm, dry and particle-free airflow.\textsuperscript{146} Different sensors in the CVI-inlet can measure water vapor and non-volatile residuals left behind by evaporated crystals. It is often combined with other instruments, such as for example a passive cavity aerosol spectrometer in order to decide the residual number size distribution in a particular size range.

Although none of the experimental researchers at MISU use a mass spectrometer, the technique is notable because it seems to be popular in the field and is used in many field campaigns. In the field of research I have studied, mass spectrometers are most commonly used in the characterization

\textsuperscript{143} For an early presentation, see Knutson & Whtiby (1975). For use, see e.g. Seifert. (2003).
\textsuperscript{144} For original details of the CPC eddy covariance system, see Buzorios et al. (1998). For various aspects of its applications and operations, see Buzorios et al. (2000).
\textsuperscript{145} For more detailed accounts of aerosol technology in general, see e.g. Hinds (1998). See also Saari (2003) for a study of an aerosol technology group.
\textsuperscript{146} For original details of the CVI, see e.g. Noone et al. (1988).
of chemical composition of aerosols. Some types qualitatively characterize individual particles; others offer quantitative information for ensembles of particles. Additional types of mass spectrometers are used for the detection of trace gases. Compared to for example particle counters, these types of mass spectrometers are later developments and more expensive.

Simulation Models

Simulations produce a representation of a certain aspect of nature that may otherwise be extremely difficult to observe, and are often performed to learn about systems for which data are sparse. As “tools”, simulation models are more complex than measuring instruments. This section introduces simulation models and their structure with the purpose to facilitate the reader’s understanding of the analysis in subsequent chapters, in which I will further develop some of the aspects mentioned below.

Dynamic models of the atmosphere are developed in the field of dynamic meteorology. Most scientists who work with models at MISU use dynamic models and this is the type that I will refer to, unless I explicitly state otherwise. In dynamic meteorology, moving air masses are regarded in the same way as fluids in motion. In models, the full complexity of the atmosphere is reduced to a small number of physical laws and numerical versions of atmospheric models are constructed in order to simulate the processes that these laws describe. In the following, I refer to simulation modeling and numerical modeling as synonyms that refer to the production of computer simulations and modeling as all activities related to developing and using models, unless explicitly stated otherwise.

A way to distinguish between the different forms of models that numerical models actually consist of is to construct a hierarchy of models consisting of mechanical models, dynamical models, computational models, and models of phenomena (Winsberg 1999: 279ff.). The first step in a simulation study is to identify the theory under whose theoretical domain the phenomena of interest lie. A physical theory tells about the most idealized systems. Mechanical models are therefore required to apply them to real world systems. A mechanical model is a basic characterization of a physical system that allows the scientist to use the theoretical structure to assign a “family” of equations to the system. However, a mechanical model is too general to be able to describe a particular system.

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148 Two frameworks referred to as ”Eulerian” and ”Lagrangian” are commonly used to describe atmospheric behavior. For more details, see e.g. Salby (1992: 53).

149 See Cartwright (1983) for a (classic) philosophical discussion about these models.
In the case of dynamic models of the atmosphere, four equations describe atmospheric dynamics at a certain moment in time. They deal with motion and changes in wind, temperature, density, and pressure. The first equation, the Navier-Stokes equation, is based on Newton’s first law (force equation) and is used to describe wind velocity changes over time. It can be used for fluids and gas, e.g. air. The second equation is based on the first law of thermodynamics and describes temperature change over time. The equation of state (the ideal gas law) is usually the point of departure for the third equation. It indicates the relationship between pressure, temperature and density. The continuity equation describes density change over time, based on the law of the conservation of mass. These non-linear differential equations express changes in atmospheric parameters, including temperature, humidity, pressure, and wind velocities in three directions.

These equations cannot be solved analytically (except under highly idealized conditions). This means that it is impossible to write closed form equations (given known mathematical functions) that would represent a unique solution to the set of differential equations. By turning the equations into difference equations and discretizing the equations, modelers who want to simulate processes approximate the solution of the equation. Differential equations relate continuous rates of change over infinitesimal intervals and discretization relate rates of change over finite, or discrete, intervals. This enables computer calculations and the dynamic model can be transformed into a computational model (cf. Winsberg 1999: 281f.).

Because of their finite form, computational models are based on a large number of discrete points, a grid, where approximated equations are calculated for each point. The atmospheric equations are related to one another in a way that requires them to be calculated step-by-step together. The grid results in a one-, two- or three-dimensional model domain. In a one-dimensional model, the points constitute a vertical pillar. In a two-dimensional model, points are spread over an area or a vertical slice. In more advanced models the grid net constitutes a three-dimensional volume. General circulation models used in weather and climate prediction are examples of these types of models. Since the models are prognostic they also include a time dimension. The grid of global models covers the globe but many models cover more limited areas. The atmosphere is, however, in constant motion and it is therefore impossible to isolate (discrete) parts of the atmosphere. Consequently, limited area models use data produced by global models to set the boundary conditions of the limited area correctly. Regional forecasts models thus rely on global models.

A fine grid compensates for the lack of precision in the discrete approximation but the grid of an atmospheric model is relatively coarse. The resolution of a model refers to the distance between the grid points. In the most detailed 3-dimensional models, constructed for so-called Large Eddy Simulations, the distance can be from a few to a hundred meters. In three-
dimensional models used for research purposes only, the horizontal distance between grid points ranges from approximately one half to several kilometers. In numerical weather prediction models the grid is normally around 5–50 km. Space is also related to time in the sense that a refined resolution requires shorter time intervals. Although the distance between the grid points can be specified by the user – at least when models are used for research purposes – it is worth noting that some basic approximations and assumptions are only valid at larger scales (interview with NN). The consequence is that more powerful computer resources are not sufficient to increase the resolution of NWP-models. Some parts of the models have to be reconstructed. Moreover, increased resolution also puts pressure on the representation of more processes, since the importance of including different processes is related to the scale of the model.

In dynamic models, dynamic processes are explicitly resolved and therefore simulated by calculating equations for motion etc. in each grid point. In principle, the dynamic structure of models – the differential equations – has not changed since they were originally formulated by Bjerknes in the early 20th century. However, many important physical processes occur at smaller scales than the distance between the grid points. In order to account for the effects of these sub-grid processes in the simulation, they are expressed in a parameterized form as a function of the variables of the resolved, large-scale processes (e.g. temperature, humidity, wind velocity). For example, in climate and numerical weather prediction models, it is only the synoptic and planetary scale motions that are resolved while other processes are parameterized. However, parameterized processes in a simulation model with a sparse grid can sometimes be resolved in a model with a finer grid that allows more detail.

To parameterize can be compared to what Winsberg (1999: 282ff.) refers to as ad hoc modeling. This involves placing a rough and ready ad hoc model inside the context of the computational model itself. Ad hoc models typically involve relatively simple mathematical relationships that are designed to approximately capture some physical effect in nature. They often derive support from empirical findings not incorporated into fundamental

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150 Parameterizations generally represent physical – as opposed to dynamical – sub-grid processes. One modeler described the difference between the terms in the following way: “You speak of models and simulations a bit carelessly, almost as synonyms, but if you should be a bit strict I mean that simulation is when you resolve a lapse and describe it in detail, whereas modeling is when you describe a lapse that you cannot resolve as a function of what is resolved. (...) I think that parameterization and modeling is principally the same thing. It is an attempt to model, that is, to create an image, or a mirroring of, the result of a process that you cannot really resolve.”

151 The term synoptic designates the branch of meteorology that deals with the analysis of observations made simultaneously over a wide area. The term is commonly used to designate the characteristic scale of the disturbances that are depicted on weather maps (Holton 1992: 5).
theory. When they are coupled to theoretical equations of a simulation, it is assumed that they allow simulations to produce more realistic results than would have been possible without considering the effect of the physical process that the parameterization represents.

Once the computational model is implemented in the form of an algorithm, it produces results in the form of very large data sets. In order to interpret the data, modelers use visualization tools and mathematical analysis and it is by doing so that they arrive at the model of the phenomenon (Winsberg 1999: 283).

**Climate Models**

While detailed weather prediction extending beyond two weeks into the future is considered impossible because of theoretical considerations and available observations, using climate models to predict average conditions for extended periods (i.e. climate) is considered feasible (Schneider 1992: 20). However, there are different types of climate models. The most advanced connect models of different systems, for example atmospheric general circulation models are connected to ocean circulation models, but also to sea ice models and land surface process modeling. There is a hierarchy of atmospheric models, which can be zero to three dimensional, where the three-dimensional atmospheric general circulation models (AGCM) are the most comprehensive (e.g. Kiehl 1992). Unless stated otherwise, I will refer to AGCM as climate models.

What we today refer to as climate models have primarily developed from numerical weather prediction models. While they are conceptually similar, they are used in different ways (see e.g. Holton 1992; Edwards 1999). The origin of most climate models is numerical weather prediction models but they run for a longer period of time. One scientist stated that “The ones who used to work with forecast models have started to use them for climate simulations instead.” In any case, climate models are based on the same set of equations as NWP-models and several weather forecast models have been developed to produce climate predictions. One researcher said,

> These forecast models are principally the same as climate models, except that if you want to make a forecast for three or four days, you can forget about the fact that the ocean is warmer in the summer than in the winter and you can

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152 It was stated above that the limit of predictability of a forecast is about two weeks. First, it should be noted that this concerns the global-scale circulation of the atmosphere, for a small-scale circulation it can be on the order of a few minutes. Second, for timescales longer than the deterministic limit of predictability, only the statistics of the system can be predicted. In the case of climate change predictions, the time evolution of the statistics of the climate system are predictable to the extent that they are driven by predictable changes in some external force, such as for example projected increases in greenhouse gas concentrations (Randall & Wielicki 1997: 401). See also Shackley & Wynne (1996) for a discussion.

153 The RCA-model used at the Rossby center is a development of HIRLAM, the NWP-model that SMHI uses.
assume that it is just as warm in three days time. Then you can uncouple everything happening in the ocean, everything in the vegetation. The only thing you need to know is that when the wind blows, the trees hinder the wind, and that there are mountains and valleys. But after three days, these things can change. The first attempt to estimate the general circulation was through the first climate model. It was not called that but it was called the general circulation model. It was done in 1955 (...) and then it was realized that so much more needed to be included. In this way, forecast models in meteorology stimulation were the gateway to climate and climate models. At the same time, to handle the climate questions, other aspects were required from meteorology.

Consequently, climate models have the same structure as weather prediction models but have gradually taken into account more processes that are of importance in a longer time-scale. Processes that are considered irrelevant for, for example, weekly weather prediction, may be important in order to understand the development of longer term patterns. Therefore, climate modeling demands more physical parameterizations than models that are used to calculate the evolution of processes with shorter durations. These slower processes are difficult to observe, and therefore pose great challenges for climate modeling. They also have a coarser grid than models for forecasting in order to save computer power, but nonetheless require supercomputers for their calculations. The typical size of each box in the grid in a global climate model is 200–300 km horizontally and with a vertical extension of between a few hundred meters up to several kilometers, but the resolution can be both higher and lower. In these models, there are on the order of 100 000 grid points or more.

To conclude, this chapter has described the historical background of meteorology and MISU, including some comments about the current research at the department. The climate question and climate politics have also been discussed. On the basis of this, it can be concluded that when studying how researchers connect their research to the climate change question, it is important to take into account, firstly, that the central role of meteorology is already established, and secondly, that this role is an effect of how the climate change problem has been formulated and stabilized. As a consequence of the focus on the artifacts involved in scientific practice, the chapter concluded with an introduction of the tools that are involved in meteorological research practices. In the following chapters I will show how measuring instruments and simulation models are not just ”tools”, but how they also play various different roles in practices.

154 See e.g. www.smhi.se/sgn0106/if/rc/faq.htm 1/7/2002.
5. The Subworld of Field Experimentation: Mobilizing Networks

“Why did you want to study atmospheric science? Because of all the collaborations?”

This was a question posed to me by a scientist I met during a collaborative field campaign to which I was invited by another scientist. He approached me by saying “as an experimentalist, I think you have to come and see what it is like on a field campaign.”

Collaboration is an important part of scientific practice and work. “The interactive process of collaboration is germane to the conduct of science and insights into how collaboration is practiced capture some of the essence of how science functions” (Schild 1996: 69). This chapter presents the subworld of experimentalists as being about organized collaborations that are constituted by different working practices.155

While social aspects have been considered in some previous accounts of collaboration, the role of material objects, such as measurement equipment and logistics in collaborative work, have received considerably less attention (e.g. Kraut et al. 1988; Hagstrom 1965). However, technological practice has been identified as a key in understanding collaboration on the basis of quantitative analysis of collaborative work (Chompalov & Shrum 1999).156 Furthermore, Schild (1996: 236f.) concludes that logistics shape both the motives that bring collaborators together and the nature of collaborative work, thus taking the material aspects of scientific practice into

155 See Chapter Four in Schild (1996) for a detailed discussion of collaboration as a work process and Chapter Three in the same book for an excellent discussion motivating the need for it.

156 However, it should be pointed out that the article analyses multi-institutional collaborations on a macro-level and that they employ a broad definition of technological practice which “does not focus exclusively on hardware; rather, it incorporates the diverse ways that instrumentation and analytical tasks are organized, characteristics of the management of topics, and technical change and uses of equipment.” (Chomapalov & Shrum 1999: 365) It is also notable that the question of how to quantitatively measure collaboration and its productivity have been a topic in several studies as well as collaboration in relation to co-authorship, see e.g. Katz & Martin (1997); Laband & Tollison (2000). See Harsanyi (1993) for an overview of bibliometric literature on research collaboration. For a more qualitatively oriented study, see e.g. Trimbur & Braun (1992).
consideration. Because of the fundamental role of measurement equipment in collecting data – the primary concern of the experimental field on the most general level – measurement technology in itself should not be underestimated in explaining patterns of collaboration. Collaboration, in turn, is a type of networking. Networks include both people and things and the network concept therefore allows one to view “collaboration” as not exclusively occurring among researchers, but among different practices which incorporate people, such as researchers and technical staff, and also measurement equipment. For this reason, collaboration enrolls both people and artifacts.

To establish useful collaborations – successful networking – scientists must make choices about how to build the most productive coalitions. In order to take advantage of new opportunities, groups of scientists are characterized by changing boundaries that are defined and redefined to incorporate scientists from other research networks or specialties (Atkinson et al. 1998: 26). However, scientists have commitments both to existing material resources and support personnel. Altering the approach to certain research problems requires making potentially expensive changes, such as hiring new staff and/or buying new instruments (cf. Fujimura 1992b: 104). This makes it a great advantage to be able to organize projects that incorporate the resources of many scientists with different equipment and skills. This is achieved by articulation work. Mobilizing different elements as resources becomes an important aspect of experimental research and this often involves collaboration with other research groups. How do they organize central activities? What types of practices does this organization of activities include? What kinds of relations are established between different scientists and their material resources through these practices? These issues are discussed in this chapter.

Experimental work in meteorological research is characterized by projects where several research groups participate, particularly regarding data collection. Several reasons can be suggested to explain why these types of projects are common in (experimental) atmospheric science and meteorology. Some scientists suggest that it is because of the character of the atmosphere as an epistemic object: air has no borders. Another reason is economic. Some types of field campaigns require cost sharing because of the expense in using an airplane or ship (but field campaigns are expensive also because they are collaborative). In addition, they enable scientists to share measurement data, a most essential resource in experimental research. The European Union also encourages collaborative research in general by

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157 Groups are the norm in experimental science and free collaboration in these fields is more likely to occur between groups than between individuals (Hagstrom 1965: 118,121).
offering grants that presuppose collaboration. This issue will not be analyzed in any detail since the purpose of this chapter is to explore how experimental networking is organized and to add to the understanding of reasons for connecting various practices by focusing more on the role of measurement technology in this.\textsuperscript{159} It is by trying to understand the nature of collaborations – how scientists collaborate – that we can gain insight into their motives and why they collaborate (Schild 1996:71).

In the previous chapter, I commented on the “boundary work” (cf. Gieryn 1999) between scientists at MISU regarding how they interpret “meteorology”. Many scientists, especially experimentalists, have a stronger affiliation to the broader conception of “atmospheric science”. Field campaigns illustrate how experimentalists at MISU have a wide basis for their collaborative networks. They are not limited to peer-meteorologists. Experimental fieldwork takes place in a context of atmospheric science in a broader sense than the narrower definition of classical meteorology. Yet this chapter is not an analysis of interdisciplinarity, but rather of the disunity or the heterogeneity of experimental working practice (cf. Galison & Stump 1996; Weingart & Stehr 2001).\textsuperscript{160}

Distinguishing between fieldwork and non-fieldwork collaborations (Schild 1996), this chapter focuses upon the general organization of field campaigns. These, however, often involve post-field collaboration in the context of data interpretation and the writing of articles (thus, a kind of non-fieldwork collaboration).\textsuperscript{161} Nonetheless, field campaigns are central in experimental research practice because it is during these campaigns that scientists make the measurements that are subsequently interpreted and which result in articles. As a project, field campaign collaboration makes up an arc of work where tasks are planned, coordinated, and integrated. According to Strauss (1985: 83), each arc usually involves several types of work and it is likely that subunits form relationships with others in order for the project to accomplish its aims. As a way to illuminate how these arcs of work function, I present a typology of working practices.

The chapter is organized in the following way. After presenting and motivating the typology of working practices that structure this chapter, I outline each type of experimental practice in more detail. This includes my analysis of experimental networking in general and the initial organization of field campaigns in particular. A separate section is devoted to a discussion of

\textsuperscript{159} For studies that aimed to identify factors that account for research collaboration, see e.g. Qin (1994); Pao (1992).

\textsuperscript{160} Disciplinary affiliation is principally irrelevant in these field collaborations (unless they are explicitly directed towards an interdisciplinary agenda), although it sometimes seems of importance for the collaborators in order to point at differences in status.

\textsuperscript{161} Thus, in this analysis of collaboration, projects, but not programs, are included. See more below.
the role of measurement instruments. A summarizing discussion concludes the chapter.

A Typology of Experimentalist Working Practices

In order to analyze how experimental work is organized, I argue that we can understand it as a network of related practices involving scientists and the artifacts that their work is tied to. A typology of practices based on the dimensions “organization” and “commitment” serves to structure the presentation of experimental work. This is shown in the table below.

Table 1.

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<tr>
<th>Organizational role</th>
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<tr>
<td><strong>Primary commitment</strong></td>
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<tr>
<td>Atmosphere</td>
<td>Entrepreneurship</td>
<td>Integrated experimentation</td>
</tr>
<tr>
<td>Instrument</td>
<td>Technical entrepreneurship</td>
<td>Instrument expertise</td>
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Working practices in the experimental subworld

Organizational role concerns whether scientists are organizing and coordinating research or if they are participating in a project initiated by other scientists or groups. This includes being invited to participate as well as asking to participate. The second dimension is more directly related to production and concerns the relationship between scientist and instrument. This is ordinarily intimate (e.g. Mallard 1998: 585) but the character and strength of the tie can differ in various respects, as I will show below. The dimension called primary commitment strives to capture the main focus of work. The alternatives are instrument and atmosphere, reflecting whether the scientist focuses on understanding and improving the performance of the instrument or understanding and learning about the atmosphere. In the latter case, the use of the instrument is a means to an end. This gives the categories, namely entrepreneurship, integrated experimentation, instrument expertise, and technical entrepreneurship. In integrated experimentation and entrepreneurship, instruments are applied – sometimes as “black boxes” (cf. Latour 1987) – whereas instrument expertise and technical entrepreneurship involve the construction and development of measurement technology. The features of these types, including their boundaries, will be elaborated in the presentation of the different categories. Technical entrepreneurship is not of great importance in the articulation of a field campaign (the arc of work) and is therefore not as thoroughly discussed.
I use examples from field notes and quotations from informants to illustrate and exemplify the practices. While the activities, concerns, and relations making up these practices are most central, different formal positions – such as scientists, doctoral students and technical staff – are integrated into working practice and will also receive some attention. I will briefly discuss the position of technical staff in the different types of practices (cf. Becker 1974: 767) as opposed to some studies with an actor-network approach, particularly by Latour (e.g. Latour 1983; 1988a). These have a strong focus on entrepreneurs and thereby promote a kind of individualism (Landström 1998: 77; see also Fujimura 1992a). The “scientist” is constructed as a spokesperson who claims to represent “nature”. The roles of other human agents are neglected. However, it is important to distinguish between entrepreneurs and spokespersons, because these are different roles. In the typology presented above, entrepreneurship is suggested as one type of role among others. This also includes spokespersons but in different ways. As a whole, the typology presents a network of practices, based on relations that constitute the organized subworld of experimental atmospheric science.

Entrepreneurship

“Contacts are essential in this business”

The above quote relates the words of a scientist who was talking about how to organize a collaborative project. When scientists seek to collaborate, they consider who would be most productive to collaborate with (Atkinson et al. 1998). The “entrepreneur” brings together the necessary material and human resources from the point of view of his/her own goals and interests (cf. Schumpeter 1911/2000). Entrepreneurship involves both an idea about a project and an idea on how it should be organized.162 However, it is of

162 The role of “ideas” concerns who is bringing people and material together, not who is “responsible” for the success of a campaign. Initiators are but a few of a crowd doing the work, but the focus of the initiators in science literature explains how it seems that they have made most of the work, while they actually have been supported by many others (cf. Latour 1987: 134f.). Entrepreneurial practice includes both initiating and coordinating fieldwork collaborations. The campaign coordinator is responsible for the preparations before instrument systems can be installed at the site and makes sure that everyone has space at the right location and in the correct conditions (in terms of heat, electricity etc.) in order to make the planned measurements. It is also the coordinator or the group s/he belongs to that takes responsibility for the overall organization of the campaign. The work of the coordinator could generally be described as what Schild (1996: 89) distinguishes as “working things out”, a form of articulation work in collaboration. It is important to note that while the coordinator is responsible for this explicit articulation work, articulation takes place everywhere (cf. Strauss 1987: 106f.). Regarding the people involved in this, I suggest that the larger the group of
crucial importance to be sufficiently open while enabling participants to pursue their own careers. In relation to making other researchers interested, one researcher said,

New questions arise and you can create new hypotheses and then it is about selling them, then you are just really a salesperson: Do you want to join us and look at this? These are my crazy ideas. But perhaps you also think it would be interesting to have a look at them?

Thus, the quote illustrates how entrepreneurship includes tailoring (selling) the research questions in such a way that they appeal to different interests (cf. Latour 1987: 109).

In very large projects, it is open for potential participants to apply for participation. More commonly, however, entrepreneurship involves either turning to other scientists to ask if they wish to participate – by using already acquired funds or by jointly applying for funds that are then divided among the participants – or by inviting them to participate and then paying them. In the latter case, the invited scientists are therefore not involved in the fund raising process. One researcher told me about the organization of a project in the following way:

It’s about finding the right people (…) and then you have to put together a science implementation plan. (…) Then we divide the project and put a frame around it. Then we are a number of players in the group, a core group, and then you start to apply for money when you have created this document, or in parallel so to speak, not first this and then that.

An important point in terms of the further analysis in this chapter is the respondent’s mention of a “core group”. Although this implies that the project will eventually include other participants, it is the core group that will actually plan the project more in detail at the same time as they start to apply for funding. As the quotation that introduced the section also suggests, contacts are important when finding “the right people”. I will discuss this relation (personal contact), as well as how one chooses who to ask, in the sections concerning the two types of practices that are enrolled in the network – integrated experimentation and instrument expertise. At this point, I will focus on collaborative interpretation, an important feature of entrepreneurial practice in that it illustrates the mobilization of enrolled participants (cf. Callon 1986).
Entrepreneurial Interpretation

A primary interest in entrepreneurship is to interpret the data that measurements have produced. Measurements and measurement technology are merely means to an end in this endeavor. “My research interests in this project are independent of instruments. I have the questions and the hypotheses and it is those I want to write about”, said one scientist who exemplifies this orientation.

One salient aspect of the interpretation of data within experimental atmospheric science is the comparison of measurements from different instruments. There is always more than one measurement that has to be studied, and although a scientist may be mostly interested in the data obtained from the use of a certain instrument, data is always compared and interpreted in relation to other measurements in order to contextualize the particular measurements of interest. For example, the scientist compares measurements of the concentration of some chemical substance in the air with the values of meteorological variables such as wind, temperature, and air pressure.163

However, a characteristic of entrepreneurial strategy is the attempt to bring together many data sets obtained from the main instruments used by several collaborators. A consequence is that “entrepreneurs” become more interested in, and dependent on, the collaborators who provide them with data, in order to enable a combination of all resources. At the end of a large meeting of the principal investigators of a series of campaigns, the coordinator unexpectedly suggested that the data from the ongoing field campaign should be processed before the time that would otherwise have been a deadline for deliveries to the database. The coordinator said that it “means that we have to push some of you!” The purpose of this proposal was to write a joint paper that could be submitted to either Science or Nature, the most prestigious scientific journals in terms of innovative scientific “findings” in all fields of research. Two leaders of the initiating “core group” immediately supported the coordinator, which gave me as an observer the impression that this was a joint decision taken by the group leaders who had initiated the project. When I spoke to some participants about this suggestion after the meeting, they were skeptical because they thought they would not have the time and that it was too optimistic.

This example illustrates how entrepreneurship involves a greater interest in coordinated activity, which also makes it more likely that it is the researcher or group who has initiated the project – playing the role of

163 Regarding the general work with data, one informant said, "It is fixing. You have to find certain wind directions, perhaps you have to look at certain points in time and select, look at weekends separately, evenings separately, and then sit and pick with your data and twist and turn it and see if you can find some conclusions. It is not always that you know from the beginning (...) what type of correlation you can find.”
entrepreneur – who is responsible for organizing workshops and similar arrangements where the participants can meet (cf. Schild 1996: 202). I will discuss some more aspects of collaborative interpretation by taking a specific event as my point of departure. The example is based on my observations during several sessions, where scientists who participated in a project met in order to work with and interpret data sets from a campaign.

The participants in this collaboration had different backgrounds and interests in the project. In order to structure the meetings, the coordinator opened the workshop by suggesting some questions that he had formulated for the meetings as a point of departure for discussions. These questions were important for the coordinator and he also took a very active role in guiding the discussion about possible interpretations.

One of the most striking features during the workshop was the large amount of images that were used in order to compare the results obtained from different measurements, represented by the images, and which had been carried out by the different workshop participants. In addition to different types of diagrams, photographs where also studied, in order to have one complementary source of weather information regarding, for example, cloudiness. Generally guiding the interpretation of these images was the recurrent comment or request by the organizer of the workshop: “What does this image tell us of importance in terms of this question?” On some occasions, a feature of a specific diagram that showed a single set of measurements was taken as a point of departure. “Look, this is what we see here. Where does this peak come from?” being one example of a type of question that could guide the study of other diagrams in relation to the one of specific interest. One case was chosen and all other measurements were used in order to understand the details of that case. During this situation, it was the coordinator who guided the discussion of how the diagram should best be interpreted by asking different questions that seemed related to his research interests.

Thus, pictures and diagrams are not transparent representations of natural phenomena and scientists construct and negotiate the meaning of visual material (see e.g. Lynch & Woolgar 1990). This requires interaction with the pictures and with other participants, and an active questioning of the representational ties between the picture and the different mediators of the experiment. This interactive process reconstructs what happened during the measurement. The essential task during data interpretation is to align

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164 For an interesting discussion about different types of imaging, see Idhe (2004).
165 The images shown during the sessions functioned as interfaces between the different experimental researchers. An interface is the means by which interaction or communication is effected at the places where different people meet or different social worlds intersect (Fujimura 1992b: 124). Due to the overall focus of the chapter, I will not develop this line of reasoning further.
measurements with a reference (in nature). Alexander Mallard (1998: 594) points out that the alignment should not just be with an arbitrary reference but with one that is true. Causes and effects are identified in the course of interaction and the significance of a peak often has to be negotiated (cf. Mallard 1998: 593).

Returning to the interpretation in relation to entrepreneurship, the coordinator of this workshop exemplifies entrepreneurial practice by trying to lead the others and by guiding their interpretation according to his research interests. Accordingly, this can be said to exemplify a case of the mobilization of allies. The other collaborators work by themselves on their own results, often in a manner relating to an interest in what one scientist refers to as their “pet question” (cf. Star 1983: 223). However, what a “pet question” or a central question is, is a matter of perspective. This partly relates to who is organizing the project, but is also related to the organization of projects as a whole. Everybody has to have some interest in the central questions of the project otherwise there would be no point in participating. It is in relation to this common interest that the commitment of each participant, except for the “entrepreneur”, appears in the form of “pet questions”.

The “central” questions and the “pet” questions influence how articles are written, as is illustrated in the quote below.

If you have a special instrument, then you perhaps have a special interest, a special question or something. So often if you come with your own instrument, you write an article as first author. (…) Like James who came with an advanced gas phase instrument, he had a particular question about a certain gas, a vertical profile for instance. He will write about it and that is perhaps not a main question in our aim but he had it as a side question. Then he writes about that but then he is co-author if I use his material and interpret it together with him, then he is part of my article, and if it is the other way round, I am part of his article.

This quote also points at the significance of publishing from a particular project, especially as a coordinator. Furthermore, it is in line with the

166 Landström (1998: 95f.) criticizes the idea of science as primarily spokesmanship of nature partly on the basis of the observation that in the laboratory, few people spoke about nature. In everyday work, many more practical issues are central, such as for instance controlling why the data processing program stopped running. This impression can partly be the effect of the locality of the study, a “traditional” molecular biology laboratory. During interpretation of atmospheric data, it is essential to bridge the gap between diagrams and measurement equipment and speak about nature and what happened in the atmosphere at the time of the measurements.

167 Compare also Fujimura’s (1987) example of how sub-projects are articulated with the larger research program.

168 Within the social world tradition, questions of authorship have been related to the question of individual careers (see e.g. Fujimura 1987; Schild 1996). See also Knorr Cetina (1999) who connects authorship to the organization of work.
suggestion that one aspect of entrepreneurship is to take most responsibility for the joint post-fieldwork phase, and thereby draw scientists and their material together in order to answer the central questions of the project, as mentioned above. In actor-network terms, workshops are essential in order to keep the network together and to (enroll and) mobilize allies.

An additional point is that this mobilization does not seem to hinder the other participants, representing integrated experimentaion and instrument expertise practices. They can produce articles on their own “pet questions”, in addition to the papers they co-author with the initiator(s). This is crucial from the point of view of project organization.

As a brief summary, it can be said that the project has to be formulated around a common area of concern in such a way that it is sufficiently open to allow others to participate and pursue their own interests “on the side”, but also sufficiently narrow to allow the initiator to make use of the contributions of all participants.

Integrated Experimentation

Compared to entrepreneurship, integrated experimentation includes more concentration on one’s own data sets and less use of data from other measurements. However, the two most characteristic features of integrated experimentation are, firstly, the focus on the interpretation of measurements rather than developing measurement instruments, and secondly, participation in larger projects initiated by others (or organizing small projects with only themselves participating).

How do integrated experimentalists become involved in projects? The following quotation comes from a scientist who told me about how she got involved in two projects organized by the same group of scientists.

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169 Since the initiator of a project is often included on more lists of authors than the rest of the participants when papers based on the project are written, initiators of projects will quantitatively have the best pay-off on the list of publications. Nevertheless, organizing collaborations takes longer time than participating in them, which is a reason why post-field collaborations are of importance in entrepreneurial practice, as the following citation illustrates: “Since I am the coordinator – this is the project I am mainly working with – for sure, I have to publish from this.” Collaborators, on the other hand, can be involved in more projects and have a better pay-off because of the quantity of collaborations.

170 The term the integrated experimentalist paraphrases integrated professional (Becker 1982: 228f.), who has the technical and social abilities and the apparatus necessary to make art. Because they know and habitually use the conventions on which their world runs, they fit easily into its standard activities.

171 This follows from the fact that it is the researchers who act as entrepreneurs who initiate and design projects from the point of view of their research interests. They ask others, who they believe can contribute to this, to join.
It is because you are doing something that is interesting for the project, that is
the first thing, but not the only important. It is a lot about personal contact. I
did not have personal contact, or just partly, but those who I work with here,
some of them at least, have worked a long time with these people and have
had these contacts for a long time. So I got into this because I had a package
that fitted into this project and I was working with something that was
interesting and then it was the contact way. It was like this one time. Next
time, it was through contacts like projects. So it is double, so to speak.

This quotation suggests two factors that are of importance if one is to be
invited or asked to participate: personal contact and useful equipment. These
aspects are discussed below.

The important role of a personal relationship has been a recurrent theme
in studies of scientific collaboration (e.g. Hagstrom 1965). “The
establishment and maintenance of a personal relationship is the glue that
holds together the pieces of a collaborative research effort” (Kraut et al.
only a way to make fieldwork more pleasant. It is also potentially fruitful
from the point of view of future collaboration in new projects, in the same
way as networking during post doc visits and conferences.

A possible way to distinguish between core elements of collaboration is to
make a distinction between “affective” collaborations, driven and shaped
largely by personal relations, and “instrumental” collaborations, driven by
desire to execute a task, but these aspects appear in all collaborations (Kraut
et al. 1988). Nonetheless, I would like to suggest that the significance of a
personal relationship differs between researchers involved in integrated
experimentation and instrument expertise. This will be discussed in the next
section. In integrated experimental practice, the personal relationship with
the group or researcher organizing the project (the “entrepreneur”) is a
primary reason for cooperation. Recalling the discussion on “pet questions”
however, I also suggest that the “pet questions” of integrated
experimentation are closer to the central (pet) question espoused by

172 During one of the campaigns I visited, I followed my host around as he tried to say hello to
everyone in the small barracks or temporary cabins, referred to as containers, where the
research groups kept their instruments and made measurements. He knew some from previous
campaigns and collaborations but others were unfamiliar also to him. Notable is also how the
establishment of personal contact is often connected to instruments, both as a reason for
interest and as a conversational topic. For example, my host was especially interested in
visiting the measurement facilities of groups with the most advanced measuring equipment
and conversations during meals at the campaign often started with the question “How is your
instrument going?”

173 About the start of a project, one senior scientist summarized that it often starts with an
idea, you write an application, and then you call a colleague that you enjoy working with and
who you think can contribute. This implies that scientists tend to choose new collaborators on
the basis of those people one has collaborated successfully with on earlier occasions. On
previous collaboration, see also Strauss (1985: 87).
“entrepreneurs” due to their similar scientific commitments to the atmosphere rather than to the measuring instruments.

At the same time, an instrumental element is always present in collaborations and this highlights the need for every participant to contribute with something specific in line with the interests of the organizing group or researcher playing the role of entrepreneur. How does integrated experimentation contribute to the project as a whole? Since measurements form the basis of any experimental work, I suggest that the most important way to contribute is to provide useful instrumentation, as the quotation above also suggested (in terms of how the researcher had a “package” that fitted in). In atmospheric experimental research, instrument resources of others are of primary interest in seeking collaborators for productive cooperation. One experimentalist exemplified this by saying “Throughout, it's like that people are interesting, a bit harshly that people are interesting on the basis of the instrument they have.” Thus, scientists are dependent on their instruments for their own production work, but they are also dependent on them to satisfy articulation work done by others and for enrolment into research projects.

The instruments that are used in integrated experimentation belong to the conventional instruments of the field, and have been in use for a long time. The consequence of this is that, at least from a technological point of view, the researchers involved in integrated experimentation become replaceable because of their lack of originality (cf. Latour & Woolgar 1979/1986: 219). Therefore, they are more dependent on personal relationships in order to participate in collaborations (enroll in networks). In this sense, “integrated” refers to social integration in the world of experimental scientists. At the same time, because the instruments used in integrated experimentation are conventional and thus well integrated in the same scientific subworld – this can also be seen as referring to a technological integration.

In addition, a close personal relationship enables researchers involved in the practice of integrated experimentation to participate in collaborations even without having the necessary instrument resources. This is made possible by hiring or borrowing instruments from another research group. This is an opportunity open to researchers in integrated experimental practice because they use conventional equipment and have privileged contacts. Consequently, the use of conventional instruments is both constraining and enabling. Borrowing an instrument implies that there are standards regarding how it should be used (sometimes presented in manuals) as well as about how the resulting measurement data should be processed.

\footnote{Scientists are also invited to participate on the basis of their interpretational skills. However, \textit{on the basis of my material}, my impression is that this is rare and relies on close personal contacts to an even larger extent than the other examples. This is also in line with Schild’s (1996: 205) conclusion that collaborators in non-field collaborations tended to know each other better than in fieldwork projects.}
The use and combination of several instruments is another feature of this type of practice that further facilitates borrowing or hiring instruments. The possibility to borrow an instrument from another scientist or group and the combination of instruments together further implies an important aspect of integrated experimental practice: general skills in setting up and taking care of instruments.\textsuperscript{175}

The Use of Instruments and Relation to Technical Staff

This section concerns the role of instruments in integrated experimentation and entrepreneurship by presenting the work that is conducted with measurement instruments, which is bound to these practices. Because entrepreneurship and integrated experimentation involves a similar relation between the instrument and the user, this analysis is valid for both practices.

At the department, scientists rely on two specific localities, other than their own office. On the one hand, the laboratory, where instruments are developed and configurations including instruments and data acquisition systems are installed and tested. On the other hand, the workshop, where instruments can be built, rebuilt or adjusted. In general, scientists only work here during preparations for campaigns, but the venues are permanent work locations for technical staff. This section briefly attends to the role of technical staff, whose work is closely related to instruments.

Integrated experimental- and entrepreneurial practice involves using one or a combination of more or less conventional instruments. The techniques are not focused as such and according to scientists using these types of measuring instruments, it is not necessarily the instruments in themselves that are difficult to handle, but rather that different combinations require different skills. Combining these instruments involves set-up, use, and interpretation. The following citation from a scientist illustrates the difficulties involved.

It takes a while to develop the experience with the instrumentation. You need to put a whole lot of stuff together. Let’s take a typical (...) payload, you have one of these particle counters, you have an OPC, we’re trying to a put a DMA in the payload, we’ll have samples, filter samples for chemical analysis, we have this hygrometer that we use, and each one of those techniques takes a while to learn how to use, and (...) how to first massage the data into a usable form and how to interpret that data. What you really

\textsuperscript{175} One of the research groups at one campaign exemplifies these aspects of integrated experimental practice in that they had brought several instruments, of which all were not part of the “deliveries”. For instance, they had loaned one to another scientist. The group leader had also brought two additional instruments in order to develop the techniques. He was not familiar with one of them but said that using an instrument at a campaign was a good way of “getting to know the instrument.”
want to do is to get the information that you’re interested in is to combine all those instruments, put them together and then get a result.

Set-up involves solving a lot of technical and practical issues, and while technical staff usually assists scientists during the set-up of equipment, they rarely participate during measurements. Their work rather focuses on instruments in the laboratories or workshop where they either participate in the preparation of campaigns (building, testing, modifying) or afterwards (chemical analysis). Building instruments takes a lot of time but it is cheaper than buying them, and although integrated experimentalists tend to use “off the shelf” instruments, they often have to modify them, as the quotation below exemplifies.

You can buy them and there is one company specialized in aerosols, (...) but a lot of people make their own, we make our own. It’s also interesting for research that (...) the first thing we do with an instrument – even if it is a commercial instrument – is to take it all apart and change it and modify it and do things with it that it normally does not “want to do”, put it on an airplane and fly it around, or on a balloon or a rocket. So there are very few instruments that we can simply buy and turn on.

Different types of developments enable new ways to measure. A scientist mentioned three basic approaches: “Either you take existing measurement techniques and try to apply them collaboratively so that it will help you, or you try to develop new techniques, or you modify existing technique.” In

176 The division of labor among scientific and technical staff can be related to what Landström (1998: 94f.) calls institutionalized spokemanship. Whereas doctoral students and scientists sometimes take care of instrument development, chemical analysis, or quality assurance, technical staff do not participate in the interpretation of data. However, everyone who belongs to what is formally referred to as the “scientific staff” has a principal right to speak of (the scientific understanding of) nature. Over a meal at a campaign, a couple of scientists told me about data processing, which they consider as a rather “technical” and “boring” work. According to them, it is not fair to let students work with data unless they were going to “use it” themselves, in other words to write an article about it or at least participate in the interpretation of the data. This involves telling something (new) about nature. Thus, at the lowest position on the academic ladder, one still has the right to be a spokesperson, and this is a clear division between technical and scientific staff. Nutch (1996: 226) notes that doctoral students and technical staff sometimes work with similar kinds of tasks and doctoral students thereby act as temporary technicians and nearly as scientists. In line with this, technical staff working in the laboratories told me about how they teach or collaborate with doctoral students but that the students leave sooner or later. It is also interesting to discuss this in relation to Latour’s (1987: 72) emphasis that the notion of spokesperson should not be limited by imposing a clear distinction between “things” and “people” in advance. From the spokesperson’s point of view there is no distinction to be made between representing people and representing things. However, in terms of people, the spokesperson only has the right to represent him/herself and the people in a lower position of the hierarchy. There is thus an institutional difference in terms of who gets to represent whom (but not what). See also Latour and Woolgar (1979/1986: 218) about the comparison of technicians and scientists as workers and investors.

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integrated experimentation (and entrepreneurship), measurement innovations primarily draw upon novel combinations of instruments, as well as different ways of sampling, constructing inlets, data acquisition systems etc, not by developing basic measurement techniques or methods. The following quotation from an engineer illustrates this.

There are some problems, if there is a major change. They don’t want to change method, because this is a method they have tested before or that someone else has tested before and if you change too much it becomes a new method and then there is a lot of questioning. Did you really get those airflows when you built it like this? So sometimes they just want it in their traditional way to be able to know that it works.

Engineers mediate between the scientists’ ideas about what kind of instruments they need and their lack of knowledge about how to build them, but at the same time as the engineers know how to build the instruments, they are not always sure about how they work scientifically.\textsuperscript{177} It is the combination of the skills and work of scientists and technical staff that enables innovations.

In relation to scientists, technicians and engineers represent what Becker refers to as craftsmen (1978) or support personnel making up a pool for an organization (1982: 77). While scientists from different groups seek support from technical staff, the engineer or laboratory technician never becomes a complete group member within the boundaries of integrated experimentation and entrepreneurship. The relationship between scientists and technicians points at aspects of general sociological inquiries in terms of the formation of hierarchy and status and, more common in the social world’s literature, marginal positions (e.g. Hughes 1971b; Star 1991b). Technical staff is invisible both in the sense that they are not referred to in articles and in the sense that they are hidden in the laboratory or workshops. They are inferior due to their involvement in the “craft work” of science, but at the same time indispensable because the scientific goal is unreachable without them (cf. Latour 1999a: 191). However, from the point of view of scientific production per se, the distinction between scientists and technical personnel is less relevant (e.g. Latour & Woolgar 1979/1986) and the designation of staff as “scientific” and “technical” are no more than stabilizations of the view that there is a clear border between “doing science” and less important technical activities (cf. Latour 1999a: 191).

\textsuperscript{177} For instance, one scientist commented: "We’ll go there [to the workshop] and say we need this (…) But we don’t have the foggiest idea about how to make it work. (…) I cannot physically make the gear; I can have an idea about what I want things to look like.” To exemplify the other view, a technical engineer said the following about an instrument: “It is about that you can sort the particles in a particular way in that probe, I think… (…) I have built a few of those but I am still not completely sure that I can explain how they work.”
Instrument Expertise

Special features of the third type of research practice concern the focus on a specific instrument or technique and participation in many field campaigns. For example, one scientist noted: “There are many researchers who choose to have a specialty and who are asked to [participate] in many more types of experiments with their special instrument. (…) That’s another researcher profile.” The primary focus of instrument expertise is the performance and development of a measurement technique. The main function of measurement results is to suggest improvements that can improve the technique.

However, the analysis of a certain particle or compound can be included in this orientation. Thus, it is not the interest in, for example, aerosols per se that distinguishes instrument expertise from integrated experimentation and entrepreneurship (within the subworld of experimentation in meteorology and atmospheric science more generally), but that the two latter always put this in relation to and in the context of “what is happening in the atmosphere.”

As I noted in the general outline of these practices, the distinctions are analytical with the purpose of pointing out certain features of experimental atmospheric research and various aspects of the practice of scientists or groups can exemplify different ideal types of practices. However, it is notable that whereas entrepreneurship, integrated experimentation, and technical entrepreneurship seem to appear in pure forms among scientists or groups, instrument expertise tends to appear more mixed with integrated experimentalism. In other words, scientists or groups who concentrate on measurement technology for most of their time often have more or less interest in atmospheric processes too, and vice versa. To exemplify, a group investigates and explores trace gases in the atmosphere and their atmospheric effects, using “a fast in situ method called IMRMS (ion-molecule reaction mass spectrometry)”, which was presented by the leader of the group about 20 years ago (www.mpi-hd.mpg.de/mauersberger/arnold/aboutgroup.htm 15/10/2004). They still work on developing it and publish a small amount of their papers in a journal focused on this technique (International Journal of Mass Spectrometry). Since part of their work is devoted to the atmospheric effects of certain trace gases and another part to the development of the instrument they use in order to study this, they exemplify a mix of integrated experimentation and instrument expertise.

Presentations of research goals exemplify the different orientations. “The focus of our group is on-line monitoring of the diurnal variations of volatile organic compounds” (http://info.uibk.ac.at/c/c7/c722/umwelt/e-index.html 10/1/2004) illustrates the orientation of instrument expertise and technical entrepreneurship, whereas phrases stressing goals to “obtain a better understanding of the dynamical, physical and chemical processes” and “to understand the interactions” illustrate the concern in entrepreneurship and integrated experimentation (see www.misu.su.se 10/10/2004). Scientists note that the ones who work with instrument development tend to have a slightly different disciplinary background compared to atmospheric scientists. For instance, one scientist stated that it is often “physicists” and “chemists” who develop the instruments for atmospheric science. The quotation from another scientist indirectly points in a similar direction when commenting upon the difficulties in getting funding for instrument development and to apply for that, “At least [it is difficult] in our business, perhaps analytical chemists can do it.” However, since the point here is to map out practices within the experimental world of atmospheric research, and since this world exhibits diverse practices, it is those as such that are of specific interest.

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Enrolment of Instrument Expertise: Providing the Technology for Collaboration

As has been mentioned above, the role of instrument expertise involves focusing on measurement technology. “We are basically instrument developers. And then we got into the field because people invite us. Can you measure this? Can you measure that?” said one scientist, who illustrates this, as well as how invitations to campaigns are characteristic of this type of practice.

Researchers practicing instrument expertise – i.e. those using more advanced equipment – participate in more campaigns than the researchers involved in integrated experimentation and entrepreneurship. Another scientist suggested, “I think we are getting quite a bit of offers to participate in other people's projects, because we have very unique instruments that could be a valuable addition to other projects” (email correspondence). According to this scientist, his group is generally involved in two or three campaigns each year, but the year we met was exceptional. By the middle of the year, the group had already participated in four campaigns. However, they argued that it was important that the instrument was not outside of the laboratory for too long for the reason that they also wanted to carry out experiments in the laboratory in order to develop the instrument. This is in sharp contrast to scientists who are less oriented towards their instrument and who do not work with their measurement equipment other than to prepare them for, or to use them during campaigns.

These aspects allude to another difference between entrepreneurship and integrated experimentation on the one hand, and instrument expertise on the other, regarding the relationship between the activities of technical and scientific staff in the different practices. Groups or scientists with a strong focus on measurement technology bring technical staff to campaigns, not only for setting up technology but also for support during the actual measurement process, especially if there are problems with the instruments.

Moreover, disciplinary borders are contested and can therefore not be used as explanations for scientists’ types of practices proposed here. See also Weingart & Stehr (2000), who argue that discipline is related to the vanishing unity of science.

180 For example, at one campaign I visited, a member of a group with two advanced measurement systems that required constant monitoring and who had more participants than any other group, said it was sometimes difficult for the group to move between campaigns, implying that they participated in quite a few. This can be compared to groups participating in about one or two a year. Nonetheless, a scientist working in a group that participates in many campaigns stated, “I think there should be (and typically is) a balance for each group between initiation of own projects and being invited in others’ projects. So since there are typically more than two partners participating in a project you typically will participate in more projects of other groups than initiating own projects. Until now I only participated in other people's projects. Our group participates in other projects a lot at the moment, but there are also a few projects which are initiated by our own group. So there is almost a balance” (email correspondence).
This gives technical staff a more salient role and exemplifies the shifting border between the types of work that belong to the technical and the scientific realms respectively. 181

Returning to the question of enrolment, how and why do “entrepreneurs” contact “instrument experts”? First of all, they have to know about the researchers involved in instrument expertise. People in the freelance systems of art worlds get jobs on the basis of their reputation (Becker 1982: 86). Among experimentalists in atmospheric science, measuring instruments and equipment more generally constitutes one basis for reputation, rather than the scientists using them. For example, one scientist commented on the “reputation” of his group in the following way:

People know it’s this group in Münich who can do measurements so in the case of ABC, I met Linda during the EFG expedition and she knew about the instrument, about the technique and so they needed someone to do these measurements so Björn asked ‘would you like to participate?’

Thus, in a way, scientists with an exclusive or “special” instrument, for example different types of mass spectrometers, function as freelance “auxiliaries” for other scientists. Consequently, in instrument expertise it is important to have a (good) reputation and a large network of contacts. However, it also seems that instrument expertise depends less on (closer) personal relationships as compared to the researchers working in integrated experimentalism. 182

From the perspective of the organizer of the project (i.e. entrepreneurship), the main purpose of involving an instrument expert is to access valuable data. Instrument expertise provides the technology for collaboration, and because the technology this working practice includes is unconventional, these types of advanced instruments are less likely to be borrowed or rented. Concerning the recruitment of such an instrument to a project, one scientist commented that, “It’s a special expertise you bring in. Like John, who has an instrument that measures gases on a very high time resolution that is a world famous instrument. If we want it, I call them.”

181 Yet the difference in status is still noticeable, at least in relation to other groups. For instance, during one campaign, there was a big dinner party to which everyone was welcome. One of the participating groups included an engineer who had worked for the campaign for most of the time, yet he did not come to the party. According to his group members, he felt outside because he was an engineer (or at least they considered that was a reason for feeling outside). Consequently, technical staff may be better integrated in the group, compared to the position of technical staff in entrepreneurship/integrated experimentation, while remaining in the position of “technician” in relation to non-group members.

182 For instance, a scientist with special equipment said he “had to” become a member of a special network in order to present himself on a web page. This exemplifies one of the strategies to achieve a reputation. Although it is not important for instrument expertise as a practice in relation to the others in the typology, from the point of view of the person it is important if s/he is invited to projects or if s/he has to “advertise”.

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The special expertise, a measurement technology and a skilful scientist in combination is invited or asked to participate as a package. This quotation also raises the question about when coordinators actually contact colleagues to ask or invite them to participate. In this case, the invitation was given after funding had been secured and consequently after the “entrepreneur” (in this case the core group) had planned the project. This suggests that instrument expertise practice does not include participation in designing the campaign and therefore does not influence its focus. Why this is of less importance for instrument expertise compared to integrated experimentation becomes evident below.

Instrument Expertise as a Relationship between Scientist and Instrument

Part of instrument expertise relates to skills in terms of operating and repairing the instrument. Sometimes scientists point out that the users’ skill plays a major role in the operation of the instrument. The scientist who best knows the instrument is generally the one who has designed and built it (cf. Mallard 1998: 579; interview with NN). Instrument expertise differs from entrepreneurship and integrated experimentation in this respect, since it also includes participation in building measurement technology on the part of the scientist. To exemplify, one scientist told me, “We built this instrument so we understand everything. If we have a problem we fix everything ourselves and we are, you know, experts of the instrument and we especially tailor it for people to measure a certain molecule.”

Perhaps the most important reason to enroll a “package” is because of the interpretational skills involved, at least from the perspective of instrument expertise. However, this should be distinguished from previously discussed practices. While researchers in instrument expertise constitute experts in working with their measurement technology and interpret the data, their interpretation is more isolated from the atmosphere compared to practices in entrepreneurship and integrated experimentation. For example, they work with a sophisticated technique that can tell them something about, for example, the quantities of certain substances in the air. However, instrument expertise does not include expertise in interpreting the role of these substances in atmospheric processes, such as for example cloud formation. Interpretation in instrument expertise is not necessarily related to the context of the atmospheric processes of interest.

183 For example, a scientist who worked with the development of aerosol analytic instruments and who used an aerosol mass spectrometer in a campaign, commented upon the findings “There are no unexpected findings yet but I have to admit, that I’m not a cloud physics and chemistry specialist, maybe the findings will be unexpected to (…), my boss, who is a cloud specialist and who will be involved with the data when we write a paper” (email correspondence).
Instrument expertise involves participation in many campaigns, but it is less involved in the special problems and questions of each of them. The quotation below exemplifies this.

There are experts in this field who have done this for 20 years. I mean there’s no way you can come and say “now I’m going to be an expert”. You can provide the technology for collaboration, there’s not the potential to develop and focus on one field. My idea is that we (...) provide others with the expertise for the measurement technique.

This quotation illustrates both how researchers involved in instrument expertise participate in many different field campaigns by “providing the technology for collaboration”, the topic of the previous section. At the same time, it illustrates the point I made about their lack of knowledge in any specific field of application. This creates a difference between the science of instrument expertise and the science of entrepreneurship and integrated experimentation, which the following quote exemplifies.

I always have to distinguish between measuring something and the science of interpreting these results. (...) There are two things, the measurement technology or science or whatever you call it and the real, the applied science, what is happening in the field. But of course the technology is always the same. So that’s why we are working in so many fields. But then the applied thing, that’s always changing. (...) Science is totally different but the technique is the same.

The researchers who exemplify instrument expertise are invited to participate in campaigns as a package because of their skills in interpreting measurements in isolation, not in the context of being able to interpret, for example, cloud processes. Concerning the interpretation of the on-line aerosol mass spectrometers that were developed in the early 1990s, a scientist involved in their development stated that:

The data they produce are hard to interpret, it’s not a measurement in a typical way (you measure a quantity and get a number, describing that quantity) – what you get is more qualitative information about what kind of particles there are, not an information that the particles consist of this and that and that component in that ratio (email correspondence).

Thus, the scientists working with these types of instruments contribute with expertise in understanding specific measurements. A scientist who was working with a new technique commented on the possibility for other scientists to use his instrument: “Well, you can use it, but you wouldn’t understand what it measures. Or at least it would be very difficult.” One reason why there are special problems related to interpretation is due to disagreements or lack of standards about how to go about the interpretation
of data from new instrument techniques. In relation to this, I would like to briefly discuss the relation between measurement techniques connected to instrument expertise and conventional techniques.

Instrument expertise is a working practice based on new measurement techniques which scientists refer to as “additional tools” for atmospheric science, as opposed to conventional instruments used in entrepreneurship and integrated experimentation. These instruments sometimes serve as replacements for conventional techniques by being, for example, faster, more efficient, more precise, or they enable a completely new type of measurement of for instance a new or smaller compound. I will not differentiate between these types of novelties in this analysis. In comparing his instrument to older techniques, a scientist commented,

It’s a new instrument that is capable of doing really fast analysis of certain compounds. (...) The conventional way to analyze them is gas chromatography, which is a very time-consuming and work-consuming method. So with our instrument we can do measurements very quickly. You need this kind of instrument when you have a fast moving platform on an airplane or a helicopter or whatever.

Another scientist that I talked to during one campaign noted that chemical analyses and collecting samples were very time consuming procedures, and added that there is low time resolution on these measurements. However, the

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184 “Conventional” is a time-dependent label in the sense that every instrument has been “new” – if not also “additional” – at some point. According to Latour (1987 :91), what – partly – “makes a laboratory difficult to understand is not what is presently going on in it, but what has been going on in it and other labs.”

185 It takes time to integrate a new method or technique in a research field. One scientist said: “Now it is getting better but in the beginning people were very skeptical about the new technique, which is right, when it is totally new and you don’t know if it’s measuring right (...) Some people were really skeptical, and some people believed everything. So that was a little bit of a problem.” Worth adding in relation to this is that according to another scientist, active at a model oriented department, it seems like modelers believe everything or nothing in terms of measurements. It has also been noted elsewhere that to have published research results accepted as valid, it is good not to be too novel by using a technology none of the readers know about. This can be seen as an expression of the dynamics of the actor-network in which novel technologies have to be supported by many enrolled actors if they are to become established (Landström 1998: 90, see also Mallard 1998), at the same time as it is simply an expression of what is conventional and “acceptable as long as everyone used it.” (Lewis 1969, cf. Becker 1982: 56) Moreover, new techniques have to be carefully tested and developed and it is a rule rather than an exception that it takes a long time before other scientists in the field accept novelties. For example, one experimentalist complained that he has used a sampling technique for over 20 years and he and his collaborators have published calibrations and tested them, but there are still scientists who do not believe in the results of the measurements because of the technique. To publish calibrations of the new instrument in articles is an “obligatory point of passage” for the introduction of innovations and some journals are especially directed towards technology development. See Latour (1987) for an important contribution to the analysis of the development and “diffusion” of new technology from an actor-network perspective.
advantage with these and other older methods is that “you know what you are doing”, and what to get, which is not the case with new, non-standardized technology. For users, there are manuals explaining how to use the instrument. This scientist and other groups work with developing the manual, so to speak, by inventing new methods as they do so. On a different occasion, he added that, in the beginning, the instrument was not even a tool, but (only) a study object. Now it is possible to use it in order to study something else but at the same time, there are still people working with and focusing on the development of the instrument itself. In this sense, they continue to relate to the instrument as a research object. This is a central feature of instrument expertise. However, this scientist also suggests that scientists may approach the same measurement technology differently depending on the context. I return to this issue below.

**Technical Entrepreneurship**

Sometimes scientists decide to replicate, mass-produce, and sell their instruments to other scientists (cf. Mallard 1998; Latour 1987). Consequently, instruments that have previously only been used by the scientists who developed them become accessible for other scientists who have not participated in their development. This practice is related the role of technical entrepreneurship. The “boss” in the following quotation exemplifies this type of practice.

They sell also the instrument – people at the institute – my boss who developed it also has companies so they sell it to other people. In principle they have made it fully automated now and you can press a button and it works. It produces results. But to interpret, the difficult thing is to interpret the results and to do that you need a lot of expertise.

“The boss” and his group are not only developing the instrument for their own use, but also try to spread (sell) the instrument to others. Technical entrepreneurship involves organizing others around or through a measurement technique, thereby changing the social network around a certain measurement technology, including the manner of working with it.

This is different from the role of entrepreneurship in organizing projects. Whereas the aim of field projects is to “sell ideas”, the aim of technical entrepreneurial projects is to sell instruments, which concerns recruiting “allies” in technical entrepreneurship. The difference is not large: “Machines

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186 Due to the focus in my study on experimental work in meteorology and atmospheric science, the material for my analysis of technical entrepreneurship is limited. However, the type is as a logical necessity from the point of view of the structure of the practices outlined above.
which are now current have often featured in the past literature of another field. (…) The apparatus and craft skills present in one field thus embody the end results of debate or controversy in some other field” (Latour & Woolgar 1979/1986: 66).  

This leads to the important temporal aspect of technical entrepreneurship, especially in relation to the work of instrument experts. The measurement systems provided by technical entrepreneurship presuppose this work. When a certain type of measurement technology becomes commercialized, it is no longer an exclusive asset for instrument expertise. As the measurement instrument becomes more automatic, with more labor congealed in it (cf. Latour 1999a: 189) and more functions (“agency”) built into the instrument and thus withdrawn from the necessary skills of its users, the more likely it is that the researchers practicing integrated experimentation and entrepreneurship incorporate the instrument into their working practices. Thereby, they change the character of the instrument from “advanced” or “highly sophisticated” technology to “conventional”, or even “standard” technology at a later stage. This means that from the point of view of instrument development, the practices outlined in this chapter are intertwined in a temporal (network) structure.

It is through the working practice of technological entrepreneurship that other scientists can also buy and use the technology. In relation to this, it is interesting to note the difference in perspectives between scientists who have participated in developing a technique – “instrument experts” – and the scientists who have started to use it as a consequence of the commercialization – “integrated experimentalists” – enabled by technical entrepreneurial practice. At a stage in the history of a technology when it is

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187 Regarding the “fatal” task of distinguishing between science and technology (in this respect), Latour (1987: 131f.) writes that during the process of enrolling allies and controlling their behavior, two moments allow us to detect and remain closer to the difference. The first moment is when new and unexpected allies are recruited, most often visible in laboratories, science and technology literature, and in heated discussions. The second moment is when all the gathered resources are made to act as an unbreakable whole. This is more visible in engines and machines and pieces of hardware. According to Latour, this is the only distinction that can be drawn between sciences and technology, if we want to study scientists and engineers as they build alliances. From this point of view, the difference between selling ideas and selling technology results from a temporal difference in the level of stabilization. When we think we can represent nature adequately, the insights can be used in order to build technology. In that sense technology is just another more stabilized form of a given scientific fact (Latour & Woolgar 1979/1986; Latour 1987).

188 This means that the technology in question becomes more and more of a black box in Latour’s (1987) terminology.

189 From the point of view of an actor-network perspective, this also suggests that the difference between organized and organizing actors is a matter of time, and not an a priori distinction between actors, in the sense that a measuring instrument used in instrument expertise eventually becomes part of the conventional instrument repertoire of entrepreneurs through the work of technical entrepreneurship, in which the ”same” actor (researcher or measuring instrument) achieves a different status.
not yet completely conventional and not only an object for study anymore, it is an instrument for “critical” scientists in instrument expertise who continue to work on developing the new measurement technology. If it at the same time is also a part of integrated experimentation or entrepreneurship, the different views involved in instrument expertise and integrated experimentation or entrepreneurship (simple “use” of the “same” technology) become salient. For instance, during one of the two campaigns that I visited, a scientist commented to me that there was too much data produced and too few people actually working on really understanding the data or developing the software and the data processing methods. He stated that there “are too many users” and “too few trying to understand.”

Differences in using new measurement technology also become salient when instrument expertise and technical entrepreneurship on the one hand, and integrated experimentation and entrepreneurship on the other, meet. During one of the field campaigns I visited, a representative from a measurement technology company came to visit a group that was using a time-of-flight mass spectrometer manufactured by the same company. The group had worked closely with the company in order to develop the instrument and, according to the representative, the company collaborates with all the groups who have bought the instrument in question, which is relatively new (about ten years). The purpose of these collaborations is to further develop measurement technology. This form of “collaboration” differs from fieldwork collaborations – the focus of this chapter – in that the center of attention (for the collaboration as a whole) is a measurement instrument, not a research question about “nature”, as touched upon above.

Nevertheless, it is interesting to briefly reflect upon this network in relation to the question of differences between users and developers of instruments. One of the members of the group who used the mass spectrometer suggested that the company seemed “more interested in science” (of the instrument) than in selling products. Because the scientist wanted an instrument that could produce similar data over a longer period of time, he seemed almost worried that the representative was “too interested” in developing and improving the instrument. This illustrates a difference in relations to the instrument. On the one hand, the user of the instrument views the measurement technology as a technical construction, an “answering machine”. This requires identical and predictable performance (cf. Rheinberger 1997: 32). On the other hand, the company representative wishes to develop the instrument, with less consideration of its role as a “stable” answering machine. This suggests enabling it to answer new

\[190\] This contradicts the idea that (technological) entrepreneurs aim for the incorporation of their instruments as black boxes in as many settings as possible (cf. Latour 1987: 105). At the same time, this interest in development may also express an attempt to develop the current version of the instrument as much as possible in order to lead one step closer to its more stabilized position as a black box, i.e. as a conventional instrument.
questions, thus acting as a question-generating machine (cf. Rheinberger 1997: 32), and also expands the market for it.

Technical entrepreneurship concerns the manufacturing of measurement technology that is somewhere in between the use in instrument expertise and on the verge of becoming conventional or standard instruments. This is important because it creates a different relationship between the user of the instrument and the provider (representing technical entrepreneurship) than that between scientist and support (technical) personnel in the case of standardized technologies, where the latter have a lower status. Since the buyer and the “technical entrepreneur” both enroll each other, the relationship is mutual, but it is not symmetrical (cf. Landström 1998: 111). By recruiting another user, the credibility of the instrument increases and it is also possible to collaboratively develop it, as mentioned above, which decreases the cost of development in technical entrepreneurship.

Finally, it is important to note the different career patterns of the scientists involved in different practices. One scientist deplores the fact that in the academy, some skills are more rewarded than others. For example, being able to coordinate, interpret, and combine results (entrepreneurship) is rewarded to a greater extent than measuring and methodology (instrument expertise). According to him, it is the people in the first category who become professors. The others stay in lower positions or even leave the academic environment to work for industry because there is no place for them in the academic hierarchy. This situation constitutes an opening for practising technical entrepreneurship.

The Role of Measurement Instruments in Experimental Practices

The above analysis shows how social networks are intimately linked with technological artifacts, thus illuminating how these are integral parts of hybrid, socio-technical networks (cf. e.g. Callon 1987). However, while my interest concerns the role of objects in practice, it does not subscribe to the actor-network approach in terms of the question of their agency. Including artifacts into the analysis of the process of networking calls for a re-evaluation of the role of things in social relations, but not necessarily by equating them with human actors (cf. Sverrisson 1993: 27). Moreover, the way in which this chapter focuses on practice at a certain point in time, illustrates the dynamic of science in a different and complementary way than, for example, is the case with the actor-network methodology which focuses on (follows) specific actors in a network (cf. e.g. Latour 1987: 138, 160).
However, it is of crucial importance to distinguish the spread and development of instruments in this story from conventional diffusion paradigms. In classic statements (e.g. Rogers 1983), technologies are developed, reproduced, and diffused to new locations and their novelty – and the resulting entrepreneurship – is technical and/or spatial. Here, I pay more attention to the people connected to the object and how the interpretation of an object changes during this “diffusion” (cf. Latour 1987: 132f.). Thus, it is not only objects that diffuse among people, people are, as it were, attracted to objects as well.

During the early developmental phase of a measuring instrument, most of the small number of people involved work together. They are involved in research about the object. During later stages of development, there are more people at different locations that use the object for research, but also more people on the back stage (workshop), supporting the former. Later, the instrument has come to act as a whole and therefore has become a black box. There are always people accompanying black boxes, but they are not the same people as before (Latour 1987: 139). We can also ask, for whom the instrument is a black box. As the interpretation of the instrument changes, not only does the practice it is incorporated into also change, but so too does the role of the people involved in it (cf. Latour 1999a: 179). In this sense, the relation is symmetrical. From the point of view of (scientific) practice, we cannot separate whom from what; a person from what s/he works with, if we seek to understand the practices in organized social worlds and subworlds (cf. Knorr Cetina 2001: 187).

Knorr Cetina’s (2001) notion of objectual practice highlights the relation between the scientist, measurement technology, and the object of knowledge. An issue of special importance in this chapter is the role of measuring instruments in experimental research. In the following, I will discuss this more in relation to the idea of epistemic objects and technical artifacts (cf. Rheinberger 1997, Knorr Cetina 2001).

Measurement technology can be related to the notion of epistemic object (Knorr Cetina 1999, 2001), developed from Rheinberger’s (1997) conceptualization. According to Knorr Cetina, intermediate (i.e. technical) objects such as instruments are not epistemic objects. However, in instrument expertise as practice, the measurement technology is not an intermediary that is used to reach something else, but is rather the central object of research work in that it is inside this object that entities exist or processes occur (cf. Rheinberger 1997: 28). All experimentalists try to understand what is happening inside the measuring instrument, but it is only in the practice of instrument expertise that it is the principal purpose of

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191 This makes it evident why it is important for researchers as part of instrument expertise to emphasize the epistemic aspects of their measurement technology; since it is from it this that they achieve their own status as scientists.
research. I therefore suggest that the notion of epistemic object applies to the measurement technology in the practices of instrument expertise and, also to some extent, in technical entrepreneurship. In entrepreneurship and integrated experimentation however, the relation between scientists and instruments is an intermediate aspect of epistemic practice. In this case, it is some part or process of the atmosphere that plays the role of epistemic object. When measurement technology that was previously only accessible within instrument expertise practice becomes commercialized through technical entrepreneurship, sufficiently stabilized epistemic objects become parts of the technical repertoire of integrated experimentation and entrepreneurship (cf. Rheinberger 1997: 29). Thus, instruments are either epistemic objects or mere technical artifacts depending on the position they occupy in the experimental context.

The distinction between epistemic object and technical artifact suggests, in other words, that the relation between scientists and their measurement technology cannot be generalized as intermediate in every instance, as Knorr Cetina (2001) implies. This relation differs depending on the construction of epistemic objects and technical artifacts in a given context. It appears differently for different actors, as is made evident in the case of instrument expertise. From the viewpoint of entrepreneurship, scientist and instrument together play the role of a “package” as a means to answer questions about the atmosphere, but the scientist who focuses on the instrument experiences this relationship as more complex (cf. Knorr Cetina 2001). This emphasizes the need for the measurement technology to be both an answering machine, in instrument expertise in relation to entrepreneurship, and also question generating for the researcher in instrument expertise. In this way, measuring instruments function as boundary objects in-between the practices.

Concluding Remarks

By focusing on field campaigns, this chapter has provided a structured account of the organized experimental world and presents a typology of practice that can be used and developed in further studies. In the introduction, I outlined the logical structure of the typology. In order to understand the relation between the categories, it is also of importance to attend to its social structure, in which the entrepreneur – as a social role – has a central position, surrounded by the other roles.

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192 This shows how the division between epistemic objects and technical artifacts helps to assess the “game of innovation” (Rheinberger 1997: 31) by illustrating the temporal, evolving and changing character of elements in scientific work.
From an entrepreneurial point of view, not only do technical staffs appear as “support personnel” (cf. Becker 1982), but so do integrated experimentalists and instrument. Becker (1982: 91) writes,

> It is an exaggeration to say that everyone in an art world is support personnel for someone else, (...) but this points to something important: every function in an art world can be taken seriously as art, and everything that even the most accepted artist does can become support work for someone else.

This is analogous to the experimental world. However, the general difference between “scientific staff” (as support personnel) and “technical staff” is not so much related to production as it is to articulation and to what they are respectively accountable and responsible for (cf. Strauss 1987). The interesting difference concerns not just the activities they perform, but also the way relations are established and maintained, especially in terms of the role of material objects in these interactions, and how people relate to them.

Furthermore, every project (arc of work) has a temporal flow and according to Strauss (1987: 102f.), it therefore makes sense to think chronologically about the initial phases, which include persuading and negotiation with others. Table 2 summarizes some differences between integrated experimentalism and instrument expertise in this respect.

Table 2.

<table>
<thead>
<tr>
<th>Practice</th>
<th>Time of translation</th>
<th>Co-production of interest</th>
<th>Scientific commitment</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Integrated experimentation</strong></td>
<td>Early</td>
<td>Large</td>
<td>Close</td>
</tr>
<tr>
<td><strong>Instrument expertise</strong></td>
<td>Late</td>
<td>Small</td>
<td>Distant</td>
</tr>
</tbody>
</table>

Enrolment of integrated experimentation and instrument expertise in relation to entrepreneurship

It is important to refer to the formation of interest of integrated experimentation and instrument expertise in terms of co-production rather than, for example, as adaptation, because while it can be suggested that researchers involved in integrated experimentation have to adapt to the dominant approaches and interests in the field, it is also likely that their goals and interests are *mutually* shaped to fall in line with each other (cf. Latour 1994). Using the concept of translation adds to the analysis of articulation of an arc of work because it highlights this process and how it
differs in terms of how working practices are incorporated into the project. At the same time, Latour’s (1994) “model” of goal formation and translation lacks a temporal dimension in terms of when different practices or actors are enrolled and how this shapes how they are enrolled. Because of the different commitments of entrepreneurship and integrated experimentalism on the one hand, and instrument expertise on the other, the translation of goals at the time of enrolment are different, as well as the point in time at which new participants enter (are enrolled) in the project (or program of action, cf. Latour 1987). Whereas integrated experimentalism is incorporated into the entrepreneurial project at an early stage – during the process of planning and fundraising –, the association with instrument expertise is established at a later stage. This is explained by the fact that instrument expertise implies participation in a broader range of fieldwork activities, partly as a way to fund instrument development and characterization. Since the researchers that practice instrument expertise participate as pure “measurement guys”, as one scientist put it, their interest (or sub-program) does not have to be translated the same way as for the researchers in integrated experimentalism. These are more bound to one field because of their research commitments and conventional instruments.

While this suggests how concepts related to articulation work (line of work, arc of work, division of labor) and translation (enrolment, mobilization) is a fruitful combination, it is also important to briefly discuss the difference between interests (in the actor-network sense) and commitments (cf. Star 1991b: 50). If interests are not imputed to actors as background causes of action but lie in between actors and their goals (Latour 1987: 109) as temporarily stabilized outcomes of previous processes of enrolment (Callon & Law 1982), they can be conceived of as co-produced during translation into a research project. Although the focus on the atmosphere or the instrument as a more long term commitment is not up for negotiation in the same way as a particular interest is, the sometimes different commitments of participating scientists regarding the role of instruments does not seem to cause trouble since field-based collaboration enables scientists to adhere to their commitments, pursue their interests in relation to these as well as to join with others through translations. Consequently, fieldwork enables multiple agendas and multiple roles to exist side by side in part due to the role of measuring instruments as boundary objects in field collaborations.

The following chapters primarily attend to experimental practice concerned with the atmosphere, rather than with measuring instruments, unless clearly stated otherwise. The major part of experimental work I have studied at the department and elsewhere belongs to this category and most of the scientists in this category refer to themselves as meteorologists, in a wide sense, or are referred to as such by other experimentalists. To understand the dynamics of the experimental world, a more inclusive approach was
required. In analyzing the *relationship* between field experimentation and simulation modeling, it is more fruitful to emphasize certain selected practices, or segments, within this world.
The purpose of this chapter is twofold. Firstly, and most importantly, to describe the working practices involved in simulation modeling. This is an esoteric field and a thorough presentation facilitates the understanding of subsequent chapters. Secondly, to problematize the relation between science and technology involved in simulation modeling, more specifically in terms of the character of simulation models as theoretical models and computer programs.

The previous chapter focused on articulation work in its analysis of practices involved in experimental research. Experimental work was conceived of as a segment of meteorology, grounded in the common activity of measuring, using measurement systems or instruments as basic resources (cf. Clarke 1990) but with slightly different roles in various types of research practices. Due to the collaborative culture, both within and between groups, the chapter highlighted coordination, organization and mobilization of networks (articulation work). The focus of this chapter, however, is directed towards a more detailed presentation of the working practices of modeling as production (work) and places less focus on how these practices are connected.

Another difference is that my material on modeling practice relies, to a greater extent than was the case with the study of experimentalists, upon what modelers have told me about what they are doing. Accounts can be used as a source of information about, for example, certain events, but they can also be used to reveal the perspectives of those who produced them (cf. Hammersley & Atkinson 1995: 156). ”Political” or ideological accounts about research work are “distorted” accounts from a naturalist point of view because they erode the naturalist project of having people tell their stories faithfully (Gubrium & Holstein 1997: 216). However, they can serve as valuable resources suitable for a different type of analysis. In addition, the modelers’ stories contain different levels of reasoning. I primarily present relatively straightforward descriptions of modeling work and activities, but data is also analyzed as expressions of the perspectives involved in modeling, an important component of these practices. This discussion does not imply that experimentalists’ stories are free from ideology or perspective, but the analysis of experimental work presented in the previous
chapter, which relies more on participant observation, allows for a more naturalistic stance than the analysis of modeling work. Therefore it has been of great importance to apply a combination of naturalist and constructivist perspectives when analyzing the modelers’ accounts for the purpose of giving an overview of modeling work. The naturalistic stance is more evident in this chapter whereas the constructivist stance is explicit in the following ones.

The structure of the chapter is as follows. First, I distinguish between different ideal types of modeling practice in order to illustrate different activities and concerns. The typology aims to illuminate the activities involved in modeling work and a typology of practices is a fruitful way of structuring this account. Because of the analyses in forthcoming chapters, it is of importance to present modeling work in some detail and therefore I present one of the two primary activities involved in modeling work (model building) both in general and from the point of view of the modeling practices. Furthermore, I discuss the role of the simulation model in practice in relation to the notions of epistemic objects and technical artifacts (cf. Rheinberger 1997; Knorr Cetina 2001). The chapter concludes with a discussion of the results and previous (climate) modeler typologies.\footnote{Previous typologies are of interest as attempts to understand simulation modeling practice in general and climate modeling in particular. However, this chapter is not about climate modeling but about meteorological simulation modeling more generally.}

**A Typology of Modeling Working Practices**

This section presents a typology of modeling practices. This is done in order to highlight the different activities and concerns, and to attempt to capture essential features of modeling work. These practices, and accompanying roles, describe the relation of the modelers to meteorological modeling as an organized social subworld. The typology is displayed below (Table 3).

<table>
<thead>
<tr>
<th>Work with simulation model</th>
<th>Using</th>
<th>Building</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Primary commitment</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Representation</td>
<td>Exploratory use</td>
<td>Theoretical construction</td>
</tr>
<tr>
<td>Prediction</td>
<td>Applied use</td>
<td>Pragmatic construction</td>
</tr>
</tbody>
</table>

Practices in simulation modeling
The first dimension of the typology concerns what modelers are doing with simulation models. I distinguish between building models in terms of developing new model components and the implementation of these new components, and using models in order to make numerical simulations. Building refers to constructing and developing the description of processes in the model, including writing the code. I focus on this type of development since the conceptual core of models has remained intact for several decades, and development is mostly a matter of parameterizations. Using models involves running the programs to simulate atmospheric processes. These activities are interrelated and, logically speaking, use comes after construction. However, for the purpose of clarity, I will start by presenting model use.

Similar to the typology used in the previous chapter, modeling practices include a dimension regarding the primary commitment in relation to the central artifact, the “inscription device” of the subworld (cf. Latour & Woolgar 1979/1986). However, in this case it concerns whether the numerical model is used as a representation of the atmosphere in order to understand it, or if interest is directed towards applying the model in order to produce predictions and forecasts.

The weather service represents predictive modeling in its institutionalized form, but some modeling work at MISO shares similar features and aims more or less directly towards possible innovations in numerical weather prediction. For individual modelers, theoretical or practical concerns are closely related to this institutional mission (cf. Shackley 2001). Modelers at the weather service are employed to work with certain activities and the roles that these modelers play are therefore determined by their position. Their situation is thus less flexible compared to modelers in academia. However, the typology is about different (working) practices connected to simulation models. Even if these are partly related to where modelers work, the purpose is to present the activities and tasks involved in these, rather than the people who are involved in this work.

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194 Earlier work also distinguishes between the production, construction, or building of models and the use of existing models. For instance, Dowling (1999) discuss designers and users and Merz (1999) authors and users.

195 For example, one modeler said that 90% of model development lies in parameterizations.

196 In Winsberg’s (1999) terminology, this means that building involves work with models both as dynamical and computational models, whereas using includes work with models as computational models and models of phenomena, but related more or less to the dynamic model.

197 This resembles the division between instrumentalism and realism but differs from the philosophical perspective due to the focus on practical considerations and activities tied to these concerns. This should not be confused with the contested distinction between pure and applied science (Gibbons et al. 1994; Nowotny et al. 2001), at least this is not the point I am trying to make about the difference between predictive and representational modeling.
I take the program and the input data for this program. The ABC-model is a program, it’s a FORTRAN code, and it’s compiled to be able to run on this computer. I have a program that is governed by certain parameters and I have to define those parameters that will then decide how the model will run. If the domain is too big I will never finish because the computer is not too fast. So it’s a compromise about the domain. Then I have to decide about the number of points – the resolution. Do I want to have a look at the arctic boundary layer? There is not so much horizontal heterogeneity in the layer so the resolution can be rather coarse. In this case it was 50 km. Then I have to think about the vertical profile, and when the winter is cold and there is no sun, you have very cold air near the ice and you have strong temperature gradients in vertical direction, so things can change drastically within the lower 100 m. So I insert as many levels in my model as possible within the lower layer. Then I need my initial data, I need boundary data – and I take that from the European center server. The first set of data is actually the initial and it is in the same format as all the other data but at a later stage in time. I don’t use the data over my area, only what is in the boundary. I throw it out and just take the boundary data that will then help my model to sustain this balance with real, large-scale atmosphere on this resolution. This data is not really in a form that ABC can understand so I have to do some processes on extraction of data. Then I execute the program. Sometimes I wait an hour, sometimes a week, I can wait two months. And then the model produces output in the form of different files. Each file contains for example one atmospheric variable at different levels. For example I have the east west component of wind over my domain and on all the levels of the model. Just taking one component of the wind doesn’t say much about what happens. Then I either plot figures, like three or two-dimensional figures of certain variables to see what the variables look like. I can make movies, I can choose colors and contours, and compare with my earlier runs. Because after every simulation, I get an output and then in my next simulation, the same phase, I change for example the sea surface temperature by one degree and I run the model again and then see, what is the impact of this sea surface temperature on the atmosphere? Sometimes there are different developments and sometimes it is as I expect.

According to one modeler, there are two principal ways of using models for simulations of atmospheric processes. One can either try to simulate “reality” as it appears or will appear, or one can simulate “alternative realities”. In explorative use, modelers simulate both real and idealized...
cases. Simulation of real cases refers to simulations with the purpose of understanding (or predicting) observations. In this case the model is set up in order to facilitate this by corresponding to the conditions where the observations were gathered (or for which the forecast is supposed to apply to). Simulations of real cases play an important role in exploratory use. It can be a first step towards idealized simulations, discussed below, as well as part of a development project to improve models. Perhaps most crucial is the use of real-case-simulations to show how a certain simulation model is capable of reproducing observations, which is a condition for accepting the use of a particular simulation model or version of it. In this sense, simulation modeling resembles theoretical work (Dowling 1999). Model evaluations are presented in articles with the primary aim of comparing simulations to observations and also to explain or discuss the differences. Model evaluation or “validation” is thoroughly discussed in the next chapter. I now turn to a more detailed presentation of idealized simulations.

Idealized Simulations: The Simulation Model as a Numerical Laboratory

Idealized simulations presuppose real simulations in order to show that the simulation model is capable of reproducing certain conditions.

If you use a model and try to simulate what has been measured – the observations – if you do that well enough, you know that the model works approximately correctly, as in reality. (...) Then you can use the model for further studies, in idealized runs, for instance where you vary the background wind and those kinds of things. Then you know that the model physically can simulate what you want.

In idealized simulations, simulation models function as numerical laboratories. “[Chemists] can mix things and see what kind of stuff they get; it is a lab for them. It’s how I use models: like a laboratory where you test things,” said one modeler. In these numerical laboratories, modelers perform experiments in a controlled environment with flexible conditions.

In idealized simulations and exploratory use more generally, models are used in order to test ideas and to try to understand the atmosphere. The aim is to suggest new ideas and hypotheses about atmospheric processes,

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199 Modelers in different disciplines as well as scholars in science studies have compared computer simulation to the notion of a laboratory (e.g. Knorr Cetina 1992; Merz 2004). Numerical laboratories enable controlled experiments of processes that cannot be tested in the traditional laboratory. However, they share the feature of being able to deal with process unrelated to where, when, and how they occur outside the laboratory (cf. Knorr Cetina 1992; see also Merz 2004). In this chapter, focus remains on modeling work and its activities (as any type of practice rather than specifically scientific). Other features will be attended to in following chapters.
including what affects them. In order to understand simulated atmospheric processes, one has to understand the simulation model (cf. Dowling 1999). This is imperative in explorative use. Partly because of the time it takes to get to know how a specific simulation model works in detail, modelers often stick to one or two simulation models. 200

Since the purpose of exploratory use is to understand, propose new hypotheses, or suggest developments, the model has to be sufficiently complex to enable the simulations one wishes to make and to answer the questions that have been posed. However, they also need to be simple enough to enable controlled “experiments”. Using a complex simulation model in order to take as many processes as possible into account is not, as the following quote suggests, an end in itself.

With the purpose to understand, you don’t need a more complex model than what you need to solve the problem. (...) It is a lot about understanding things in simpler terms. Reality is so complex, so many things happen. It’s about peeling off those things you don’t think are very important. (...) Make a model as simple as possible to solve your problem. 201

This also implies that exploratory use involves little interest in parameterizations for the sake of including as many (effects of) processes as possible. The conceptual, dynamic part of the simulation model, based on the laws of physics, is primary. Parameterizations are considered as more arbitrary or, as one modeler said, “archaic”, perhaps since they are “messy” (cf. Latour 1995) and far removed from the general aim for simplicity and elegance in the physical sciences (cf. Edwards 1999).

In idealized model studies, modelers perform simulations with various (virtual) physical settings in order to answer questions about what is important in certain processes. Sensitivity simulation is a type of idealized simulation where modelers explore potential weaknesses of the simulation model and identify sensitive parameters by performing perturbation simulations where certain processes are omitted or substances are included. Perhaps simulations of the impact of anthropogenic emissions of CO₂ are the most widely known process tested with this type of simulations. Sensitivity simulations are not always realistic but this is not considered as a problem since the aim is to understand mechanisms rather than to predict and

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200 In practice, the choice of simulation model is restricted for several reasons. The “choice” of model depends on who you work with, especially as a doctoral student, but is also a matter of institutional affiliation. Modeling work at the weather service or at climate centers generally focus on one particular model, for instance HIRLAM at SMHI. Some codes are available for everyone over the web, whereas others are available by contacting the code developers. The computer power requirements of some models also restrict the possibility to use them everywhere. Consequently, it is questionable how much freedom a modeler has in choosing a model for “pure” research purposes.

reproduce a real course of events. This also means that very little data is used during initialization, i.e. the start-up of a simulation when the computer model uses some input-data in order to start calculations. For example, one modeler told me the following about one of her idealized simulation projects: “I’ve used idealized data, a vertical profile of temperature and wind and everything and then started the model from that. I’ve just been interested in how [this factor] affects and then you want as simple data as possible.” The comments made by another modeler further illustrate this limited use of data.

The problem with the type of models I use as numerical laboratories is that you can only specify a vertical profile of everything, in a domain of perhaps 100 km, or larger, a thousand km. The model should produce all the variability that exists in reality in this box. The level of detail to start a model is often so low that you can’t really say that you use measurement data to initialize the model but you use measurement data as guidance together with background information.

Thus, while some type of data is always required to set initial conditions for the model simulation, the model can use the generated output to continue calculations, and in exploratory use, initialization with observational data is of less importance. However, the lack of detail in the model set-up is not considered as a major problem since exploratory use is not essentially about processing observations. At the same time, the initial data that is made use of is crucial precisely because of the lack of detail since this particular data has to be representative for a larger region. This is also implied in the quote above, stating that only one vertical profile can be specified.

As was noted above, familiarity with a simulation model facilitates the interpretation of simulations and model output, which takes a lot of time in explorative use. However, just like in experimental practice, this is considered to be the most important part of the work. About this activity, one modeler noted,

It’s actually when the model [run] ends that the interesting work begins. Then I have data and then I have to figure out what happened in the model that produced what we look at. Then again we have to decide is it a wrong feature, is it a problem in the model or is it something with the way the model works or is it something that is happening in the atmosphere? Sometimes you look into other models, other data sets and try to find similar features.

The activity described in the quotation above illustrates how certain aspects of modeling work also in fact resemble experimental work (cf. Dowling

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202 Another reason for this, at least with regard to general circulation modeling, is that this type of modeling aims to reproduce the average circulation for specified conditions (see e.g. Nebeker 1995: 156; Holton 1992: 361).
1999), in which experimentalists are preoccupied with scrutinizing experimental set-ups to uncover possible sources of artifacts and use the work of others in order to do so. There is also a resemblance regarding the exploration of the development and behavior of the simulated processes. Through the function of models as inscription-devices, modelers study and interpret model output by looking at tables, graphs, and diagrams and by watching “movies” enabled by graphic interfaces (cf. Dowling 1999: 269, see also Hacking 1983: 167).

I have this whole three-dimensional world in the computer. Then you sit and look at images and twist and turn and try to understand. (...) How does the wind blow? How does that depend on temperature? Then you try to find a couple of figures that are in some ways representative of what is happening and try to paint an image of what is happening through the figures.  

In addition to the way in which it illustrates the work of interpreting model output, the quotation also raises the issue concerning the presentation of the result of this interpretation. In journal articles, idealized simulations are presented in a more narrative form, complemented by figures that serve to illustrate the points made in the text. In these studies, the simulation models have replaced the study of the atmosphere with the study of the modeled atmosphere. Although they try to connect the modeled atmosphere with the real atmosphere through observations, it is basically the features of the modeled atmosphere that they strive to understand and then translate to (knowledge about) the real atmosphere. In contrast to experimental work, there are often only one or two authors of articles that present the results of explorative use.  

Explorative use is also something that modelers tend to  

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203 A similar quotation that places more emphasis on the technology involved in interpretation is the following: “When you are looking at this, you can get an idea of what is happening because it’s a huge amount of data, and first you need to simplify it. And this is a very nice [software] package to simplify. It actually saves a lot of work for me. It shows the atmosphere. Then when I find something, an interesting feature, I say whoops and I want to have a look in more detail. I want to have a detailed look at what is happening in this spot. And then I go to other software and put in only a few data sets that are files with variables that I want to have a look at and then I see”.

204 Modelers seem to see their work as more individual and less collaborative in comparison to experimental research. To exemplify, according to one modeler, “It is more common for the people working with measurements that there are larger groups. I think in dynamic(s) [meteorology] it is more often found that people are independent.” This is also reflected in the number of authors and joint production of papers, which is much lower than in experimental work and evident simply by looking at the publications from the different sections at MISU (see e.g. IMI Biennial Report 2001-2002). However, from the viewpoint of the simulation model, modeling clearly involves collaborative work in the sense that several modelers contribute to the model in different ways, but most often at different times. Thus, if modeling collaboration would have been a focus, the collaboratory concept, defined as a new organizational structure for scientific activity that accounts for computer-mediated collaborations (Lederberg & Uncapher 1989; cf. Bowker et al. 1997: xvi), would perhaps had been a good point of departure.
work with alone or at most with one colleague who may or may not be in the same formal position but always part of the scientific staff.

Weather Forecasting as Applied Use

Applied use of models aims to produce forecasts and in sharp contrast with explorative use, it is the result of the work of an ensemble of people, including professional meteorologists, programmers and modeling researchers. Due to my focus on meteorological research, this practice is therefore less central compared to the others, but still important for understanding the subworld as a whole.

Meteorological simulation models are used both for pure research purposes as well as for routine numerical weather forecasting at the weather service. Generally, research models are used for research and operational models for regular forecasting. However, some models are used both in research at academic departments – for instance in order to improve understanding of a certain process and to generate hypotheses – and as weather prediction models – in order to produce regular forecasts for the public and special customers like the Navy, airports or the power industry. While the two types of models are conceptually similar, their numerical coding is often different in the sense that simulation models for operational use are coded so as to be as frugal as possible in terms of computer power. To predict actual weather, more process descriptions are also included. Applied use thus involves more complex models. However, just like the difference between climate models and numerical weather prediction models, the difference between research models and operational models for forecasting is not clear-cut.\textsuperscript{205}

In applied use, the inner logic of a simulation model is secondary to its predictive performance, which aims to fulfill the everyday expectations of the public, shipping, aviation etc. concerning the reliability of results. One modeler commented on how models have to be constructed in relation to the demands of forecasting: “You can’t be on TV and say ‘tonight there’s no forecast because the model couldn’t handle this’ ha ha! It doesn’t work. It has to work for every type of weather, on everything it has to give an

\textsuperscript{205} At MISU, both operational models and research models are used. Predictive modeling is based on the use of forecast models whereas exploratory use tends to start from research models. Part of the reason why the choice of model differs between exploratory and applied use arises from the possibility of being able to choose any model for the purpose in representative modeling, while applied modeling is often tied to a certain model, since it often takes place at the weather service. It is important to take into consideration that MISU and SMHI represent two different types of institutions – a fragmented university department and a relatively homogeneous authority, with a research center under its roof. This has many consequences for the work that are beyond the scope of this thesis due to the focus on the working practices of social worlds rather than the institutional structures related to this.
answer.” Whereas research models may have a more restricted use within the range of their application, the general use of a forecast model is a sign of its superiority from the perspective of predictability, as is exemplified below.

With a research model you (...) can do what you are interested in studying and you can understand what is going on and show something, understand something and publish results. But it doesn’t mean, in no way that that model could perform better than that forecast model that gives rain this morning. So I think (...) any really good model is tested as a forecast model.

The quotation illustrates how representative and predictive modeling involves different views on what a good simulation model is. From an applied viewpoint, the more accurate the weather prediction (compared to observations), then the better the simulation model is, largely independent of the physical basis of the model it seems.

Improving the use of observations is held as an important aspect of creating better forecasts, and in reducing data error, which is one of principle types of modeling errors. The other is the model error, which I return to below. Data error makes it is impossible to start simulations on the basis of perfect initial data conditions.206 This is important for studying real cases and especially for weather forecasting. Whereas the global observation system, and to a lesser extent, experimental meteorologists, provide the data, the modeling side of this problem is to develop the implementation of data. Data assimilation is a branch of meteorological research that strives to improve the use of observational data in model simulations, thereby minimizing the data error. This type of development work is of most importance in the field of numerical weather prediction.

**Assimilating Data**

Initialization procedures are especially important in operational weather prediction since a precise forecast is the goal of this activity. Researchers working with data assimilation try to solve the problems with the initial conditions by trying to develop the ways the model makes use of available observations. Nowadays, data from the observational network system of satellites, radio probes, weather stations, and aircraft are used through data assimilation. According to modelers who work with developing data assimilation, it is a “synthesis” of modeling and measurements. One modeler described the idea behind data assimilation in the following way:

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206 So called ensemble simulations are where several simulations with slightly different initial conditions can be used to see how different weather patterns and atmospheric patterns develop in relation to this. These types of simulations are not discussed here. The chaotic development of weather and the related quest for precise initial conditions are briefly discussed in Chapter Four.
Data assimilation combines observations and dynamics, or theory if you want or modeling, to really make a compromise between these two types of information. Because what one often forgets is that when one measures and then compares some model with the measurements is that the measurements are not the truth. The measurements also contain errors. (...) We take observations but we always assign them errors. Then we take the model, or the description of the atmosphere, and then we assign error to this piece of information. Then we try combining these two pieces of information in order to estimate what the atmosphere is like right now.

This illustrates the pragmatic “synthesis” of observations in data assimilation, but it should not be neglected that this is directed by the concerns of (predictive) modeling. While data assimilation represents the most direct use of observations in simulation modeling, observation data cannot be “fed” into the computer for use in computer simulations without consideration. A modeler explained, “We always have to start from something and that is what we call first guess or a priori information and that is always the model, or the previous model forecast”. If the first guess would not be considered, observations “would throw the model out of its own balance”, as another modeler expressed it. He continued, “There are lots of small structures and if it happens that your observations are located in one of these small structures it is not representative for the area that the model thinks it is representative for.” This is why observations that are too far removed from the first guess of the model are not used in simulations. In the words of the same modeler, “You assume that the first guess, that is typically our forecast, is very close to reality. You only correct this first guess. If observations show something completely different, they are taken out.”

Data assimilation thereby exemplifies how observations are incorporated and translated into the model framework rather than simply used for calculation. Simulation models are therefore much more complex tools than simple “number-crunching techniques” (cf. Winsberg 1999:275; 2001), but also for other reasons.

Data assimilation is a growing branch in numerical weather prediction and many believe that developments in this area could improve forecasts. However, development also depends on computer power, and data assimilation is very expensive computationally. Notable in this regard is that a scientist with a lot of experience of data assimilation suggested that the status of assimilation has increased over the years. From its erstwhile position of being considered as something technical that students were recruited to do – just spreading the observations out over the grid volume, as

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207 Data from experimental work is too scarce for this use and one modeler said, “such data [measurement data] is set up for different processes than those that define large scale atmosphere and for example global models that assimilate different data measurements, their treatment of for example the Arctic boundary layer, their scheme of the energy between ice surface and atmosphere is much simplified so those situations would not fit in this scheme.”
she put it – data assimilation has recently achieved scientific status and moved into the university research department. This can be compared to the distinction that Becker (1978) draws between art and craft. Activities organized as craft can become art, but conversely, activities that are organized as art can also become craft. In this case, data assimilation has changed from “craft” to “art” because of the increasingly sophisticated estimation of errors and the procedures it employs to make as good a use of data as possible. This also implies that the concern of the weather service, where the most pure form of applied modeling takes place, affects meteorological research activities at university departments. The weather service does not only make use of what academic researchers produce, but its expectations also seem to influence meteorological research and illustrates the close connection between meteorological research and weather prediction.

Constructing Models: Producing Parameterizations

Most modelers, especially in academia, concentrate on avoiding and reducing the model error – the second error I mentioned above. The model error arises because the model does not describe processes “exactly” as they occur in the real atmosphere. To develop new components is one way of working explicitly with this error and it involves several different steps, partly dependent on the point of departure for the development and its aim.

Parameterizations are simplified process descriptions adapted for simulation models. Parameterizations can be developed from observations but modelers normally start with physical theory from which they derive equations. This is consistent with the ideas about how things should be done in the subworld of modelers and I concentrate on this way of developing parameterizations in this chapter. Nonetheless, formulating the mathematical and theoretical expression is only a part of developing a new parameterization. In this section, I will briefly review the process of constructing a parameterization as there are many similarities between theoretical and pragmatic model construction/building.

The Process of Producing a Parameterization

In order to develop and construct a parameterization, modelers start by trying to understand the process and formulate a set of equations based on theories and the literature about that process. Consequently, developing

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208 Experimentalists who develop parameterizations generally start from observations. Consequently, they also participate in the development of models, although they do not work as much with the implementation. See more on this in Chapter Eight.
parameterizations involves spending time on solving and articulating equations. In order to determine coefficients and constants of the equations that make up parameterizations, modelers use observations. A modeler described this activity as follows:

When you have decided what type of parameterization you want to use, what degree of complexity it should have and so on, then by using observational data, you can probably determine the values of some of those terms that you got from calculating (...) In each of those parameterizations there are a lot of constants and you have to get these from observations and plot. Then you see that this figure has to be three because you see that on the basis of the [plotted] curve.

After formulating the mathematical expression, modelers translate equations into a numerical code. The materialized form of a numerical model is a computer program. Thus, implementing a parameterization into a simulation model involves writing it in the form of a computer code in the computer language FORTRAN. In experimental work, data processing and quality assurance are low-status activities compared to analysis and interpretation of data. In modeling, programming has a similar status, it represents the craftwork, it is learnt by doing, and it is quite time-consuming. Modelers sometimes start from an existing code that has been written by themselves or by others. If a modeler wishes to use the same formulation of a model or parameterization as someone else has done before, s/he can ask the author for the piece of code, which is open for everyone to use after it has been published. Thus, accumulation and collaboration are important aspects of modeling practice in spite of appearances to the contrary.

After initial development or implementation in smaller, simpler simulation models, construction generally involves the implementation of parameterizations in larger, more complex models. This is a necessary step in the development of a parameterization since the performance of a parameterization cannot be valued in isolation, in part because the point is to take the effect of a process into account, in part because a parameterization that does not “work” is useless. However, several features of simulation models make implementation a complex task, not least because all components are interrelated. In the words of one modeler, “You cannot change anything anywhere in the model without affecting everything else in all places at all points in time.” This means that implementing a parameterization does not only change the representation of the isolated process, but the whole model.

An additional problem in constructing models is related to the interaction between different elements of the model. To change something in the

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209 A more untraditional method is to extract parameters or constants from model simulations. I will not discuss this further here because of a lack of information about this approach.
conceptual formulation in one parameterization often impinges on what happens in another parameterization. Thus, in development work, modelers have to work with the numerical code and the equations simultaneously in order to deal with unexpected and unwanted consequences.

When you start changing inside the parameterization, you discover quite fast that it is not independent of the rest of the model. But if I change something it will have implications on other things too and then I probably have to go in and change other coefficients at other places for the purpose of the whole. The same thing applies if you only go into the numerical code, you can bet on there being something in the parameterization that explodes. So you have to consider what feedbacks it can have had. Then I must change the parameterization to make everything work again.

Thus, it is difficult to predict the effects of changes in terms of unintended feedbacks in the model. If the modeler is familiar with the model and knows how it works, it facilitates prediction of changes and feedback but the more complex the model, the more difficult it is to predict possible problems. Since it takes time to learn how a model works, modelers prefer to use simulation models they have worked with before, as noted above and exemplified by the following quote.

If you have a whole model, like DEF, (...) you are not familiar with all the parts yet. It is one of the reasons why I stay with the other model because there I know exactly what I am doing ha ha! In ABC, there are more parameterizations going on. Well I actually don’t know all of them.

In sum, implementing a new parameterization involves a lot of work that much more resembles hands-on work, as opposed to conceptual work, in order to make the simulation model work (again) and produce acceptable results (cf. Schild 1996; Star 1995b). One modeler talked about the development of models and added, “It’s about making the model produce data that is okay. That’s what modeling is, to set it out. Is it a reasonable description of what we see? Or if it’s forecasts, that it is a reasonable forecast.” But what is a reasonable description or forecast depends on the purpose of modeling, as I will discuss more below.

Building Parameterizations on the Basis of Theory – Theoretical Construction

Theoretical construction puts most emphasis on the theoretical, conceptual model and equations, thereby involving deduction by hand, and sometimes
collaboration with scientists working only with developing theories or analytical models (as opposed to simulation models).  

The representation of physics in the dynamic model is most important. However, the representation of physics can be regarded from (at least) two different perspectives: how well certain processes are represented in the model (compared to observations), or how well the simulation as a whole corresponds to what is considered to be real conditions (observations). In theoretical construction, the focus remains on the parameterization.

This can be illustrated by passing one comment on a meeting that was convened by several modelers about a climate assessment report. During the discussion concerning the importance of the boundary layer description in regional and global climate models, a simulation modeler working with constructions of boundary layer processes at a university department argued that the boundary layer processes are important and have to be improved. Another modeler agreed that it was indeed an important process, but emphasized that it was (only) one of them. This implies that processes are part of a whole and a possible interpretation is also that this whole is more important.

When parameterizations based on novel conceptual models or theories are presented in journal articles after preliminary simulations and/or implementation in larger models, articles center on the conceptual basis of the parameterization and the result of the simulation, without mentioning coding. The code is considered as something secondary in relation to the “model”. This is illustrated by the following quotation:

> The model is more about how you connect different theories to make a consistent description. The next step is how you code in the computer. (…) You can have the same model but completely different ways to code. (…) What you publish in the journal isn’t the code. It’s the equations. How you program the computer is a practical thing that everybody can do in his or her own way.

In addition, the quotation suggests that a “model” can be separated from the code, thus pointing at two forms of the simulation model (cf. Winsberg 1999). An interesting question in relation to this is what defines the “identity” of a simulation model from the modelers’ viewpoint (cf. Merz 1999). If the different conceptual models making up the theoretical formulation of the simulation model are primary, it suggests that the identity of a model is tied to its conceptual formulation rather than its computer code.

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210 Theoretical practice involves the development of new parameterizations by using physical theory, mathematics, a pen and paper, without using computing to figure out how atmospheric processes can be described. Taking the different forms of a model into consideration, theoreticians translate the mechanical model to the dynamical model, whereas numerical modelers translate dynamical models to approximations and finally computer code (cf. Winsberg 1999).
Nevertheless, the same modeler commented that there are quite a few different climate models with various names but asserted that they are very similar (conceptually). According to him, every center wants to have a simulation model of its own. He added, “It’s enough with a piece of code to give the model an own identity.” On the one hand, the identity of a simulation model seems to build upon its conceptual structure and components. On the other hand, a piece of code is all that is required to differentiate a simulation model from others. However, programming was mentioned in passing as something self-evident but less important. Accordingly, in cases where several modelers were participating in constructing a new parameterization, it is the modeler with the lowest rank – typically a doctoral student – who programs. To summarize, in spite of the epistemic *discourse* surrounding models, embracing the view of simulation models as theoretical, mathematical models, the fact that simulation models are also computer programs including pieces of code cannot be neglected in understanding the practice of simulation modeling. Not least considering the *practical* implementation problems, which illustrate that a consideration of numerical coding is crucial to the task of making the model “work”.

Furthermore, the strictness in terms of the physics of parameterizations in theoretical construction also has consequences for programming and numerical qualities. For example, it is not an explicit aim to reduce the time it takes to simulate by simplifying the equations at the expense of physical descriptions. Instead of adapting to existing resources, modelers who work with theoretical construction partly neglect existing computer resources, as one modeler who works with construction at MISU, suggested, “Sometime in the future they will be able to have 20 km resolution on their [climate] models. (...) So you have to prepare for that.”

However, there are other aspects of existing models that are taken into account. Although FORTRAN language is no longer one of the most efficient, this is the language used by existing numerical weather prediction models. Due to the difficulties in changing models, all new components are also programmed in FORTRAN, since “possibly the ultimate hope of every atmospheric modeler is to make a contribution to an operational NWP model” (Zagar 2004:38). This statement is supported by many comments from modelers. For example, about the development of a new parameterization based on a new model – consequently exemplifying theoretical construction – one modeler said: “Even if it is a very good [parametric] model, it takes perhaps five, ten years before it is implemented in a weather forecast model. So it takes a very long time before you see the result.” This suggests that there is a strong connection between meteorological research and operational weather forecasting and also how modeling developments is a collective practice.

Although there is a general assumption that better physical descriptions (through better understanding) are the key to better forecasts, this does not
mean that parameterizations that are held as more physically correct necessarily improve models. In relation to the implementation of new parameterization developed according to the principles of theoretical construction, one modeler argued that: “If we would put in the type of model that is very physically correct in [this weather forecast model], it would go to hell.” However, some modelers accept temporary degeneration of results in order to prepare for improved physics in the longer run, but this is less evident in pragmatic construction.

Pragmatic Construction of Parameterizations

Pragmatic construction tends to build on the work of theoretical construction. For example, according to one of the modelers at the research department of the weather service, people like him apply scientific knowledge in order to make use of and develop tools and techniques that are adjusted to the current stage of technological development. This is opposed to basic science and the production of new knowledge, which he said takes place at university departments. This also has consequence for the constellation of people involved in pragmatic modeling construction, in which a programmer accompanies one or several persons focused on simulation modeling as such. It is also an additional example of how modeling development requires collaborative efforts.

Pragmatic construction is directed towards implementation in an operational model – this is a “natural” consequence if the modeler works for a research center or the weather service. It is therefore adapted to the existing stage of development and directed towards developing a parameterization for and changing a particular (operational) model. Models used for operational purposes tend to be developed as packages. Through a process of mutual adjustments, parameterizations, and changes of resolution and input, variables are co-generated and interconnected. Operational models also sometimes need practical solutions rather than theoretically correct formulations, in order to make the model work and produce reasonable results. The difficulties in implementing new parameterizations arise partly from how existing models have been adjusted (see also Edwards 211).

211 In order to make the simulations more computer efficient, programmers function as support personnel for the modelers at the weather service and at large modeling centers (cf. Becker 1982). Translating equations into numerical code is a task for modelers, who produce the basic code. However, this code is sometimes only preliminary since programmers continue where the modeler leave off in order to make the simulation model use computer power more efficiently, while not changing the operations that the code executes.

212 Physical parameterizations should be distinguished from non-physical adjustments, which are for example added when the consistent application of equations leads to unrealistic developments in terms of e.g. extreme temperatures or sharp contrasts. For more on the use of so-called flux adjustments for balancing the problems caused by coupling models of the atmosphere and oceans, see Shackley et al. (1998).
2001). For instance, a modeler said, “It is well-known that there is something wrong with the latent flow of heat in almost every weather forecast model (...) and if you ask people who are developing models they say that of course it can be fixed but then we don’t get the right precipitation.” Consequently, changing just one part in a large model often makes the results of simulations worse, in the sense that agreement with observations decreases. In the words of the same modeler, “In that way, errors in separate processes can be hidden by compensating errors in these big complex model systems, and often if you import an improvement in one process description in one of these model systems, the final result becomes worse, because you took away a compensating error.” Thus, changing each part in a model is a huge task and the consequence of this is that the process of developing operational weather forecast models is more akin to patching than to renovating. This is a common problem for all larger models, irrespective of whether they are used for weather or climate prediction (see also e.g. Shackley 2001).

So-called tuning refers to the tendency to adjust coefficients or reconstruct equations in order to achieve a better result. It seems to be a widely used practice and almost a necessity in predictive modeling. It is, however, controversial and criticized, especially by modelers who are involved in representative modeling. In the following quotation from an article entitled “Measurements, Models and Hypotheses in the Atmospheric Sciences”, the authors state:

> The problem with tuning is that it artificially prevents a model from producing a bad result. (...) It might be argued that tuning is justified in the service of numerical weather prediction, because a good forecast is an end in itself, regardless of how it has been obtained. The trouble with this ‘end justifies the means’ argument is that in the long run better scientific understanding is the key to making better forecasts. (Randall & Wielicki 1997: 404)

Although modelers *in principle* reject the idea that it is acceptable to produce better results for the “wrong” reasons, this may be accepted in predictive modeling for the purpose of producing a better prediction.213 Thus, tuning creates a boundary between representative and predictive modeling. At the same time, it is not always clear what counts as tuning or not.

One modeler expressed the applied view on development when stating that a new implementation is not an “improvement” unless the result of model simulations becomes better, i.e. produces a better agreement with observations. According to him, if it does not, it should not be implemented.

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213 Although modelers generally seem to accept tuning in weather forecasting, a modeler concludes that: “In climate models, you cannot afford to have any systematic errors in the system.” This attitude coincides with the approach of what Shackley (2001) refers to as the climate model constructor, as opposed to the climate model seer, who has a more pragmatic approach.
The following quotation further exemplifies this perspective on development in predictive modeling.

As long as you don’t test your small development or small theory in the forecast, it is nice, but I think that for every theory in meteorology, starting from observations or just equations, a real test is a NWP model. If you can make something work, so that it improves forecasts, then you really achieve something.

This illustrates the commitment to prediction in pragmatic construction and predictive modeling more generally. Above, I noted that the representation of physics can be seen from two perspectives: how well certain processes are represented in the model (compared to observations) or how well the simulation as a whole corresponds to what is considered to be real conditions (observations). In pragmatic construction, the parameterization is viewed from the perspective of the whole, although construction work concerns a specific process. One modeler asserted: “It is not possible to say that every part of the model should be a representation of reality as accurately as possible, they have to work together.”

Furthermore, at the weather service, there are strict deadlines. A weather prediction model in operational use has to deliver forecasts on time, which is part of the reason why an efficient code is necessary. Developments have to be practically oriented and must not out-pace existing computer resources. Hence, numerical models used for operational purposes tend to contain more empirical parameterizations with more constants, less equations and more practical solutions in general, as compared to numerical models primarily designed for research purposes. From this perspective, increased complexity (better physics) and higher resolution are not necessarily positive (cf. Shackley et al. 1999: 432). It is important to consider computational expenses in numerical weather prediction and it is important that parameterizations are cost effective. However, this quality is also mentioned as valuable for parameterizations more generally. According to a researcher at the weather service, modelers working with research models easily exceed available capacity by adding more computations. Thus, he thought that balance between supply and demand in terms of computer power is impossible. Moreover, taking computer power into account is also important in climate modeling because of the long time it takes to run the simulation. Consequently, the identification of limitations in modeling is related to the resources that modelers work with and the aims of their activities.214

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214 According to Shackley et al. (1999), from the 1960s or 1970s there has been a belief at large modeling centers that increased computer power would lead to better GCM’s, especially for NWP, but also for climate change. Computer power has developed, but this does not seem to solve all of the problems (Shackley et al. 1999: 431).
Finally, I would like to comment on exploratory use in relation to pragmatic construction. The aim of producing good results for all conditions may involve compromises in relation to the physical processes that are represented in the model. Because of this, the “best” forecast models are not necessarily the best from the point of view of exploration, where transparent representation/simulation is essential.\textsuperscript{215} From an exploratory viewpoint, different models can be good at different things, and operational models can even be seen as inadequate for research. One modeler said,

I don’t want to use [this weather forecast model] as a research tool because it is a great weather forecast model, but since you don’t know everything they have switched on and adjusted, I don’t feel like using it. (…) For a weather forecast model, it is very important that the result is correct, that you get a cyclone or weather. That’s why I don't think you should use an operational model for research.

This quotation indicates how “tuning” in operational models makes it more difficult to know what has caused the results. It also exemplifies the tension between, on the one hand, a good result and, on the other hand, strict use of physical principles in constructing simulation models.

Tension between Representation and Result

Depending on whether the evaluation of parameterizations takes place within theoretical or pragmatic construction and if the purpose is to be useful in explorative or applied use, modelers may come to different conclusions, as the following quotation exemplifies.

The point was to simulate a tropical cyclone and then [the modelers] looked at different ways to do a parameterization (...). Which one would give the best result? Which one is physically correct? And what is the goal? Is it to have a physically correct parameterization or to have one that gives a good result? It depends on what you should use the model for. It is always a difficult trade-off.\textsuperscript{216}

\textsuperscript{215} Some modelers mention problems with working with these models because of their more specialized coding. This makes them more difficult to use for modelers who work with theoretical construction or representative use since they rarely have any formal education in programming and consider it as a side-task.

\textsuperscript{216} The attitudes towards parameterizations are considered decisive in Shackley’s (2001) distinction between different epistemic lifestyles in climate change modeling. Parameterizations are used as a \textit{prism} through which differences between the modeler types (seers and constructors) are detected. “While for climate model constructors the sole purpose of parameterizations is the representation of physical referents, for climate seers physical referents are only one consideration to take into account alongside other factors related to the model’s intended application (tractability, computer power requirement, interpretability, and so forth)” (Shackley 2001: 117).
In this quotation, the modeler compared the “best result” (i.e. prediction) with the “physically correct” result, as if these are two possibly contradictory characteristics. According to modelers, the “basic philosophy” behind parameterizations – and simulation models in general – is “the more physical the better”. However, a “physically correct” parameterization does not necessarily improve the result of the simulation model. This illuminates the tension between the predictive performance of a model and its construction. The modeler cited above suggested that it is “always” a “difficult” trade-off, but it is likely to be of special importance in theoretical construction, in which the “physics” of the parameterization – and the simulation model as a whole – is most important.

One modeler, who told me about the development and implementation of new components in an operational model, said “You develop something that works nicely and is physically correct, when you put it in the model then it is in interaction with all other processes in the model, so you can really make forecasts worse.” He added “that happened more than once to people”. Since parameterizations are often tested in smaller models or research models before they are implemented in operational models, this implies that even parameterizations that have worked well in one model do not necessarily work as well in another. In line with this, several modelers emphasized that parameterizations should be “portable”, which primarily refers to their numerical stability.\textsuperscript{217} The purpose of making parameterizations portable is to enable implementation in different models without disturbing the rest of the model too much. Considering that programming changes (translates) equations from being isolated immutable mobiles into mutable computer code, and thereby producing greater uncertainty in their interaction with the rest of the computer program, it becomes practically self-evident that portability is an important characteristic of parameterizations from a practice viewpoint – a conclusion that is impossible to draw if one maintains the view of simulation models as principally theoretical constructions in a mathematical form.

More generally, model construction strives to make the numerical model “work” as a whole, which is a general prerequisite of simulation modeling because simulation models are materialized analytical models. In practice, these characteristics of models have to be taken into account; otherwise simulation modeling becomes impossible to carry out. For the analyst, the practice perspective reveals the role of technology – the materiality of modeling. At the same time it is difficult to grasp in-depth the role of model technology, coding, computing, etc., not least because modelers tend to

\textsuperscript{217} Models are also discussed in terms of portability, but this seems to have more to do with application for different cases and regions. For example, in relation to one model that was commonly used, it is suggested that, “The portability makes it possible to place this limited area model over arbitrary regions on the globe, and subroutines may be exchanged with certain other model systems” (www.misu.su.se 10/10/2004).
“black-box” the role of computing themselves (or blame on it the problems in simulation modeling). However, epistemic and technological aspects remain closely related in model development (cf. Merz 1999: 305).

The Role of Simulation Models in Practices

How can work with simulation models as question-generating scientific objects or answering machines be related to the practices presented earlier? This discussion relates to some previous studies in combination with the findings from the present one in order to further elaborate on how science meets technology in simulation modeling.

The simulation model is principally an epistemically object for the modeler concerned with representation, but every simulation model has to “work”. While concentrating on the conceptual foundations, representative modeling also involves tracing problems in the coding (the computer program) or programming, as is the case in any type of model construction. In applied use, the relation is reversed and the users conceive of the model more as a simple tool (cf. Morrison & Morgan 1999). Simulation models for applied use (operational models) are valued in line with their “score”, which basically evaluates their capacity as answering machines and their function as technical artifacts (cf. Rheinberger 1997). Simulation models used for exploratory purposes are also evaluated or, rather, “validated” or “verified”, according to their ability to reproduce observations. However, whether or not a simulation model agrees exactly with observations is secondary in this practice, since the primary question is if it is possible to use the simulation model to learn, understand, and make suggestions about the atmosphere, i.e. use the model as a question-generating tool of investigation (cf. Rheinberger 1997, Morrison & Morgan 1999). Nonetheless, modelers still have to prove that the simulation model works as an answering machine before it can act as a question-generating epistemic object.

In her study of event generators, a type of simulation model used in particle physics, Merz (1999) suggests that the simulation models “oscillate” within a space that is delimited by their conceptual content as epistemic objects and their black-boxed features as technological artifacts. This implies a continuum – rather than a dichotomy – between these forms of objects. Consequently, one may ask to what extent the role is stable in principle while shifting in the practical activities of meteorological simulation modeling. In the following, I will briefly discuss the implications of Merz’

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218 This inverts the chronology of the process compared to how epistemic objects/technical artifacts have been discussed previously. Merz (1999: 296) has also pointed out that event generators in physics accomplish different tasks in different contexts and functions are drawn out in the local setting in a dynamic that has no direction in time.
(1999) discussion in an attempt to further clarify the role of simulation models as epistemic objects or technical artifacts.

According to Merz (1999), simulation models become epistemic objects when modelers work with the conceptual cores of the model – the dynamic model. Merz (1999) also relates Rheinberger’s (1997) notion of technical artifact to the idea of the black box and states that when modelers trace unexpected or “wrong” results, they “open up” the computer program. Thereby, their consideration of the conceptual foundations of the model and investigation of its computational form makes the simulation model act as an epistemic object (cf. Merz 1999: 309).

Latour (1987: 131ff.) provides the most extensive discussion of the term black box in which a crucial feature of a black box is that it acts as a unit, but also that it has the character of routine use, as Rheinberger (1997: 30) has noted. This can be interpreted in different ways but of primary importance in this context is how this relates to the notion of epistemic object/technical artifact. On the one hand, looking into the problems of a tool and seeking the solution in its different parts implies opening up the black box, since it is no longer a unit. On the other hand, it does not follow that this involves contesting its capability to act as a unit. Relating black box to stabilization, I view a black box as a stabilized fact or a technology that is taken for granted and given the function to act as a unit.219 Therefore, tracing “technical” problems does not necessarily mean that the black box has become destabilized and has lost its position as a part of the technical repertoire, a primary feature of a technical artifact. Thus, what appears to be mixed in Merz’ (1999) discussion is the form of the model that modelers work with during different activities (e.g. conceptual or computational) and its role (epistemic or technological) in a particular practice or subworld.

In scientific practice (experimental system), the role of an object or thing, in this case the simulation model, is not the same as its form (including the status implications that this perception of form has in the scientific community, i.e. the status of conceptual work vs. “technical” work). The notions of epistemic objects and technical artifacts concern the role of objects in practice and in fact, epistemic objects can exist in multiple forms (cf. Knorr Cetina 2001). While modelers work with the simulation model in different forms, they are all connected through the simulation. This “mix” is essential in understanding simulation modeling and it is not simply because they are founded on a theoretical construction that simulation models function as epistemic objects. Conversely, simulation models do not function as technical artifacts simply when coding and programming is on the agenda.

219 This is not made clear by Latour (1987), yet it is of crucial importance if his analysis is to remain symmetrical in terms of science and technology (while it in fact is strongest when applied to technologies). To define black box as I have done above makes it more applicable to both science and technology.
Rather, the complexity of analyzing simulation modeling arises exactly from the fact that it is by being a computer program that the theoretical construction can produce “new” and question-generating worlds around which scientists can meet and collaborate on new types of projects.  

Concluding Remarks

This chapter has presented a typology of modeling practices. Typologies of simulation modelers, primarily climate modelers, have been presented in earlier work. Shackley (2001), for example, presents a typology that is based on the distinction he makes between different epistemic lifestyles in climate change modeling. Another distinction is between the “purist” and the “pragmatist” (Shackley et al. 1999: 431f.), and a third between scientists who seem primarily interested in how well the model simulates the dynamical features of the circulation and others who are more interested in the model’s simulation of energy fluxes; thermodynamics (Shackley et al. 1999; Shackley 2001). I will address some general problems with these typologies, of which one is the focus on individual modelers (or their lifestyles). This hides the fact that what is taking place in simulation modeling is not primarily a struggle between people, but rather between practices involving different perspectives about what climate research (or in this case meteorology) should be like and where ensembles of people rather than individual modelers constitute the principal actors. Even understood as ideal typologies they do not make up for this since the focus on individuals (or ideal type modelers) neglects the role of the simulation model and how it is incorporated into practice, and remains limited to the modelers’ view of

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220 Conceptual models are not capable of producing “new worlds” in the same sense.

221 Epistem ic lifestyles refer to the set of intellectual questions and problems, and the accompanying set of practices, that provides a sense of purpose, achievement, and ambition to a scientist’s working life, as well as the more mundane sense of carrying out those activities necessary for ‘getting the job done.’ (Shackley 2001: 114f.) The concept also includes the social networks and connections through which scientists organize their individual and collective work. Thus, the concept of epistemic lifestyle implicitly consists of two dimensions and this creates problems in Shackley’s construction of the three styles; climate seer, climate model constructor, and hybrid climate policy style. While the hybrid policy style is based on choices guided by political interests “outside” scientific production, the distinction between constructor and seer is based on epistemic choices. The categorization of modelers is also based on two different levels: The individual in the case of the seer and constructor and the organizational in the case of the hybrid political style.

222 The pragmatists’ aim is linked to current policy and they are involved in using and applying models for various purposes, mostly concerned with the study of past or future climate change. For the purpose of developing better representation of the key processes involved in climate and climate change, the purists are principally interested in the analysis, development and improvement of state-of-the-art climate models, guided by academic questions and less interested in policy-oriented climate change projections (Shackley et al. 1999).
models and how they think models should be used. Thereby, the crucial role of simulation models in actually articulating work and bringing scientists together (and, in consequence, focusing conflicts) cannot be recognized; a role that becomes evident not least in the work involved in building and developing simulation models. This question will be addressed in the concluding chapter.

It follows that simulation models are also important in terms of the interconnection between practices. In the previous chapter, experimental practices were primarily analyzed as being tied together through collaborative projects, but they were also connected through an innovation process in relation to measuring instruments. In a similar way, modeling practices are connected throughout the lifetime of a simulation model from its construction and use for research purposes to its pragmatic (re-) construction, development and use for operational weather forecasting. Thus, the practices more literally constitute a line of work, ranging from theoretical construction, explorative use, pragmatic construction, to applied use. However, it is the simulation model, rather than collaborative projects, that brings the working practices in modeling together as a collective (subworld).

I draw two primary conclusions about meteorological simulation modeling on the basis of this chapter. First, I conclude that the clear division between weather forecasting and meteorological modeling for research purposes is not only a matter of different commitments but that it also results from boundary work (cf. Gieryn 1983; 1995). Weather prediction seems to serve as a frame of reference for meteorological modelers in academia, even for those who seem uninterested in if their research improves weather forecasts or not. The development of numerical modeling for simulation purposes within meteorological research has been very oriented towards numerical weather prediction (see e.g. Edwards 2001) and MISU has historical roots in numerical weather prediction. While there are differences in terms of, for example, the role of the model and patterns of collaboration, there are close links and similarities that the modelers sometimes wish to de-emphasize or diminish, for instance by taking strong positions regarding tuning. Consequently, to understand meteorological research based on simulation modeling, I have found it important not to exclude work related to numerical weather forecasting a priori, but to study existing relations in order to understand how the boundaries between them are created, for instance by clearly differentiating between operational models and research models and attitudes towards and use of tuning to improve results.

Second, simulation models are materialized mathematical models and I propose that the materialization in itself is a key aspect in understanding simulation modeling as working practice. In simulation modeling, an abstract model is manipulated through a digital machine, which inextricably entwines the epistemic and the technical together (Dowling 1999: 271). If analysts of simulation modeling refer to computers as the “infrastructure” of
models (Shackley et al. 1998), they reduce simulation models to (incomplete) theoretical constructions whereas “this ‘new mode of doing scientific work’ (Galison 1996: 137) is inseparable from the technology used to perform it: the digital computer” (Dowling 1999: 261). As Fritz Rohrlich (1990: 508) points out: “One must not be led to the naïve view of scientific computing: the computer simply provides much needed speed in carrying out a large number of mathematical operations and it helps in keeping track of many variables (…) The computer does a great deal more than that.” Without attending to the role of computing in simulation modeling, we neglect the complex work that the combination of theory and computing requires.

However, modelers working with representative modeling emphasize that simulation modeling is related to theory. This deserves a brief discussion in relation to the status of different forms of work. This rhetorical positioning of simulating practices serves a social function (Hine 1995: 120; cf. Dowling 1999: 263) since it acts to alter its status. Working with a dynamic model has a higher status than working with a computer program because the former is closer to theoretical and conceptual work (cf. Kraut et al. 1988; Dowling 1999) while the latter is closer to “technical” work or craftwork (cf. Becker 1986b: 151). Describing the work in theoretical terms, particularly with parameterizations, gives the impression that it is primarily a cognitive practice – in line with the view of simulation modeling as a more individual work than field experimentation – when in fact it involves a lot hands-on work and (a different type of) collaboration, especially in relation to implementation in existing simulation models. “‘Thinking is hand-work’, as Heidegger said, but what is in the hands are inscriptions” (Latour 1990: 46). In this case, the hands are on the keyboard.

Finally, the pragmatic usefulness of a model arises from its role as a “virtual laboratory” (cf. Dowling 1999). Simulation models represent (reconstruct) phenomena and idealized simulations institute new principles and entities and follow their own logic – they open up new worlds, acting as study objects themselves. Therefore, notions of virtual or alternative reality potentially contribute to grasping how simulation models function as epistemic objects and also the type of representation that simulation models

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223 Turkle (1995: 36ff.) argues that there are two primary modes of interacting with computers. On the one hand, the calculation mode is when one is conscious of and interacts with the internal mechanisms behind the machines’ (outward) behavior. The simulation mode, on the other hand, is when one negotiates the interface of the computer, without paying attention to its underlying programming.

224 There is a notable difference between the simulations in particle physics (Merz 1999) discussed earlier and meteorological simulations in this context. Particle physicists simulate an existing detector and therefore the detector and the simulation model constitute partial objects of the epistemic object (cf. Knorr Cetina 2001), and in that sense a referent. Knorr Cetina (2001: 183) argues that the meaning and importance of partial objects as well as epistemic objects in practice do not arise from replacing a “real” object (a referent) but through their way of expressing themselves, leading to further research and thereby leading epistemic practice.
generate. Nevertheless, it is a crucial point that scientific simulations assume an *empirical world* in many ways: as the world to be modeled, as a test for the validity of the results, or as the world in which the results of the simulation are implemented (Knorr Cetina 1999: 245f.). Consequently, part of simulation modeling practices consists in connecting simulations to this empirical world as a reference (cf. Knuttila & Voutilainen 2003: 1493). An interesting question that I will address in the next chapter is therefore how this connection is constructed and how (field) experimental practices are involved in this.

Recalling the last chapter on experimental practice, it did not indicate any crucial steps in which experimental work rely on simulation modeling. This present chapter showed how modelers depend on data to be able to carry out some modeling practices. Because of the quantity and geographical spread of data used in data assimilation, it relies on observations from the World Weather Watch. However, experimental data is of use particularly in constructing parameterizations and evaluating models. Thus, in these activities, modeling does not depend on collaboration with peer modelers as much as on the work of experimentalists.

For every world, other scientific and non-scientific worlds with which it interacts constitute a set of social audiences that attend to its work (Gerson 1987). Each audience holds unique expectations of other scientific social worlds and subworlds, makes different demands upon its research, and offers a different pattern and amount of resources in return (cf. Clarke & Gerson 1990: 191). In the case of meteorology, the production network traverses the subworlds of simulation modeling and field experimentation and generates *intersections* where segments of the two subworlds meet. The two following chapters will analyze these intersections and the related negotiations and translations more closely, primarily from the point of view of simulation modeling.
Although experimentalists and modelers perform most of their work separately, some activities act as bridges between them. The evaluation of simulation models is one such activity that I will focus on in this chapter. As I discussed in Chapter One, establishing a (universal) criterion for verifying knowledge has constituted a central focus in classical epistemology. As the expression “model evaluation” suggests, it concerns the verification of simulation models and the results derived from using them. As opposed to a more philosophical discussion of this issue, this chapter will analyze the stories I was told about model evaluation practice, i.e. comparison of model output and data, in relation to what comparison is about and how it is performed as an internal arena where the different perspectives of modeling and experimental work become salient. An additional interest is how the scientists understand their work in relation to normative conceptions of science and how this can be related to the production process.

In the two previous chapters, I outlined the different practices that make up the subworlds of modeling and experimentation. By doing so I also laid out their internal differences. Since this chapter focuses upon the relationship between the two subworlds as an internal arena, internal differences among experimentalists and modelers are partly neglected due to my focus on the common perspective in experimentation and modeling respectively. In order to analyze the link between simulation modeling and field experimentation as constitutive of the same production network, but forming different subworlds, this chapter draws more explicitly upon the “extended translation model” (Callon 1995: 50) in the actor-network approach. I will therefore show more clearly how the production of meteorological knowledge is related to its social organization.

The first section suggests how the comparison of data and model output can be approached from the perspective of network building. Thereafter, the comparison of model output and data is briefly discussed in relation to

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225 For more philosophical work, see e.g. Plutynski (2001); Winsberg (1999).
226 For sociological work on norms in science, see e.g. Mulkay (1975); Mulkay & Gilbert (1981). See also Merton (1938; 1973).
227 While this structure may give the impression that the analysis was mainly theory-driven, the theme of model evaluation and falsification developed during the analysis of data.
the falsification principle (cf. Popper 1959). In the following, I analyze modelers’ arguments in relation to the principles of model evaluation. It is concluded that model evaluation is a way for the modelers to produce social legitimacy for simulation modeling as a segment of the social world but that comparison of output and data is also a standard way of producing reference in relation to the empirical world.

From Center to Periphery

Because simulation modeling “acts” at a distance from the real atmosphere by using a combination of equations and representations of processes, it can be seen as a “center of calculation” (Latour 1987). Before turning to the consequences of this more in detail, it is necessary to discuss a few aspects regarding inscriptions, inscription devices, and transformations, and also to briefly recapitulate from Chapter Two, where the analytical tools were presented more thoroughly.

Inscriptions result from transformations of matter to forms enabled by so-called inscription-devices, “items of apparatus or particular configurations of such items, which can transform material substances into a figure or a diagram” (an inscription) (Latour & Woolgar 1979/1986: 51). The consequence of this is that inscriptions are regarded as having a direct relationship to the material object under study. Callon (1995) suggests that any text is an inscription but ignores the idea of inscription devices as transforming matter into form (words, numbers). In my view, however, this is a fundamental aspect of scientific production. A crucial difference is thus between the “inscriptions” effected by simulation models and those created by measurement instruments (as inscription-devices). These inscriptions entail completely different practices.

The experimental translation chain can be depicted in the following way (cf. Callon 1995: 51).

(matter) → instrument → marks → diagrams → tables → curves → observational statements etc.

In the chain above, marks represent first order inscriptions, diagrams second order, etc. The translation chain in computer modeling differs from the “basic” (experimental) translation chain by “black-boxing” everything that has led to the theoretical statements and “laws of physics” from which the

228 In Laboratory Life, Latour and Woolgar (1979/1986: 76) also discuss literary inscriptions as the production of papers. From this point of view, Callon’s (1995) statement is less important.
model formulation is derived and then implemented into the computer (see below).

变形的陈述 → 方程 → 离散化 → 近似 → 计算机代码 → 输出 → “手稿”

Empirical findings may be the original point of departure for these statements and in that sense a transformation has taken place at a much earlier stage. However, the part of the chain that the modelers work with does not include any transformative (matter to form) stage in a strict sense. Through initialization, simulation models start calculations from data or model output in order to produce more “data”. However, the path of these calculations is directed by the construction of the simulation model, which is able to generate output on the basis of very little initial data.

From this perspective, then, simulation models can be seen as centers of calculation (Latour 1987). Centers of calculation calculate only with inscriptions and mix inscriptions together from the most diverse disciplines in these calculations (Latour 1990: 59). In simulation modeling, the construction of parameterizations exemplifies this feature by mixing theoretical assumptions with inscriptions resulting from various measurements.

While modeling represents work carried out in the center, experimentalist practices come to be seen as local and peripheral. It is important to refer to center and periphery rather than inside and outside since the center is the core of the actor-network and it is this network we are disentangling here. Hence, model validation exemplifies a movement from the center to the periphery to show how assumptions in the center hold in the periphery (of the network).

Translating the world towards the center is one thing; gaining an unexpected supplement of strength by working inside these centers on the nth degree inscription is another. (…) Nothing is irreversibly gained at this point if there is no way to translate back the relation of strength that has been made favorable to the scientists’ camp. (Latour 1987: 247)

This involves bringing together inscriptions in the same shape from different realms, for example diagrams resulting from different measuring instruments used at different places, model simulations and statistical calculations, which appear similar because of their transformation into the same shape. Furthermore, Latour (1987: 243) notes that on the basis of studies of scientific practice, it can be concluded that “abstract” forms of mathematics never “apply” to the “empirical world” because at a certain point instruments start to inscribe forms or figures on paper. A calculation on a piece of paper can apply to the outside world only if the outside world is itself another
piece of paper in the same format. According to Latour (1987), scientists build their networks by giving the outside the same paper form as the things that their inscription-devices produce.

Measurements start the experimental translation chain by transformations taking place in a local, specific situation. Simulations, on the other hand, exemplify formalism, the acceleration of displacement without transformation (Latour 1990: 47) since simulation models are not part of (original) inscription devices but centers of calculation. Consequently, the point with comparing curves from output to curves from data is to attempt to attach (as opposed to detach) traces of the material world to formal arguments, and by doing so to produce an “inscription” that seems to share the same feature as inscriptions of an earlier order: the appearance of a direct relation between formal reasoning and the material world. However, it is not because experimental work involves transformation that reference is an automatic consequence. Latour (1995) has analyzed this particular chain of translation in his ethnography of the “pédofil” in Boa Vista, where he shows how step by step, soil turns into the idea of soil as lumps of earth move to a discrete color in a geometrical cube, and are then coded into coordinates and finally into inscriptions. I follow a similar line of reasoning but approach simulation modeling as a different case of reference production.

The comparison of data – as a product of experimentalists – and model output – the products of modelers – suggests that the evaluation of simulation models can also be regarded as an intersection where social worlds and subworlds meet. Within symbolic interactionism, the conceptualization of interaction among disciplines and other scientific worlds is a matter of analyzing patterns of negotiation and commitment among them (see e.g. Strauss 1978a; Becker 1982), which also brings practitioners perspectives to the fore. The analysis therefore takes its point of departure in the “negotiations” between modelers and experimentalists with regard to the evaluation of simulation models, including the modelers’ expectations regarding the supply of data, in order to illustrate model evaluation both from a social world and an actor-network perspective. These are not necessarily coextensive: From an actor-network perspective, social worlds cannot be used to explain, but should be explained themselves. Thus, while these tools complement each other in some cases, they may result in an analysis that is also to some extent contradictory, as I discuss in concluding remarks of this chapter.
Confronting Models with Data – Falsification Re-negotiated

Modelers use data to compare their simulation results with in order to evaluate model output. Comparing model output and observations is a challenging task in many respects. Simulations produce output divided into many variables, all of which should ultimately be compared to data. Because the simultaneous evaluation of all parts of a simulation requires huge amounts of data, evaluation is often reported in a series of articles. This makes a complete model evaluation a much more complex task than, for example, instrument calibration, which only concerns one parameter. Yet similar to instruments, it is especially important to compare model output with data in the case of new or modified models, where the purpose is to establish the model as a useful scientific tool. Data is also compared with model output of real case-simulations. In articles, model evaluation is often referred to as model validation, although it is questionable whether models can be validated at all.

Edwards (1999) discusses some epistemological issues regarding validation of climate models mainly based on an article in *Science* on the same topic (Oreskes et al. 1994). Climate scientists use the term validation to describe the evaluation of model results against empirical observations, but this usage is contested. Oreskes et al. (1994; cf. Edwards 1999) maintain that modelers cannot properly speak of validation or verification, which in their view implies proof of truth. Models are inductive arguments and since inductive propositions cannot be proven with certainty, models, like theories, cannot be verified. Perfect agreement with observations does not guarantee that the principles, which the model embodies, are true. Validation is less stringent than verification but Oreskes et al. (1994; cf. Edwards 1999) point out that it is commonly used as a synonym for the latter. At best, they claim, models can be confirmed through agreement with data. A confirmed model remains within the set of viable alternatives for true explanation and raises the probability that the model embodies “true principles”, but it cannot confer absolute certainty. This is consistent with Popper’s doctrine of falsification, which holds that scientific hypotheses can be proved false, but not true (Popper 1959). In concluding his discussion of validation, Edwards

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229 Depending on the type of simulation, modelers use different types of data to compare with. In this chapter, I will primarily discuss evaluation of research models with experimental data from field campaigns in order to retain the focus on the relationship between experimental work and modeling. I will only analyze the evaluation of models used for research purposes.

230 For instance, in one article it is stated: "Since the modeling system is so complex, it is complicated to perform a total validation of the entire system simultaneously. It also requires a very large and detailed dataset of all dependent variables. During the development stage, every part of the model was tested and compared to observational data. (...) A number of validation experiments were performed for the meteorological part of the model for different types of terrain and surface conditions" (Svensson 1996: 645).
(1999) notes that the current results of climate model evaluation suggest that large-scale features of current climate models are well simulated (cf. Dowling 1999: 267). This discussion illustrates a more traditional and asymmetrical approach to this question where the role and function of epistemology, as formulated by rationalist philosophy of science, is taken for granted. It differs from the present analysis that seeks to understand if, and in that case how, the methodological rules proposed by for example Popper and Lakatos play a role in practice, not to what extent models are “true” or well-constructed according to these rules (cf. Norton & Suppe 2001).

What do the modelers say about model evaluation? Modelers at MISU emphasize that it is only possible to invalidate, not to validate models. Some mention Popper’s idea of falsification in relation to their own practice. For instance, one modeler said,

I think Popper is very basic. All theories are good until they are falsified and fall. It’s the same thing here. We set up a theory about how the Earth’s climate works that is dependent on temperature and radiation etc. With what we know today, it gives us a temperature, but perhaps we have totally missed a component in the system. We can’t say anything about that.

Hence, it is impossible to show that a simulation model is successful once and for all, as the following quotation suggests: “You cannot take a case and see that it works well for this case and then think everything is all right, but you have to test many different cases.” Another modeler emphasized this too.

You can only invalidate a model. It doesn’t matter how many datasets you use to interpolate your numerical model with. It’s enough with one single dataset that the model can’t deal with and the model is out. You can never validate a model. You may know that a model works for this particular lapse of events, at this particular place, in these certain conditions, but one can never know how it would work to apply it on another phenomenon at a completely different place.

However, models do not seem to be “invalidated” or declared inadequate on the basis of observations. Although modelers in principle hold that models can be invalidated but not validated, in practice they neither verify nor

231 See also Fuller (1992) for a critique of both classic epistemology – which tried to impose normative considerations on science – and the tendency for (empirical) studies of science such as this one to discuss norms only as they emerge from their material. According to Fuller (1992), both approaches lack critical potential in terms of the revision of knowledge enterprises, which is a question for a social epistemology (see Fuller 1988) but beyond the scope of this thesis. See also Chapter One.

232 Shackley and Wynne (1996: 284) also suggest this when they report that climate modelers found it “ridiculous to think that one data set (…) could throw into question the authority of GCMs”.

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falsify models. Recent discussions have also illustrated confusions and uncertainties that accompany model evaluations (e.g. Randall & Wielicki 1997; Shackley et al. 1998; 1999).

This is not surprising considering earlier sociological research about science. First, sociology of science showed a long time ago that neither the principles of classic epistemology nor Merton’s (1938; 1973) norms of science govern how science is actually produced (Mulkay 1969; 1976). Second, scientists cannot explain scientific practice through recourse to rational criteria (Bloor 1981; cf. Sundqvist 1990: 59), which also follows from Bourdieu (1977: 19), who suggests that descriptions that draw upon rules tend to express a social practice that obeys different principles. Whereas rhetoric or discourse often have their own logic and tend to be invoked to legitimate purposes, practical work has another logic (Bourdieu 1998).

Thus, the need to be critical towards scientists’ statements about their work to outsiders (see e.g. Mulkay 1975) is perhaps most evident regarding their appeals to norms or “rules” of science such as Popper’s falsificationism. However, even if rules are not followed, this does not mean that the reference to Popper must be regarded as merely standardized verbal formulations and rhetoric which is independent from the scientists’ understanding of their own practice (cf. Mulkay 1976: 69; Mulkay & Gilbert 1981). The rhetoric of rules may play an important role as part of practice and the perspective that lies behind this can be analyzed (cf. Latour & Woolgar 1986: 283; cf. Gubrium & Holstein 1997). I will therefore approach these narratives both as perspectives and as rhetoric (discourse) (cf. Gubrium & Holstein 1997). Thus, modelers “defend” modeling and simulation models in different ways, and offer various suggestions to explain

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233 For critique of the view of rules as a way to understand the reproduction of practices, see e.g. Bourdieu (1977); Turner (1994; 2001). Within the field of science studies, see e.g. Knorr Cetina (2001); Collins (2001); Mulkay (1980). Compare also Blumer (1969: 19).

234 In any case, Mulkay and Gilbert (1981: 128) have pointed out that any attempt to translate philosophical conclusions into rules of scientific action, without clear recognition that a major transformation of perspective is involved, will be misleading because philosophical analysis of scientific knowledge begins with the logical structure of end products of scientific practice. In contrast, scientists are involved in making a contribution to a science. From a practice viewpoint, philosophy starts at the wrong end, with ready-made science, not with science in action (cf. Latour 1987).

235 Mulkay and Gilbert (1981) have also analyzed scientific practice in relation to Popper’s ideas in terms of how much scientists know about Popper, if they use Popper’s rules and in that case how, and how to understand scientists’ reference to Popper. They conclude that there is a disagreement on the meaning of Popperian method and rules and that the general issues of how particular acts are related to rules are indeterminate. Furthermore, Mulkay and Gilbert (1981) suggest that Popper’s practical failure reveals a basic dilemma facing any prescriptive philosophy of science. My analysis shares some conclusions from Mulkay and Gilbert’s (1981) paper but differs in the sense that it is not concerned with the rules per se as much as with how the rules prescribe a connection between simulation modeling and field experimentation. It is this relationship, embodied in the activity (model evaluation) that is the focus of this chapter. See also Mulkay (1975) about norms in science.
why models are useful and thereby bolster the belief in models in spite of the lack of correspondence with data (cf. Evans 1997; see also Star 1985).

The following sections present an analysis of modelers’ arguments in relation to model evaluation. Modelers’ arguments are divided into three empirical themes. First, data problems refer to arguments that see problems in data rather than the models. Second, modelers refer to the theoretical model that the simulation model is based upon, thereby largely neglecting the falsification principle altogether. Third, questioning the comparison of data and model output involves (different) views on what constitutes agreement, but also a problematization of comparison in itself. In relation to these arguments, I will discuss the views of experimentalists as well. Thus, this chapter is an attempt to analyze model evaluation both as a matter of connecting simulation models to experimental practice in a production network (traversing the subworlds), and as an intersection between subworlds.

Attributing the Problems to Data and Experimental Work

On the most general level, modelers and experimentalists share an interest in the atmosphere but their different activities also result in different practical concerns. Many researchers point out the distance between modelers and experimentalists and consider it a problem.

A big dilemma within meteorological research is that the experimentalists are over here [the researcher points his hands to the left] and do their stuff, while the modelers are here [the researcher points his hands to the right] and do their things. Then they meet sometimes, and often it is the modelers who “meet” the experimentalists because they want data so that they can test their models. It is rarer that the experimentalists “meet” the modelers because they want something.²³⁶

This respondent mentions that the two groups of researchers “meet” but that modelers are mostly concerned to do so since they need data as a resource in some of their working activities. Experimentalists, on the other hand, are less interested in the resources modelers provide. According to one experimentalist, “the empirical scientist could ‘live’ without the modelers – just put their instruments in the field and measure whatever is in the air” (email correspondence). Consequently, the provision of data seems to be one

²³⁶ Experimentalists sometimes use models in order to calculate air-mass trajectories for the purpose of interpreting measurement data, where it is important to know what type of air-mass that passed through the site during the time of the measurement. This can be done over the web by using operational models. It is sometimes a step in the interpretational process of measurements but not used directly in order to interpret. It seems to be additional in the same way as photographs and log notes.
of the problems in model evaluation as an intersection. What are the reasons for this?

**Insufficient Data**

One problem the modelers face when they wish to compare simulation output with data is that they do not always find observations that they consider useful.\(^{237}\) It is very difficult to measure some features, especially in the case of inhospitable locations. Measurements of the stable boundary layer in the Arctic region exemplify these problems. In addition, some parameters of (modeling) interest are impossible to measure, as is exemplified with the quotation below.

> All the things you would like to know about the atmosphere we can’t measure (…). A lot of the things we can measure about the atmosphere aren’t exactly the things that you want for instance if you have a theoretical model about the atmosphere and about how that works. What we can measure is perhaps not what the theoretician would like to have.\(^{238}\)

Another reason why modelers cannot use data is that experimentalists do not always refine or calculate the data into the quantities the modelers want, at the same time as modelers generally lack the knowledge to make use of raw measurement data (see e.g. Randall et al. 2003, interview with NN).\(^{239}\) This is related to the circumstance that experimentalists have different concerns compared to modelers. Experimentalists are interested in starting data interpretation as soon as possible and less interested in working with more data processing than is deemed necessary. Another example of modelers’ lack of understanding of the concerns of experimentalists is suggested by the following quote, which exemplifies how modelers would like experimentalists to consider the requirements of modeling when they design their experiments, if they wish to make use of their measurements in modeling.

> Very often (…) when one is going to run numerical models and it appears that if you would have thrown out some expensive aerosol instrument and

\(^{237}\) Since simulations are often used to generate representations of systems for which data is sparse (cf. Winsberg 2001: 447), this is probably a general problem in simulation modeling. At the same time, the World Wide Weather Watch and the field campaigns enable access to more data than for instance in astronomy.

\(^{238}\) It is important to note that in this question, the respondent (an experimentalist) does not mention “modelers” but “theoreticians”. However, as I noted in the previous chapter, modelers sometimes refer to their work as theoretical and in any case, they consider it more theoretical than experimental work (see also Dowling 1999).

\(^{239}\) A few respondents mention that this problem is attended to through the initiation of projects with the specific objective of coordinating measurements and creating databases in order to make them useful for both experimentalists and modelers. This, however, seems to be relatively new and under development.
perhaps sent a few more radio probes you would have…[had better data available to compare with] But that’s not sexy enough.

Nonetheless, what this modeler views as being “sexy” or attractive rather than useful, may be of importance for answering the experimentalists’ questions. Other instruments, such as radio probes used for measuring temperature, might just require time for data processing, when experimentalists prefer to spend time on the data they have produced for their own purposes and for writing research papers. Moreover, “some expensive aerosol instruments” are likely to refer to the instruments tied to instrument expertise, thus suggesting that these are of less value for meteorological simulation modeling.

Further, experimentalists point out that modelers often expect results that are incompatible with what experimentalists can produce.

It is important that the models are constructed in such a way so that it’s possible to work on with existing data, or data that we can get in the close future and in that case, much work on modeling is made on questionable grounds. To test a certain aspect would require a million airplanes or something, at the same time in the air. Many modelers do things we do not have the conditions to create data for to test or support.

In spite of the fact that it is difficult to compare output from models with existing observations, the modelers I ask do not prioritize rectifying this stated deficiency. For example, one modeler said that it is too time consuming to (re-) construct models with the purpose of making better use of observations. Simulation models develop gradually and contain in their construction built-in standardized ways of working with them. In the view of modelers, experimentalists should consider this when they make their measurements and not expect models to change for the sake of data.

Not least since data processing in general is something experimentalists consider a boring, routine job, not counting as “doing science”. They complain that empirical articles often mostly consist of a presentation and summary of observations with too little interpretation. Some suggest that it is due to a lack of time since the analysis of raw data is a full-time job. This also seems to be one of the reasons why modelers suggest that they are not interested in doing this in addition to their modeling work (see also Randall et al. 2003). Worth noting, however, is that many model simulations are set up in such a way to facilitate comparison with observations from certain campaigns.

Compare Nutch’s (1996: 224) note that one of the sociological dimensions of Kuhn’s (1970) Structure of Scientific Revolutions is the attention to the bifurcation of scientists into theoretical leaders and data-collecting workers. This is also related to Becker’s (1982) discussion about support personnel in art worlds. In this context, it becomes evident that any experimentalist can become a support worker for a modeler and it is thereby unclear who is the “scientist” and who are the support people in this relation (cf. Becker 1982: 91).
also worth adding that a lot of information from measurement instruments is not meaningful in the model, as the following quotation suggests.

Of course there are possibilities to extract information in the models in a way that makes it more comparable to the measurements that are taking place. But (...) as long as you speak of basically all models from the meso scale and up, that’s in the scale from a few kilometers and more, the information that you measure in detail with precise for instance aerosol instruments, that information does not exist in the model. It doesn’t work to modify the model in order to give that information.243

Consequently, this modeler claims that adjusting models in order to enable better comparisons with the data that the experimentalists produce is, in many cases, not considered to be an option. On the other hand, some scientists think that it is experimentalists who should consider if their data is needed for modeling purposes. As one respondent put it,

The one who makes measurements and does not know what to use it for is (also) on shaky ground. Both camps have a great responsibility to actually know what type of data that’s needed for the existing model. A typical social behavior on the measurement side is that once you learn to make a certain measurement, then you want to do that for the rest of your life and defend this with that these measurements are needed, continuously, without having the slightest idea how they can be used for existing models.

However, it is noteworthy that because of the dynamic consistency of models, one of their strengths from a modeling viewpoint is exactly the fact that they can give information about parameters that are difficult or impossible to measure.244 The model is an internally connected system and this is also one of the reasons why it is considered difficult to change existing models since, as is noted in Chapter Six, a small change in one parameter affects the performance of the whole model. Modelers also state that an additional strength of the dynamic consistency of models is that they always result in one value for a parameter. Measurements made on different occasions are independent from each other and different techniques used to

243 This has to do with the meteorological basis of many atmospheric models, not least climate models. This is elaborated in Chapter Eight.
244 This is described in more detail in the following quotation. "There’s another strength in analyzing measurements for numerical models and that is that there are parameters that cannot be measured. If I use the vertical velocity in the atmosphere to exemplify, the wind has a certain velocity in horizontal direction but it also has a third, vertical component. (...) There are no measurement instruments that are accurate enough to measure the mean vertical wind velocity, but it is derived from the model as a dynamical necessity. Since the model is dynamically consistent, you can calculate the vertical wind when you have made the horizontal wind field and the temperature field look proper (...). Therefore one can say that if the rest of the model seems to correspond to measurements, then one can also believe other fields. (...) Even if you can’t measure them. You can fill the holes.”
measure the same parameter tend to give different answers. This is a recognized but bothersome “fact” among experimentalists, but a reason to question the reliability of data among modelers.

The quotation also points at a lack of communication between modelers and experimentalists regarding what data modelers need. The lack of communication between the groups seems to be considered a general problem, as mentioned above. In spite of the current gap between modeling and experimental practices in terms of model evaluation, it should be acknowledged that on the basis of informants’ accounts, there are projects where experimentalists and modelers work together and try to combine their efforts. From the point of view of model evaluation, a successful way to collaborate presupposes communication between modelers and measurement-oriented researchers from the beginning. However, one scientist suggested that funding for a measurement-oriented project with a modeling component does often not include the modeling contribution. The reverse situation – including experimentalists in modeling projects – seems to be very rare. In part, this is probably related to the long duration between measurement and the existence of useful data. It takes several months, sometimes even years, for experimentalists to organize joint campaigns that take place at various places where they measure for a few weeks. These measurements generate interpretable data after months of processing, sometimes including chemical analyses and quality control. Thus, while experimentalists organize a whole campaign to measure a certain process, simulations of the same process are conducted relatively quickly in the modeler’s office (cf. Knorr Cetina 1992: 125). In a sense, this illustrates the logistical advantage with inscriptions in the equation form and how simulation models, as centers of calculation, can master the organization of space and time in an almost literal sense (cf. Latour 1987).

**Status and Expectations**

Although the lack of data is considered problematic and several modelers emphasize the constant need to have data,246 there are also modelers who pointed out that they do not necessarily need more new data. “There’s quite a lot of measurement data,” said one modeler. He continued, “The ones who are working with measurements have their goals, which do not necessarily correspond to those of people who work with models. But both of them say they need each other and that is sometimes not completely obvious.” The view that modelers and experimentalists (partly) have different goals is a

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245 This scientist also suggested that many scientists try to maintain the separation and support their own type of research when they have the ability to do so in for example, reviews and scientific councils.

246 For example, one modeler said, “You always would like to have more data than you have to verify because there are so many degrees of freedom in how the model can behave and how the atmosphere can [behave].”
consequence of the segmentation of the social world of meteorological research in field experimentation and simulation modeling into two subworlds, but this quote also suggests that dependencies and expectations are exaggerated. Furthermore, while the imbalance between what modelers expect and what experimentalists supply is considered as a constraint from the point of view of some modelers (cf. Clarke & Gerson 1990: 191), it does not follow that modelers view this dependency-relation as a disadvantage. Some claim that experimentalists also need modelers. For instance, by commenting that experimentalists would “die out” if modelers did not use their data, or as the quotation above suggests; that the dependency is not as clear as some suggest and that the work of experimentalists is perhaps not that important from the point of view of modeling.

Part of this perception of the relationship can be related to the status difference between the groups, but there are other things at work as well. The following quote refers to both of these group identities:

I think that those who are more theoretical, or those who are modelers, who sit and run their models, they sometimes think (...) that those who are out measuring, they do not care about theories about why it is like this. They are just out measuring and they write about what they measure and it is as simple as that. While those who are out measuring think that those numerists [sic], or those who run models, they are too hooked on that. They have to observe reality as well.

Although experimentalists often mention that modelers do not respect their work and explicitly mention the difference in status, they also use the fact that they are “out in reality” as a reason why experimental work is important, as the quotation suggested. Experimentalists consider their practice as closer to reality, both in principle and in practice. For example, during a meeting, an experimentalist declared to co-workers in his section: “We have a closer coupling [to the atmosphere] than the mathematical and numerical models

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247 Modelers often (implicitly) ascribe themselves a higher status because modeling work is more “theoretical” than experimental work, which is considered as something more “technical”. In terms of conceptualizations of production work, modelers seem to classify much of experimental working activities as gadget work (hands-on work) rather than the higher status conceptual work (cf. Schild 1996: 88). However, it is also notable that the concept of social world is ill-suited for questions of status as it is for questions of power and inequality. This is also one of its important differences compared to e.g. Bourdieu’s (see e.g. Bourdieu & Wacquant 1992) concept of field.

248 It has also been noted elsewhere that theoreticians and experimentalists (Traweek 1988) and simulation modelers and experimentalists (Saari 2003) in other fields have developed a disregard for each other, expressing itself as jokes about various aspects of the other working practice. The following quotation exemplifies this as well. “There is a considerable amount of elitism and group feeling. Us theoreticians. Us modelers. We who are actually making measurements and who look at reality as experimentalists. [The experimentalists’] jargon is about that those bloody theoreticians do not know anything about the real world that we are measuring.”
describing the atmosphere.” Modelers seem to view this in a similar way, at least from the point of view of being outside. For instance, one modeler who had not participated in any field campaigns expressed his regrets and said at a seminar “It would have been nice to touch upon the atmosphere a little bit.” About the early simulations in physics after World War II, Galison (1996: 155) writes, “for experimenters, the Monte Carlo never came to occupy a position of ‘true’ experimentation, as exemplified in debates that continued decades later over the legitimacy of according doctorates to students who had ‘only’ simulated experiments”.

The examples above imply that to some extent, this is an exaggerated but nevertheless apt description of the current view in meteorological research, at least among experimentalists.

This conception that field experimentalists go “out and measure” is interesting for several reasons. It suggests that field campaigns not only play an important role in creating commitment and group feeling among experimentalists, i.e. in creating their social subworld, but also illustrates how the experimentalist science subworld is constructed on the basis of a “closer” relation to “the atmosphere”, due to the experience of going “outside”. However, this would be meaningless if the experimentalists were unable to bring the “outside” with them inside their network of scientific production (the laboratory) (cf. Latour 1990).

Experimentalists do travel, but while they leave their concrete laboratory (the department), they never leave their scientific practice – the laboratory as a theoretical notion and a practical reality. This is essential for how field experimental production work takes place, but by emphasizing the importance of geographical, spatial locations such as “out there” or “outside” and “in here” or “inside” in relation to simulation modeling, the fact that experimental practice is also based on chains of translations and does not provide unmediated access to reality is played down.

To summarize, modelers point out that it is important to compare models with observations and in principle, modelers seem to view measurements as an authoritative source for evaluating models in terms of if the measurement technique is successful, if the experimentalist has calibrated the instrument properly, if there is an error in the instrument etc. As a matter of fact, modelers tend to emphasize problems with data and not with modeling, and

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249 Monte Carlo simulation is a type of simulation method in which random numbers are used to simulate stochastic processes.

250 During the first campaign I visited, I found myself thinking that it was something of a paradox that experimental scientists talked about being “outside” and “in reality” when making measurements. All of the arrangements, all the equipment that is brought and required, and the conditions that have to be satisfied in some of the containers, gave an impression of moving mobile laboratories to the field, rather than a more common notion of being “outside”, thus literally supporting the view of how laboratory practices extend science to the “outside”.

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often call attention to the errors in data when they speak of comparison.\textsuperscript{251} Perhaps the most problematic question is that from a modeling perspective, experimentalists perform measurements at sites that are not “representative”.\textsuperscript{252} This issue will be discussed in more detail below. However, in this context it is worth noting that the focus on what is “representative” from a modeling viewpoint, exemplifies different ideas about what constitutes a useful or useless observation – data quality – among modelers and experimentalists.

Shifting Focus: Attributing Certainty to “the Physics” of the Simulation Model

In addition to data comparison and the replication of observed patterns as a measure of the capacity of the model, modelers point at the importance of “the physics of the model” in judging its qualities. One modeler said that if the simulation model does “absolutely not” reproduce the observations one would ask:

Can the physics of the model really recreate those measurements, the phenomena you want to recreate? We know it can. All the physics are in this model. So it’s cool. Then there is always a question about the resolution of the model, the distance between the grid points in the model.

Then she continued to discuss the problems with the resolution in the simulation model that she was using and concluded that “But it’s for a research purpose. You can’t run it like this operationally to make a weather forecast. I use it as a numerical laboratory and try to understand things.” This statement illustrates several points. First, the physics of the model is primary in exploratory use, as I also noted in the previous chapter. Second, if the “physics” of the simulation model is good, it is acceptable from the point of view of representative modeling that performance is not completely in agreement with data. Third, it is assumed that the lack of agreement results from the features of the simulation models that surround this basic structure, such as the code or, as in this quotation, the resolution, rather than the “physics”.\textsuperscript{253} However, the quotation below exemplifies a different opinion.

\textsuperscript{251} Shackley and Wynne (1996) have also noted that climate modelers sometimes have been more concerned about the precise conditions surrounding measurements which challenge climate predictions rather than questioning the authority of climate models.

\textsuperscript{252} See also Chapter Six for a discussion about the problems with this regarding data assimilation.

\textsuperscript{253} This can be compared to Merz’ (1999: 301) observation about how physicists distinguish the “physics” of the simulation model (event generator) from the computer program. Merz suggests that this separation is needed in order to apprehend the object’s multiplex character. This can also be related to what Lakatos (1970: 133) refers to as the “hard core” of the research program, which is “irrefutable” and develops slowly through a preliminary process.
I know that I can study the problem I want to study with this model, but when you have sent in those articles, it has happened that someone has said that this you can’t do with that model. But then you have arguments: Yes I can do it with this model because of this and this. But everyone does not believe in models and you should be skeptical about what you get out of it. But if you can argue from the physics why you can use a model, it is okay.²⁵⁴

This quote exemplifies how the claim that “the physics” of the model is correct represents a cascade of inscriptions that single discrepancies or inscriptions cannot surpass (cf. Latour 1990).

In a social setting such as a seminar, it seems easy to question the agreement between the lines representing model output and the data in diagrams and figures. However, while the mutability of one inscription can be high, it is harder to question accepted theoretical statements and physical laws.²⁵⁵ These represent piles of inscriptions made much earlier in the production chain, some of which have been black-boxed and moved beyond the realm of scrutiny a long time ago. Consequently, by referring to “the physics” of the model, modelers refer to statements that single disagreements are unable to de-stabilize. In the previous chapter, I noted how attempts to connect simulation modeling to theoretical work serve the social function of increasing status. In this context, it becomes evident that these attempts also serve the function of creating a more stable network.

It is also interesting to note that the possibility to act as a successful dissenter is limited by the activities in the social worlds and the knowledge and skill they require (cf. Collins & Yearley 1992a). One scientist said that the experimentalists are lost “if the modeler starts writing some horrible formula that the measurement specialist perhaps remembers from studies ten of trial and error, and its protective belt, here exemplified by the resolution or the computer program (cf. Merz 1999). Compare also Kuhn’s (1962/1996) notion of paradigm. ²⁵⁴ Notable in this quotation is the use of “know” and “believe”: the modeler “knows” but some “do not believe”. Knowledge and belief are polemical modes of relations rather than psychological states, and to “believe” is thereby an accusation leveled at others (Latour 1999a: 271). We do not believe in scientific facts because they are true, but scientific facts are true because we believe in them (cf. Bloor 1976) and because networks are built to enforce them.

²⁵⁵ The interpretational flexibility of representations (see e.g. Amann & Knorr Cetina 1990; Woolgar 1990), also salient in the view concerning the comparative diagrams mentioned above, contradicts the view of inscriptions as immutable mobiles. The conception of inscriptions as immutable mobiles has been criticized for ignoring the idea that meanings change in different contexts and that it is therefore only the physical appearance that remains stable (Knorr Cetina 1995: 165, see also Knorr Cetina 1981). However, Latour (1987: 241f.) does in fact discuss the idea that no mobiles are completely immutable since, to be useful, they must be instantiated in and therefore adapted to a particular setting. Furthermore, Latour (1990: 41f.) admits that it may be possible to overestimate single inscriptions but not the setting where the cascades of more inscriptions are produced. This is because the cost of dissenting is higher for each collection of inscriptions. This is basically the same argument as the one I put forward above. The requirements of knowledge are different depending on whether the aim is to settle a local dispute or to extend a network (cf. Latour 1987: 253).
years ago.” Alternatively, if experimentalists “start talking about a sophisticated detector that makes a new type of measurements, then there are many who feel lost in the modeling camp” (interview with NN). It is therefore likely that the topic and character of critique depends on whether the dissenter is a member of the same social (sub)world or not and among whom the presentation takes place.

Recapitulating the tension between the results of simulations and the physical construction of models as discussed in the previous chapter, one modeler noted that “it is a large paradox that the [models] which are good at making weather forecasts are not good at making, what should you say, physically correct forecasts.” However, because opportunistic tuning of parameters and coefficients sometimes contribute to the better results of forecast models, “too much” agreement with data is actually likely to create suspicion rather than confidence, at least from the perspective of representative modeling. Consequently, because of the “common knowledge” that the most physical models are not the best forecast models (while more physics is said to produce better forecasts) reference to the tuning of operational models can be used to defend representative modeling, rather than to question it.

In one model comparison project using an idealized case, modelers compared different models with a detailed model of turbulent movements (a Large Eddy Simulation, LES) used as the “key” to the correct state of the atmosphere for the process in question. It was assumed that this produced the most truthful results, because it represented the “best fundamental understanding” of the process (Mauritsen 2004: 2). The theoretical properties of a model – its derivation from known physics – rather than general agreement with data, decided which model output, and consequently which simulation model, was considered best at reproducing real conditions in this case. Furthermore, the discrepancy between the LES and the other models was the largest in the case of what is generally considered as the most successful forecast model, i.e. in terms of agreement with observations.

Randall and Wielicki (1997: 405) give an example of this when they write “we often hear it said in seminars or informal conversations that ‘such and such a model has been tuned’ or ‘so and so must have tuned the model in ‘order to get such good agreement’.”

In (inter) comparison projects, modelers compare models in order to find out how to improve them. A common aim is to compare the performance of a specific parameterization and thus, these projects concern both use and development. They are based on a shared interest in improving models at the same time that they do not presuppose a joint effort in order to develop the models. In these projects, a simulation model is required in order to participate and in that sense, comparison projects resemble experimental collaboration. However, the goal is to produce similar results and not to share data in order to use it. Therefore, they also resemble quality test projects for monitoring instruments, where members of the technical staff meet in order to compare how well their instruments perform in relation to a common standard (cf. Mallard 1998.) The reasons behind these collaborations are therefore different from those that drives fieldwork collaborations, which appear as more of a necessity in the experimental line of work.
The modeler who told me about this project therefore considered these simulation models as inadequate for the case that the comparison project concentrated upon, but did not conclude that any simulation model was inadequate in general.\footnote{In terms of separate parameterizations, it is well known that the performance differs depending on the situation. For instance, one modeler said, "It is rare that a parameterization scheme is perfect for all situations (...). Most often they are good at certain conditions, perhaps really good, but then another situation occurs and then it is perhaps not at all as good, then another scheme is better."}

The possibility of the argument “the model is not good for this case” is a consequence of a detachment from the situations that shaped the basis of the computer model at an earlier stage in the history of scientific production. Every theory arises from situations (Star 1989a), but the argument “the model is not good for this case” shows that the early part of the translation chain has been black-boxed. Because the simulation model can be referred to as separate from a situation (a case), which is a consequence of its basis in theory, it has become an autonomous object, floating like a “flying saucer above the rest of science, which by contrast becomes experimental” (Latour 1987: 242). Thus, experimental work in this sense becomes representative of the local and peripheral in relation to the work of modelers at the center.

Nonetheless, it is important to consider that in order to produce reference, it must be possible to go back along the track that produced the account (Latour 1987; 1995) and follow how it was produced. From this point of view, turning to physics for legitimacy points towards the center of the center rather than attachment to the peripheral. This is a reason why it is still of importance to compare model output with data, i.e. a legitimate representation of the empirical world (“reality”) – in spite of modelers’ beliefs that the core of the model is irrefutable (cf. Lakatos 1970). Although it is not a primary concern in this chapter, it is also notable how turning to physics black-boxes the translation from theory and analytical models into a numerical framework. The argument reduces simulation models to theoretical constructions and uncouples modeling work from simulation models as such.

Problematizing the Comparison

In this section, I focus upon the comparison itself. When modelers have observations that they wish to compare with model output, they make diagrams to visually compare curves from data and output. They do this in addition to or rather than using statistical tools. Numbers have to agree at the same moment in time and correlation measures cannot deal with this satisfactorily.\footnote{One modeler mentioned the following: “Regular statistics will tell that two things are very similar, (if) with similar values at similar times. But in meteorology, two models can both be}
sections. First, I discuss the difficulty that modelers and experimentalists have in agreeing upon what constitutes agreement between simulation output and observations. The second part presents reflections and acknowledgments of the difficulties involved in model evaluation. As will become evident, these are crucial for my final conclusions.

(Im) possibility to Agree

According to modelers, experimentalists and modelers have different ideas about what constitutes acceptable and reasonable agreement with data as well as what can be expected from models in terms of this. The extended translation model does not talk of assent and dissent but rather of alignment and dispersion of translation networks (Callon 1995: 55). To speak of the closure of debate is to privilege the discursive (rather than the practical and material) dimension of science. Nonetheless, assent and dissent is still interesting from the point of view of social worlds and how their perspectives are constructed. For example, on one occasion a modeler showed me a diagram with two curves, representing the result of a model simulation and observations. He said that for him, the model agreed with the observations but added that an experimentalist would not be in agreement since the curves were not identical. According to this modeler, “people with no experience of modeling” complain that models are “bad” if they do not exactly reproduce observations, irrespective of how much they know about how models work and what a “good” result is.

One reason why modelers and experimentalists have different opinions about how well output and data agree can be traced to different foci regarding “details” and accuracy. Two examples from a workshop serve
to show how modelers concentrate on general patterns and larger trends while experimentalists are more preoccupied with details. These examples also illuminate the different perspectives of modelers and experimentalists and how these perspectives cause disagreement in situations when they meet.

The coordinator of a field campaign organized the workshop as a post-fieldwork collaboration in order to interpret data from the campaign. A few invited experimentalists attended as did a researcher – referred to as the modeler – who normally works with models but who had participated in the field campaign. Some other people from the department came to listen on a number of occasions as well. On one occasion, one of the participants showed the result of a simulation of air trajectories. This was depicted in diagrams and as lines on a map with the measured area in the center.\textsuperscript{262} One of the experimentalists started to develop an interpretation of measurements in relation to the trajectories based on a very precise understanding of model simulations. However, he was interrupted by two participating modelers who claimed that it was impossible to base interpretations on such a detailed reading of the modeled trajectories. On a different occasion, everyone was looking at a diagram based on measurements of temperature and humidity and the modeler said to one of the experimentalists “I would say that nothing is happening, but with your level of detail it’s actually an inversion”.

The following quotation from a modeler constitutes another example of how the subworlds internalize different expectations in terms of the comparison of data and model output – as well as from simulation models in general.

There has been and there still is the way that those who work with models, they only work with models. Those who work with measurements only work with measurements. The understanding between those groups has been a bit bad. If I write about a simulation of a case with a model and the reviewer is a measurement guy, then there is a very large chance that he will trash it and say that it doesn’t agree with reality like this and that. But that’s because you assume another… The ones who measure think that if you start from a model sub-, sub-, sub-routine, you change a parameterization of something which affects everything on top as well. A modeler can never disregard that, while someone who makes experiments can disregard this completely, does not need to take responsibility for it. The one who is doing the experiment must rather take responsibility for the detailed accuracy.” At the same time, models are seen as accurate in a relational sense. One modeler said, “If the model gives a value that is what the model has calculated. There can be \textit{errors} in the model but otherwise it is consistent with the rest of the values of the model. It gives \textit{that} answer. But there can be errors in measurements. There is a certain inaccuracy in all measurements. There is a certain level where you can start to measure and a small error \textit{can} exist in measurements all the time.”\textsuperscript{262} These are the types of simulations that are used in order to trace the origins of different air masses. The simulations are produced by models with somewhat different structures than those most commonly used by the modelers I have studied. It is beyond the scope of this argument to explain these in detail.
that can’t exactly reproduce reality, it isn’t a good model, but a model is never more than a model. It will never reproduce reality exactly.

This quotation also suggests that modelers perceive experimentalists as having unrealistic ideals about what models can accomplish. According to this modeler, experimentalists expect models to “exactly reproduce reality” (as they measure it), whereas modelers think they can learn and understand a lot about the real atmosphere by studying the performance of the model, even if models reproduce slightly different values compared to measurements. This suggests that from the perspective of experimental work, the model replaces the real atmosphere (the epistemic things in experimental practice) and as such, it should be a “copy” of it as they have measured it in order for it to be valid. I will return to this below.

More generally, I suggest that disagreement follows the boundaries of subworlds. People see things differently because of the perspectives rooted in their different activities and maintained in innumerable exchanges in the course of their professional lives. This is also consistent with the previous section about arguments in relation to data. Just as an experimentalist cannot judge what is a good model from a modeling point of view, modelers cannot judge what good data means from an experimental point of view. Thus, while appearing as (political) rhetoric to legitimize modeling, these are also practical thoughts based on the activities that different scientists are involved in and the perspectives that emerge from these. In other words, if one fails to consider the activities and practices in simulation modeling (as have been described in more detail in Chapter Six), many things of practical importance may seem to become just rhetorical.

**Complex Comparisons**

Above, I discussed problems concerning the comparison of output and data in terms of how to judge what constitutes an agreement. Another difficulty with comparison per se is that it is not always evident what is being compared. One modeler stated:

The problem when you make this kind of model comparisons is that in nature, everything happens all the time. All processes, at all scales, all the time. In the model, perhaps you want to study especially this, how this happens. Then you don’t want a low pressure to be on its way here because it simply disturbs. In the model, you can see that there is no low pressure coming, it is just the way it is, but in nature, when you have measured, perhaps there was a low pressure passing by and perhaps you didn’t check that completely because you never really measured that process. There are always processes in the atmosphere that you don’t have full control of that can affect what you are trying to study.
Consequently, important questions to consider when making comparisons concern the causes behind what modelers see happening in the modeled atmosphere on the one hand, and the measured atmosphere on the other. Do they compare the outcome of the same or of different processes? Do they compare the same “thing” at all? One researcher said that she and her colleagues who work with satellite measurements joked that comparing clouds detected by satellites and clouds produced by models is like comparing apples and pears. She said that they are both fruits but slightly different. A modeler also exemplified this when he reflected upon what is measured.

There are philosophical questions and completely practical ones too. What is a measurement point really representing? Is it a point you measure. (…) If you consider putting up a measurement instrument for wind on one side of the house it is obvious that if it blows on the other side, the measurements are not good. (…) But it is the same thing that makes it different when you measure chemical elements too. If you measure close to a road and there’s traffic pollution, then you will measure higher values there than what it is really… When you then put it in the model, it is perhaps one time one kilometer over two meters. That’s not what you measured over the road. So you can never say that you have precise correspondence to measurements. That’s usually difficult to understand: What the models say and what the measurements say and how to compare them in a reasonable way.

The tension between the different representations arises because there is an assumption that there is a real atmosphere that both measurements and models are trying to get at. The problem is how to represent it. But modeling is not about working with this atmosphere and the practical problem is to solve what a measurement point represents in the simulated atmosphere. The modeler quoted above said that measurements made in close proximity of a road give “higher values than it is really”. Really where? The volume in the simulated atmosphere is a consequence of the mutual adjustment of theoretical presuppositions and computer sources. Thus, a “representative” observation would be “representative” of the average value of this volume and produced for internal use in the modeling world. To be “representative” in this way has no meaning in relation to field experimental work – it is an internal modeling reference. I will raise related questions in the concluding remarks of this chapter.

Concerning the comparison more specifically, in one published article it has been argued that comparing model results to observations “is the best one can do at this point, despite the fact that its credibility is questionable. The measurements are taken at points, and the model calculates averaged values over a volume” (Svensson 1996: 957). This quotation clarifies that
models and measurements “classify” or map out the real world as volumes or as points respectively. This complicates comparison.\textsuperscript{263}

However, the fact that modelers and experimentalists agree upon making a comparison between the two forms of inscriptions in the first place, and thereby remain inside scientific networks (laboratory practices), is a basic presupposition from the perspective of network building (cf. Latour 1987). The disagreement between modelers and experimentalists about how well the forms they produce correspond becomes a small issue compared to this.

That science works outside and its predictions are fulfilled is a consequence of that they remain inside the networks that gave birth to them (...). The predictable character of technoscience is entirely dependent on its ability to spread networks further, if the outside is really encountered it would be chaos (Latour 1987: 248f.).

This does not contradict the fact that researchers discuss comparisons from the point of view of what they represent and thus, refer to the outside. It is in part by the production of similar categories (numbers) that simulations and measurements appear to circulate something constant through a series of translation, in other words, a reference (cf. Latour 1995: 246), even if these numbers refer to different social constructions of reality (points vs. volumes) as elaborated above.

Comparison as a Standard to Convince Others and Produce Reference

As has been mentioned above, simulations produce output divided into many parameters, all of which should ultimately be compared to data. This makes a complete model evaluation a much more complex task than, for example, calibrating an instrument. Nevertheless, there are further similarities between the comparison of model output and data and the calibration of instruments than the ones discussed in the introduction to this chapter.\textsuperscript{264} Calibration is a standard procedure to ensure that measuring instruments function correctly.\textsuperscript{265}

\textsuperscript{263} Winsberg (1999: 291) discuss a similar issue but “adopts” the modeling perspective in this question.

\textsuperscript{264} It is important to note that calibration of the simulation model here refers to the comparison of model output with what is seen as an authoritative source, in this case measurements. This has nothing to do with what Randall and Wielicki (1997: 403) discuss when they note that modelers sometimes say they calibrate their models. Calibration in that instance refers to the same thing as tuning, except that tuning has negative connotations while calibration has positive connotations.

\textsuperscript{265} Instruments are calibrated as a means to control that the measurements are consistent with standard measurements. This type of quality control is important in order to create trust for the results among other scientists and one example of this are articles where the calibration of
There is no principal difference between instruments and models in terms of how practitioners should follow, or at least appear to follow, what is considered as appropriate methodological rules and standards relating to measuring or modeling (cf. Landström 1998). An experimentalist claimed that calculations were indeed enough to check his instrument, but added, “If you calculate, nobody is happy”. If he used standard measurements to calibrate the instrument, the most common form of calibration in the field, “everybody is happy”. This implies that there can be different ideas about the importance of standards among those who do or do not work with instruments.

In terms of comparisons of model output and data, there seem to be differences in how modelers and experimentalists define the situation and what they expect from it, as mentioned above. For experimentalists, correspondence between model output and data seems to be the most important criteria in order to accept simulation models as valid. For instance, a modeler said the following regarding often asked questions during seminars and presentations of modeling work:

A question that you often get if you have made a model is “have you compared it to observations?” How does it look like compared to reality? (…) Everyone could ask that, but primarily those who have been out and measured ask that question. But a good modeler probably asks that question too, because a model can be as refined as anything but if it does not describe reality, it’s worthless anyway.

This is not evidence of a lack of concern among modelers about comparing results with data, but it is interesting to note how this modeler experiences that experimentalists do focus more attention on this compared to modelers. This can be related to the suggestion that any scientific practice demands a viewpoint: you need to believe in it and as you believe in it, you do it. Thus, it is primarily in relation to and in contact with experimentalists that the question of whether simulation models are truly representative of atmospheric processes arises, not only if the simulation model is more or less useful (cf. Sismondo 1999: 247). This suggests how a shared perspective

new instruments is the main topic. Together with data quality assurance, which is a part of post-fieldwork before interpretation can begin, these are the most important measures used in controlling the quality of observations.

Another reason for this is probably that modelers are interested in many aspects of modeling, including the correspondence between output and data, whereas this specific feature is one of few that experimentalists can relate to. See also Boumans (1999) for a discussion about built-in-justification in models.

It is also possible that the modelers I have talked to and interviewed hold an exaggerated view of experimentalists’ skepticism. In any case, the skepticism towards simulation models does not prevent experimentalists from motivating their research as contributing to models, as the next chapter shows. In this, the level of detail inside the simulation models seems to be more at stake. Thus, resistance may be pragmatic and it is clear that it would not be beneficial
arises from the common activities participants engage in. In this case the basic credibility of models is not an issue for modelers (cf. Shackley & Wynne 1996: 285) inside their subworld. For experimentalists, real atmospheric objects are epistemic things and these are what simulation models substitute. When they seek to look at similar features in simulation models they judge these according to what they know about the epistemic thing through their use of measuring instruments as technical artifacts, i.e. observations. It is an outsiders’ perspective. As experimentalists and modelers share the abstract aim of understanding weather and climate, it is nevertheless important for modelers to convince experimentalists (and others) that their models are adequate descriptions of the atmosphere.

According to Latour (1999a: 102f.), scientists need someone to convince in order to build alliances and extend networks, but they also need data. Why do they need data? By using instruments and equipment in field campaigns, experimentalists transform atmospheric processes into fixed categories – they mobilize the world (cf. Latour 1999a: 99f.). The important point here is that modelers also require data in order to claim that for instance modeled, simulated cloud formation can tell us something about real cloud formation, i.e. as transformed and mobilized through experimental practice (cf. Mol 1999; Latour 1998; 1999a). Data thereby becomes a source of authority in relation to model output. Modelers’ arguments in relation to the principle of comparing model output with data can therefore be seen as an effect of the circumstance that comparison with data has become a standard means to create a credible network in modeling (cf. Mallard 1998: 577). The arguments exemplify detours in relation to the falsification principle as a legitimating criterion from the point of view of science as an epistemological enterprise (cf. Latour 2003a).

Latour (1999a; 2003a) views scientists as captured by philosophical views and draws a sharp line between, on the one hand, the (political) rhetoric of unitary Science, expressed by scientists and asserted by (rationalist) philosophy, and on the other hand, the practice of science and scientists. However, this discussion suggests that scientists also make use of philosophy and that it is a way for them to understand their own practice (cf. Fuller 2000: 342). Perhaps of more importance is that the legitimating for experimentalists to refuse simulation modeling altogether. It is more likely that resistance is situational and related to different views on simulation models.

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268 This resembles Mallard’s (1998) analysis of a test (trial of reference) between representatives of metrology (the science of measuring) and experimental research, which involved a comparison of a source of authority (metrology) with what should be evaluated (a measurement technology for air-quality). The challenged researchers claimed that the discrepancies came from imperfections of the test, not from a faulty instrument. For instance, they suggested that the test did not really control for experimental conditions and another objection challenged the strategy of purification, which is constitutive of metrological evaluation in general. The process of evaluation led to doubts about the correspondence between different instantiations of the “reality” that the instrument was supposed to measure.
criterion appears as being related to a non-social legitimacy, independent from people (cf. Latour 1999a). Yet, the creation of reference, the scientific way to convince (“letting the facts speak for themselves”) is in fact how allies outside modeling come to be recruited in support of these practices. This is a core aspect in establishing modeling as a scientific practice because it is by means of this activity – comparing data and output – that modelers show how the alternative realities of models apply to the “real” (measured) atmosphere, not only for themselves but also for others.

Concluding Remarks

I think it is very important that you do (...) not only stay in your model world, because that is very easy to do, ha ha! It’s so simple. Well, it’s not so simple but it is easier to understand than data sometimes. (Modeler)

This chapter has offered a sociological analysis of the relationship between different scientific practices by presenting stories about the comparison of models and data, including an analysis of how modelers and experimentalists relate to each other and each other’s work. By approaching the stories with a flexible analytical stance, I suggest that these stories relate to questions of legitimacy in different ways.

From a social world view, the stories reveal a practice perspective in representative simulation modeling, both in relation to field experimentation and to simulation modeling as such. In relation to the falsification principle, model evaluation shares features with legitimation processes, which govern the manner in which various lines of actions are evaluated and accepted or rejected (Gerson 1983: 360). In this case, the modelers’ argumentation can be seen as an activity directed towards gaining social legitimacy for the modeling world. Thereby it involves the social construction of the sub-world of representative modeling itself in relation to field experimentation. However, it is also a part of how simulation modelers view and think of model evaluation, a part of their practical perspectives (cf. Becker et al. 1961).

With an actor-network perspective, I see comparison of output and data as a standard way of circulating reference in simulation modeling. More specifically, the chapter highlights a particular “hutch” or part in the chain of simulation modeling and how it involves a constantly shifting ground, either pointing at the problems with data or the strengths of theoretical models or the difficulties in comparing the two different scientific products. Comparing model output and data is in itself a translation with the purpose of not only making two scientific inscriptions appear equivalent, but ultimately making what they represent equivalent. While these inscriptions are similar in form,
they are produced by different translation practices and I suggest that fundamentally, the point of comparing them is to show how they nevertheless *represent the same thing* (cf. Latour 2003b). As such, this translation forms a crucial part of the circulation of reference in the subworld of simulation modeling and in field experimentation, and therefore in the social world of meteorology more generally. The creation of reference may also be seen as a particularly important way of gaining legitimacy in a *science* world – a type of social world which also has its own special features.

However, it is important to acknowledge that these conclusions also rest on different ways of analyzing the researchers’ narratives, as descriptions or perspectives or as rhetoric. To draw the comparison of social world theory and actor-network approach even further, I will return to the question of *what* is actually compared in model evaluation – in other words what it is that experimentation and modeling produces – in relation to constructivism. As Knorr Cetina has pointed out (1993: 557), what is discussed in terms of constructivism is not whether one recognizes the pre-existence of an (unknown) material world (this is taken for granted), but whether one assumes the preexistence of specific objects before they have been delimited and defined by science in a particular way. According to Annemarie Mol (1999: 75ff.), constructivism involves plurality projected *back into the past* in its acknowledgement of the *possibility* of alternative constructions of reality. Thus, what Mol (1999; 2002) adds is the suggestion that if different scientific practices delimit specific objects in different ways, they perform *different* objects. Mol (1999) thereby seeks to address questions of *ontologies* (see also Latour 1999a: 113ff.) and reality itself as multiple.269

From the viewpoint of multiple realities, the practices of modeling and experimentation enact the atmosphere as a multiple object and access to (objective) reality becomes an ordinary event (cf. Mol 2002:179). Noortje Marres (2004) suggests that Mol takes up the pragmatist notion of “being in the world” where humans are embedded in the world rather than locked outside reality, but since pragmatists (e.g. Dewey 1922; 1925) developed this to account for the human subject, the understanding of objective reality is unaffected. According to Marres (2004), the embeddedness of the human subject applies both to experiential and objective reality in Mol’s (2002) extension of the argument and reality is to some extent reified. At the same time, Mol (2002) does not re-affirm that reality is subject to human choice.

Furthermore, the notion of practice appears to be *decoupled* from perspective in Mol’s (2002: 10ff.) account. Schatzki (2001: 2) suggests a general view of practice as arrays of (human) activities, materially mediated and organized around shared practical understanding (i.e. perspective).

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269 This is also an example of how these types of questions have been attended to in later work influenced by the actor-network approach.
Decoupling practice from (human) perspective follows from the actor-network understanding of action as (reduced to) behavior, which is necessary in order for the actor-network approach to study humans and nonhumans symmetrically.

However, in social world theory, the different perspectives among the (human) practitioners are intimately linked to their practices. These perspectives are not simple viewpoints that can change during a discussion. They have important consequences for scientific production. In my view, the social world framework leads to the conclusion that different practices produce different social objects (including sociomaterial objects) in the sense of perspectives, but it avoids questions related to the ontological status of the material world (cf. Hacking 1999: 169), as mentioned above. From this perspective, the fruitfulness of moving the discussion from the epistemological to the ontological level is questionable. Further, if one retains an interest in the view of actors, one may also ask for whom reality is multiple since scientists surely do not recognize this multiplicity. In fact, to compare output and data rather appears as a way to establish the (social) existence of one (objective) atmosphere.

To conclude, I wish to note that while modelers strive to connect and relate simulations to the material reality of the atmosphere, simulation modeling produces a form of (social) reality – this view is consistent with the discussion above. Although comparison of data and output is part of what proves simulation modeling “scientific” and essential in distinguishing modeling from “numerical games”, the practical usefulness of simulation models is a consequence of the fact that they represent a form of computer “game”. Simulations bring a mathematical order where before there was only seemingly random detail (Winsberg 1999: 286). Simulation models thereby create a world of mathematical order when simulating theoretical realities, i.e. ordered versions of reality rather than the messy world of (measured) waves or wind.270

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270 In a paper on computer simulations in physics, Galison (1996: 144) quotes from a chemist: “classical mathematics is only a tool for engineers and physicists and is not inherent in reality.” Yet, Lynch (1990: 169) suggests that illustrations in scientific texts create the impression that the objects they represent are inherently mathematical.
One of the problems involved in model evaluation, which was the focus of the previous chapter, is that at least from the viewpoint of modelers themselves, experimental researchers do not supply modelers with the data they need. Modelers also need data to construct parameterizations, since all physical parameterizations have some empirical components. This intersection between the two worlds is the topic of this chapter.

There are many similarities between using data for evaluation and parameterizations concerning how modelers conceive of the experimentalists’ attempts to contribute to the integration of models and data. The tensions between modelers and experimentalists frequently arise both in informal conversation and in articles in academic journals. For example, Randall et al. (2003) write about the difficulties involved in integrating data and models and list a number of problems that modelers face when they need observations (Randall et al. 2003: 465). First, it is difficult for modelers to find data that suit their purposes. Second, they are generally neither capable of, nor interested in, dealing with sampling bias or random errors. Third, modelers lack the expertise to make use of raw measurement data.

An essay written by two atmospheric scientists about the relationship between models and measurements in the atmospheric sciences summarizes the situation – as well as the general concerns of modelers and experimentalists.

Many kinds of data can be used with SCMs and CEMs,271 but first the data must be suitably reduced. It is not realistic to expect modelers to deal with raw radar data or raw satellite data or raw aircraft data: with few exceptions, modelers lack the expertise to perform such tasks and, in any case, most modelers would rather spend their time modeling than reducing and quality controlling someone else’s dataset. These are facts of life. It follows that modelers rely on observationalists272 to manipulate measurements into a ‘ready-to-eat’ form that the modelers can easily use. It is also a fact of life, however, that most observationalists would much prefer to spend their time

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271 These types of models are well suited for testing cloud parameterizations against field data according to the authors of the article (Randall & Wielicki 1997).

272 Referred to as experimentalists in this thesis.
collecting more data, devising better measurement techniques, etc., rather than preparing reduced products for the convenience of modelers. (Randall & Wielicki 1997: 405)

In this quote, the different concerns of modelers and experimentalists are presented as inevitable “facts of life”. The situation appears as “natural” taking into consideration that modelers and experimentalists form two subworlds with different objectives. Why would experimentalists care about the needs of modelers? However, the incentives for experimentalists to produce data that is not only useful from the perspective of experimental work, but also from a modeling perspective, are not discussed in the article cited above or in Chapter Seven.

Two general suggestions serve as points of departure in attempting to address this question in more depth. First, different objectives in terms of working practices do not automatically exclude shared interests or commitments. There may also be historical and other reasons for maintaining some sense of community. Indeed, different groups within a production world are normally seen as complementing each other, e.g. doctors and nurses etc. In addition, an arena may form where social worlds can meet (Strauss 1978b; cf. Clarke 1990), in spite of differences. Second, translating the interests of others into ones own is an important strategy used by scientists (e.g. Callon 1986; Latour 1987). A social world can translate the interests of another social world into its own and thereby change the goals of both (cf. Latour 1994). These two statements are of central importance in this chapter, where I will argue that climate change is an arena where negotiations about the relationship between modelers and experimentalists take place and where parameterizations play an important role.

Climate Change as an Arena

A unification of meteorology took place during the 20th century (Nebecker 1995), but numerical weather forecasting does not include all types of research at MISU. It mainly concerns dynamic meteorological modeling. However, the historical description in Chapter Four revealed that researchers at the department have been active in debates about three established environmental problems: acid rain, ozone depletion, and climate change – sometimes on both national and international levels. A senior researcher noted,

All the large problems [acidification, ozone depletion, climate change] partly emanate from this department. Not so much the climate problematic except for the basics of the carbon cycle but model development and weather forecasting are very important for climate [research].

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Whereas acid rain was a concern in chemical meteorology and ozone depletion a question dealt with at the Section of Atmospheric Physics, the engagement in climate change has become more widely spread. Before turning to the current situation, a brief discussion of past developments is worthwhile.

Unlike the situation in a number of other countries in the 1990s, climate research in Sweden has not been formally coordinated (Nolin 1999).\(^{273}\) One possible reason for this is that MISU was the center of climate research in Sweden when climate research expanded in the 1990s and in a sense had the possibility to co-ordinate research itself (Nolin 1999: 134).\(^{274}\) However, the question relating to the extent that they did so remains unanswered, as does the question concerning the situation today.

Most researchers at the department emphasize that the future climate is one of the most important areas of research.\(^{275}\) There are also (still) several members of the department who participate in international programs primarily directed towards climate issues. Nonetheless, a researcher commented upon the position of climate research in Sweden and at MISU today in the following way:

> If you look at how much climate research today that’s made in the sense of actually studying how the climate changes and why it changes, it’s very modest and that’s not because there’s a lack of competence but it is because the research is so fragmented.

What I would like to emphasize is the opinion that a “poor” situation is a result of fragmented research. This is also in line what an experimentalist said about the current situation at the department.

MISU itself has this incredible potential for interdisciplinary work that is very broad and necessary and important in that stage, particularly in climate

\(^{273}\) Although co-ordination of Swedish climate research has mainly been absent, the Swedish regional climate program (SWECLIM) is an exception to this rule. A few of the scientists at MISU were part of the initiating group of researchers together with others from SMHI, Göteborg University, and Uppsala University. SWECLIM was initiated in 1995 and lasted for eight years. Most of the work took place at the Rossby center, situated in the same building as SMHI, and part of the activities that started during the program have since become permanent. The program was directed towards climate modeling and climate scenarios and involved close co-operation mainly between modelers from MISU and SMHI.

\(^{274}\) This lack of co-ordination can be compared to the development of ozone monitoring as a research area, where research seems to have been successfully co-ordinated (Nolin 1995).

\(^{275}\) However, researchers at the section of meteorology at Uppsala University (MIUU) have showed less interest in climate related questions. This is notable in current research applications (2001-2003). See also Elzinga and Nolin’s (1998) report on Swedish climate research and politics. Consequently, there is a difference between these departments in terms of their engagement in climate. Institutional affiliations may affect the inclination and possibility to frame research (Shackley 2001). I abstain from speculating about the differences in this case since it is not documented in my material and institutional matters are beyond the scope of my theoretical focus.
Many researchers point out that more cooperation between experimentalists and modelers would improve meteorological research, especially climate research, and the two quotations above are also examples that indicate the existence of good opportunities for important climate research to be carried out at MISU. Climate change can be seen as an arena where several social worlds, including modelers and experimentalists in meteorological research, meet (cf. Strauss 1978a). Consequently, studying climate change research is a potentially fruitful way to analyze the relationship between experimental work and modeling as different social subworlds.

Modelers and experimentalists in meteorological research are not the only subworlds engaged in the climate change issue as an arena. Shackley et al. (1998: 190) discuss relations between climate modelers, the surrounding sciences, the climate impact community and the policy community, and note the potential of climate models to unite several areas of environmental science. This includes meteorologists and climatologists who have specialized in handling data bases, observational scientists (experimentalists) who have either linked up with model groups or pinned the rationale for data collection upon the need for improved climate models, and satellite agencies. Without showing how this works in more detail, Shackley et al. (1998) refer to this as an example of a translation process, where the interests of the other scientists have been translated into those of climate modelers.

To delimit studies of climate research to climate modelers, while neglecting to study the scientists who supposedly have been translated, can also be seen as a result of translation itself where a “few obtain the right to express and to represent the many silent actors of the social and natural worlds they have mobilized” (Callon 1985: 224). However, Joan Fujimura (1991: 222) emphasizes that it is important to examine all the different participants in scientific work – including human winners and losers and nonhumans – and to understand why and how some human actors follow the will of other actors (and why and how some actors resist being enrolled). In other words, studies of local practice and small-scale climate research are also required in order to understand if, and in that case how, climate modeling has translated interests.

What is Climate Research?

Recapitulating from Chapter Four, climate models have developed from numerical weather prediction models and the structure of climate models and NWP models is therefore very similar. As Edwards (1999) notes, the
differences between them lie in how they are used, not in their structure. However, because climate models try to predict weather on a longer time scale, more descriptions of processes have to be included (see e.g. Holton 1992: 359). Effects of processes are simplified and represented as relations between simulated parameters and modelers often use data in order to determine coefficients in these parameterizations.

Edwards (2001) suggests that climate modeling has become an *obligatory point of passage* for truth claims about future global warming (cf. e.g. Latour 1987; Callon 1986) and that it is a fundamental organizing principle for the global epistemic community that surrounds the climate change issue. Without a model of what would be the natural development of climate, scientists cannot separate the effects of rising concentrations of greenhouse gases from natural climate variability and according to Edwards (2001), climate models are the only available tools that can predict the magnitude of global climate change. They are also crucial in order to study and test which processes are of importance in understanding and predicting climate change. However, all scientists who claim to perform climate research and call themselves climate researchers are not working with climate models. On the contrary, a researcher suggested that anything goes under the name of climate nowadays.

You see that it was over the last decades that it became a fashion. Climate is a fashion right now and all of a sudden everything between heaven and earth is climate and, consciously or unconsciously, the researchers want their research to have money. (...) There are loads of things that are presented as climate with a more diffuse connection. (...) During the 1970s it was acidification. Then people studied how the wind was blowing and called it acidification research. Now people study how the wind is blowing and it is called climate research.

A senior researcher at MISU commented upon this development in a similar way:

The climate question, it has affected very much, that’s for certain, perhaps sometimes almost a bit too much. The climate question is incredibly central and important. It is the most important question for us to study, of course it’s like that, but now it has in a way become so hot that everyone tries to jump

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276 However, Edwards (2001) does not mention that this way of problematizing climate change is not given (and has changed [Miller 2004]). Climate models have been established as the crucial route towards cumulative knowledge about climate change, primarily because of the way meteorological researchers have participated in the construction of the question (cf. Elzinga & Nolin 1998).

277 The hierarchy of research disciplines participating in the climate arena is a hypothesis of this thesis, assuming that meteorology has the highest position. Thus, while social scientific climate research for instance investigates how society can or should adapt to climate change and does not actively use climate models to produce knowledge, it is likely to start from the scenarios the models produce.
on it and work with climate related questions and use climate as some kind of keyword to get attention and resources and those kinds of things.

This researcher noted that the climate question has affected research very much and that “everyone” tries to work with climate related issues. I would like to make two comments related to this. First, I argue that what constitutes “climate research” or “climate-related research” (and what does not) are questions of negotiation in the arena and a matter of boundary-work (cf. Gieryn 1999). Second, researchers participate in this construction, including the researchers who “try to” “deal with climate related questions”.

Translations can make things relevant (Latour 1987: 126) but this does not mean that some things are relevant and that others are made to be. It is rather a question of how something comes to appear as natural. In this case, I suggest that the centrality of climate modeling has an appearance of naturalness, not of construction, due to the stabilization and establishment of climate modeling as an obligatory point of passage. Relevance has always been made at some point in time and while this chapter does not attempt to answer how this took place, it seeks to show how contemporary researchers relate to this in order to produce “climate research”.

The meaning of climate research has changed historically, not least since the question of climate change became a “hot” topic (cf. Nolin 1999: 128f.; Miller 2004). Researchers also have different ideas about what should be included in what is called climate research. For some – especially modelers – having a relation to climate models is essential. One modeler also notes the close relation between weather and climate “forecasting”:

About 25, 30 years ago, there was a pretty strong development of weather forecasts, but that has come a bit more in the background with this thing about climate, which has become The Big Thing. But it is also about motivating meteorological research for grant agencies and about why it is important and then you must relate to what’s important for the public. Then climate is a big thing and weather forecasts have been such a thing. (…) There are many research problems that are based in both climate and weather forecasts. Today it is perhaps a tendency to use the climate label for things that are mostly about forecasting since it is easier to sell climate rather than forecasts.

This respondent suggests a rather loose boundary between the research topics including a tendency to frame them differently according to funding opportunities – a question I will return to shortly.

Others emphasize that it is too narrow to focus on climate modeling exclusively. One experimentalist said that since clouds are one of the most important parts of the system that affect the climate, the development of clouds becomes a part of climate research. Another experimentalist noted that any research about particles can be related to climate, but he also added
that it is really a matter of where to draw the boundary. The quotation below exemplifies various ways that the climate label is currently interpreted.

A lot of the things we do are climate research, but that depends a bit on how you define climate research. With a wide concept of climate research, then everyone in the whole IMI-report is doing climate research. If you take climate research as *climate* research, then you perhaps look more at climate zones, temperature scenarios in Sweden for instance. (…) Looking at climatological parameters – that’s perhaps climate research in a more conservative view. (…) [Climate research for me] includes understanding the processes regulating the climate and understanding the radiation balance of earth; what is hindering the incoming radiation, what hinders outgoing radiation to go to space?

The respondent mentioned a number of different ideas about what climate research is and thereby suggests that researchers disagree about what is included in the term “climate research”. Consequently, to define climate research *a priori* in this chapter would be to take sides with some researchers while declaring others as only “seeking attention and resources”.

A less normative and more symmetrical way is thus to investigate how climate relevance is created (and negotiated). How is climate research constructed and what is the role of climate models in this?

**Translating Climate Research**

In order to study how researchers negotiate the meaning of climate research, a site where this takes place has to be chosen. Above, I cited a researcher who argued that climate is used as a keyword to attract resources. Since most Swedish climate change research has not been funded through a special program, it has been necessary, and still is, for researchers to apply for new, climate-marked money in the “old way” (Nolin 1999: 129). At the same time, the “climate research” label has changed over the years. Consequently, grant proposals can be data sources for analyzing how “climate research” is constructed.

The elaboration of research problems in funding applications has been discussed in previous work. Knorr Cetina (1982: 123f.) argues that a scientist who realigns research to match the orientation of a project or funding allowance influences the outcome of research. “Problems” – such as climate change – can appear as “external” input to scientific work. Accordingly, when funds are earmarked for certain problem areas scientists are encouraged to choose problems of general concern. Hence proposals

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278 This is also in accordance with the symmetry principle (Bloor 1976). See Chapter One.
279 Scientists’ *transepistemic* involvements are the locus in which the decisions invoked by laboratory selections are defined, revised and negotiated, in connection with negotiations over
consist of chains of problem translations, which start from a definition of purpose and continue through fine-grained elaborations of method, source material and processes.

This is similar to so-called funnels of interest; a series of problematizations that starts with a problem, or interest, and then translates the reader to a specific problem-solution (Callon et al. 1986: xcvi). As noted earlier, examples of this are often found in introductions to scientific papers but Knorr Cetina (1982) suggests that this idea also applies to grant proposals. It is through these elaborations that financing agencies and scientists negotiate what the problem is and also how it is to be translated into actual research tasks. Most importantly, to refer to research problems as “external” input to science is to ignore the process of elaboration, which penetrates into the core of scientific work (Knorr Cetina 1982). This makes it more appropriate to study climate problematization as an (discursive) activity, in line with the idea that the “climate research” is still in the making.

Consequently, funding proposals are good sources for studying the translation of problems such as climate change. The point here is to show how meteorological researchers problematize, as a form of translation, in order to construct and negotiate the meaning of climate research. The next section discusses how researchers present their research in relation to the climate change question. Primarily, it draws upon research applications but also the discursive practices surrounding them.

**Problematising Research**

First, I will briefly recall the position of the IPCC, discussed in Chapter Four. By establishing itself as a credible and scientific basis for knowledge about climate change, IPCC has become a major authority in terms of climate questions and a fruitful reference for researchers. Other studies have more or less explicitly shown how the IPCC functions as a mediator or knowledge broker (Miller 2004; Mattson 2005; Skovdin 1999), i.e. as an intermediary between the producers of knowledge and the policymakers (cf.

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the resources at stake in various relationships (Knorr Cetina 1982). The notion of transepistemic arena is an attempt to dissolve the distinction between epistemic and non-epistemic factors, and consequently also between the inside and outside of science (Knorr Cetina 1981; 1982).

280 In this case it is important also because meteorological researchers participated in the early problematization of climate change. Problematization involves both the definition of a problem and how to approach it. Meteorological researchers approach the climate change problem primarily as an environmental problem and try to study its causes. For instance, meteorological researchers can black-box the importance of climate models; they have already become crucial features of climate change research.

281 Grant applications are public, but in order to protect the anonymity of the researchers at MISU I will not include the full reference to the applications I cite from. See Chapter Three for a full description of the material I have used.
Liftin 1994). However, while this function is well-established in terms of how policy-makers consume scientific knowledge, less is known about the relation between IPCC and the researchers.

The researchers’ accounts and the funding applications confirm the mediating role of the IPCC. Since the IPCC reports include sections directed towards the scientific audience as well as sections and summaries more intended for executives, researchers have a positive view of the function of the IPCC reports as a communication channel to politicians and funding agencies. The following statement from a researcher illuminates this: “You can call it a communication surface between the ones who do research and the ones who have money. Of course it’s good with a bouncing surface, a report to lean against.” The IPCC reports have been very useful for researchers in this respect since they represent a credible source to rely on in motivating research. One researcher asserted that “A reason to write the IPCC reports from the beginning was obviously to get more money to that type of research and that question [human induced climate change].”

In translating the climate question into a favorable direction for atmospheric scientists in general and meteorological modelers in particular, the IPCC has already done a large part of the job (cf. Miller 2004). In other words, the relevance of this type of research has been established before the researchers appeal to it in their applications (cf. Latour 1987). The analysis in this chapter concentrates on how the arguments of the IPCC are used in motivating current research.

In the following, I present the two primary entrances to the climate question that are used by researchers: reference to the Arctic region and reference to the effects of aerosols (these can also be mixed). I also discuss the differences between experimental and modeling proposals.

The Arctic Region

The Arctic is a sensitive environment that has been identified as an important region for the study of climate change. The importance of IPCC was discussed above and the following excerpt from an interview illustrates this as well as the interest in the Arctic region.

[IPCC] is excellent for funding. [The IPCC report] shows that we have the largest uncertainties in the climate simulations in the Arctic basin. Why? Then I can go on [in the application] why this is why we have to do this research. That’s why we need money. That’s a very good angle of approach then. It’s clear that it is a support.

282 As Landström (1998) notes, many of the classical actor-network studies focused on new areas of research, but like in her study of molecular biology, some of the aspects of how climate change has been problematized have already been stabilized, such as for instance the role of the IPCC in determining major uncertainties in the current understanding of the causes behind and magnitude of climate change.
It is also interesting to mention the recently published Arctic Climate Impact Assessment (ACIA), a project of the intergovernmental forum Arctic Council and the non-governmental organization International Arctic Science Committee.\textsuperscript{283} Several scientists from Sweden, including two modelers from MISU, participated in this assessment.

Several proposals, involving both modeling and measurement components, point at the uncertainties involved in modeling the Arctic climate. In one proposal, the applicant writes: “Detailed modeling of the Arctic is a relatively new field which has risen out of the realization that the development of the climate in the Arctic may be critical to a large part of the northern hemisphere” (Application 2002 5610, p. 6). The poles play a crucial role for large-scale atmospheric movement, but their remoteness is likely to have decreased the interest in developing numerical weather prediction in these areas. The lack of knowledge about other special features of the atmosphere in these regions (such as the boundary layer structure) is perhaps also a result of this situation.

Climate model projections for the assumed greenhouse gas increases indicate that the largest impact in absolute temperature will be found in high latitudes (Räisänen 2001) and that the primary indicator of climate change in Arctic modeling is the melting of sea-ice (Tjernström et al. 2003: 2). Modeling projects are therefore motivated by the fact that climate models are “poor” at simulating Arctic climate. This conclusion arises partly from the fact that the results of model simulations of the Arctic climate have larger discrepancies than simulations for other areas. This is considered a weakness since it is assumed that the more similar the results are the better.\textsuperscript{284} This is also an assumption behind inter-comparison projects, such as the Arctic Regional Climate model Inter-comparison Project (ARCMIP), where some researchers at MISU are involved in both identifying model deficiencies and improving the description of Arctic climate processes in numerical models. Since this involves comparing model output with data, which I have discussed elsewhere, I will not further consider it in the current context.

There are other ways of connecting Arctic-studies to climate. Modeling proposals present the development of new or better parameterizations for (regional) climate models as an aim, in order to improve model representation of the Arctic atmosphere, for instance in the following way:

The main motivation is to improve global climate models in regard both to their description of the Arctic boundary layer dynamics and for predicting aerosol concentrations in the Arctic region. The Arctic region is both subject

\textsuperscript{283} It is possible that future Arctic-studies will refer to this report as opposed to IPCC, but the report was still in the making during the time of my field study.
\textsuperscript{284} The same assumption lies behind the “conclusion” that the result of the first simulations of the effect of increased amounts of carbon dioxide was impressive because of how closely it corresponded to current climate simulations (Elzinga & Nolin 1998).
Experimental projects also emphasize the uncertainties involved in model simulations of Arctic climate, and stress the importance of measurements in achieving a better understanding of Arctic conditions.

This proposal requests funding for the development of new techniques required to understand more fully the importance of the interaction of organic and inorganic constituents of the aerosol and their effects on the formation of cloud drops in the Arctic, which is an area of great sensitivity to climate change (Application 2003 3085, p. 1).

Another issue – besides the motivations in applications – is worth noting with regard to the interest that researchers at MISU show in the Arctic region. In each of the last two biennial reports (2001–2002, 2003–2004) from IMI/MISU, a special section is devoted to Arctic studies in relation to climate (in addition to the chapters on the three sections). This can be seen as an attempt to call attention to the Arctic and its importance from a climate point of view. At the same time, it signals a shared interest among members of different sections at MISU – including both modelers and experimentalists – that is important from the point of view of the climate question. The interest in Arctic studies is also the reason behind one of very few cross-sectional collaborative projects at MISU.

This project is primarily experimental and based on a field expedition where measurements specifically gathered for modeling purposes were also included. Nevertheless, one of the participating modelers complained that more of these measurements would have been helpful. This exemplifies that while the Arctic region creates a more delimited space of interest, it does not itself reduce the distance between modelers and experimentalists in terms of their different working practices. It can thus be characterized as a boundary object with coincident boundaries (cf. Star & Griesemer 1989: 410f.). The Arctic functions as a boundary object in the relation between modelers, experimentalists and also funding agencies, but it only partly serves to facilitate cooperation because it leaves several collaborative problems unsolved. These are related to the differences discussed in the previous chapter. Therefore, more specified problem descriptions can further facilitate collaboration (in some ways), which I will discuss in the following.

**The Aerosol Effects on Climate**

The representation and understanding of aerosol and cloud processes have been recognized by the IPCC as a major uncertainty in climate models. The
importance of reducing this uncertainty originates in the idea of aerosol effects, which is explained in the following way in one of the proposals:

Aerosols affects climate directly by scattering back or absorbing incoming solar radiation, and indirectly, by the formation of condensation nuclei (CCN), which influences clouds microphysical properties and consequently their optical properties and life times (Application 2003 3445, Appendix A, p.1).

This passage is quoted from a specific proposal but all introductions in proposals involving aerosol and cloud measurements include variations of this statement regarding the effects aerosols have on climate. On this basis, a chain of new statements leads to the specific research problem that the researcher or group of researchers intends to focus on.

Three categories of these chains can be distinguished from the point of view of how they link research to climate models. All research projects aim to solve the weaknesses of climate models by improving the understanding of certain processes, but how new knowledge will or can be used in climate models is sometimes only implicitly stated. It is as if improved understanding per se improves models. The extract below exemplifies this situation.

The IPCC has repeatedly identified aerosols and clouds to be the key uncertainties in our present ability to model the Earth’s climate system. The individual droplets in a cloud all started their lives as aerosol particles. (...) Understanding the processes that determine cloud properties (...) requires understanding which particles, out of all those that are present in the atmosphere, actually form cloud droplets (Application 2001 2484, p.2, my emphasis).

If the quote above illustrates the first category, the second category maintains that the findings can be used to develop a quantitative description of a certain process that can improve climate models. Since these types of processes are only represented in a parameterized form, it points at the development of parameterizations but it does not explicate how the information can actually be implemented in these models. The quotation below illustrates this.

Fundamental gaps remain in our knowledge of the aerosol-climate relation: the sources of particles in the nucleation and Aitken mode, as well as the chemistry of the latter, and the interaction of organic and inorganic constituents. It is the CCN numbers that determine cloud radiative properties and a quantitative description of the number of organic and inorganic

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286 It should be pointed out that although much experimental research at MISU is related to aerosols, this is not necessarily the case for experimental meteorological research in general, but my study is about research at MISU.
portions and the way such particles take up water will be essential in improving climate models (Application 2003 3085, p.8, my emphasis).

A third type of proposal explicates both what the problems in the models actually consist of – as distinct from pointing only to “uncertainties” – and how the proposed project will help to solve them by producing a parameterization. This includes proposals for constructing a parameterization from measurements as well as from theory or previous modeling efforts. The following extract is an example of this type.

The aerosol climate effect is the most uncertain part of the anthropogenic forcing (…) Simulations with (…) models [which have added more details to the aerosol code] show that CCN concentration is crucial to determine the indirect aerosol climate effect, part of the large uncertainty must thus result from our incomplete knowledge of the source processes. One source is the oceans. (…) Sea salt is one of the components in marine aerosols resulting from this source and the organic component is important for the production of CCN.(…) Most of these effects can only be assessed in a quantitative manner if the primary marine aerosol source is included in regional or global transport or climate models. The primary marine aerosol production includes a complex chain of processes. Therefore, it must be parameterised before it can be included in large models. However, the uncertainties in the existing parameterisations are large (Application 2003 3456, p.1).

To summarize, both experimentalists and modelers translate their research problem into climate terms by pointing at the gaps and uncertainties that the IPCC reports have identified in climate models. These uncertainties are typically due to the insufficient or lacking description of a certain process (i.e. a parameterization). Whereas the IPCC identifies important areas for research that both modelers and experimentalists draw upon, the situation is especially favorable for experimentalists. The IPCC reports provide opportunities to connect their research to climate models and their identified uncertainties, independently of their knowledge about for example the construction of a cloud parameterization or its function in models.\(^{287}\)

There seems to be a general consensus of opinion among the meteorological researchers in my study that the largest uncertainties in climate models concern the description of processes that are not explicitly simulated – parameterizations – and that this is where the most important improvements can be made (see also Edwards 2001).\(^{288}\) Consequently,

\(^{287}\) This can be compared to what van der Sluijs et al. (1998: 310) write about climate sensitivity being a way of indexing GCMs given that all the scientists who “work with GCM modelers by adding new processes to the models (such as ecological modelers and atmospheric chemists)” cannot possibly understand GCMs in all their complexity.

\(^{288}\) In this context, parameterization is used in a broad sense of the term, as referring to descriptions of sub-grid processes in models irrespective of whether these are explicitly referred to as parameterizations or not.
connections to this topic are potentially fruitful from the point of view of climate significance, and researchers involved in both simulation modeling and experimental work formulate the rationale for their research in terms of producing or improving parameterizations for climate models. The following quote from one researcher exemplifies this: “The aim of my research is to develop better parameterizations for the global models, in order to make them better at weather forecasts and better at climate time scales.” Because of the importance of parameterizations in translating problems in line with climate research and also because it appears as a common interest among both the experimentalists and the modelers in my study, parameterizations will be discussed in detail below.

**Parameterizations as Boundary Objects**

The production of parameterizations has primarily been a concern of modelers but as a consequence of the turn to climate, experimental research is also translated into the framework of modeling through the production of parameterizations. Several empirically based projects conducted by researchers at MISU present the development of parameterizations, on the basis of their data, as a more or less natural sequel of their project. However, there are differences in terms of whether the construction of parameterizations is included as a task in the project, or if modelers outside the project are expected to continue the research work where experimentalists finished their work on data processing. In the first case, parameterizations are a specific and defined objective included in the description of the project. In the second case, “quantitative descriptions” or “parameterizations” are used more loosely as keywords. “Parameterizations” here refer to a part of models which represents the knowledge about a process, but it does not explicitly consider on what premises the model can take the process (as described on the basis on experimental data) into account in a parameterized format. These differences were touched upon above.

This situation supports the idea of needing to view climate modeling as an obligatory passage point in terms of the discursive label climate research, because experimentalists couple their research to uncertainties in these models. If climate modeling holds the position of an obligatory point of passage, it is valuable for all parties to participate in the development of models, or at least to appear as if they are participating: A modeler as well as an experimentalist pointed out that the coupling of experimental work and climate modeling is often much looser than formulations in grant applications suggest. One modeler stated that the data produced with such motivations is rarely used, primarily because the experimentalists have not extracted the information that is necessary to be able to build a
parameterization (cf. Randall et al. 2003). Consequently, as in the case of model evaluation, there is a gap between where experimentalists consider their work to end and where the modeler can begin to use the results. At the same time, it is questionable whether experimentalists wish to be reduced to pure data suppliers thus making their activities resemble monitoring (cf. Randall & Wielicki 1997: 405).  

An attempt to collaborate in a climate-oriented program can serve as an example of a basic problem in relation to making experimentalists provide data to modelers. In this case, the experimental group of researchers had to leave the project in the early stages and a modeler who was involved in the program commented on this in the following manner:

[It’s impossible to say that we will follow measurement data slavishly] and that led to a conflict in [the project] in the beginning. It was a participating measurement group (…) that was working with boundary layer measurements and they didn’t want to affiliate themselves to the model structure in motivating their measurements but wanted to do more … They had to leave the project. It didn’t work.

This statement raises one of the central problems involved in the integration of models and data. This relates to the compromise between the concerns in experimental work in which the interpretation and understanding of measurement data is at the forefront, and where the willingness to provide useful data for modeling is of secondary concern. In other words, this can be seen as an example of how different audiences’ expectations may be incompatible with one another and sometimes incompatible with the problems that scientists seek to pursue (Clarke & Gerson 1991).

A relatively recent application – for a climate research school – seems to renegotiate the situation regarding climate research by emphasizing the importance of producing results that can be used in climate modeling for research to be able to fall under the definition of climate science.

There is a tendency among atmospheric scientists to motivate interdisciplinary research with the climate-change problem. This is natural in the struggle for funding of well-motivated interdisciplinary research but it tends to lead to such a wide definition of “Climate Science” that hardly anything within Earth sciences is omitted. (…) While not attempting to prescribe to any particular definition in a more general context, this proposal thus attempts to narrow the field somewhat. This definition thus includes process-oriented studies, also including experimental work, as long as the

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289 For example, one modeler said: "Building parameterizations is something mainly run by modelers and they use the data they access and the data they recognize the look of and the data that has a structure that is suitable for [building parameterizations]. There are measurement campaigns or more permanent measurement spots which have been built exclusively in order to do these kind of studies (…) But then it’s not the same [type of] experimentalists in a way but more monitoring [rather than research]."
target within the project is to utilize new knowledge in a climate model (Application 2003 3079, p.5, my emphasis).

By explicitly presenting a narrow definition of climate science (in a particular context), as opposed to mere implications in experimental applications, it serves to undermine the possibility to receive funding based on formulations of climate relevance or climate research that do not explicitly show how the project contributes to climate modeling. This can be interpreted as part of the process of interessment, whereby modelers seek to lock experimental research into the proposed role by defining what experimental work should be like if it is to be defined as climate research. Interessment is facilitated by boundary objects, through which researchers can accomplish translation without coercion. In this case, parameterizations have the possibility to function as boundary objects because they appear as objects that both modelers and experimentalists can gather around and work with, in spite of the current discrepancy between discourse (rhetoric) and practice.

The way experimentalists construct a relation between their research and climate model parameterizations in grant applications is an example of how they try to maintain the goodwill and support of relevant audiences (cf. Clarke & Gerson 1990: 191). To point at the possibility or goal of developing parameterizations on the basis of the data they produce is also the most direct way in which experimentalists align themselves to climate models. However, while experimentalists address both the interests of funding agencies and modelers by doing so, grant applications are directed towards funding agencies. Thus, the applications aim to enroll the agencies into the program of action proposed by the experimentalists, who thereby receive financial support. For example, if funding agencies offer money to climate research, a profitable strategy for the researchers is to present their project as if it is important from the point of view of climate. However, this is not an example of enrolment in line with the agenda of the modelers as much as it is an attempt to translate the interests of funding agencies into the goals of the experimentalists by referring to the obligatory point of passage: climate modeling.

Although experimentalists build their problematizations on the basis of climate models, and sometimes parameterizations, it does not follow from this that interessment take place in practice. The experimentalists’ problematizations translate the interpretative frames of funding agencies but mostly give the impression of enrolment into the working process of climate

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290 I do not consider the composition of the councils of funding agencies. For instance the national science councils consist of fellow researchers and it is uncertain what effect this may have on decision-making, for instance in relation to their own research interests.
modeling.\textsuperscript{291} This further supports the idea that parameterizations function as boundary objects that serve different purposes for experimentalists and modelers and that they are accordingly inserted in their translation processes in different ways.

Flexible Features of Parameterizations

Considering the question of parameterizations as boundary objects, it is interesting to study the experimentalists who motivate their research in terms of parameterizations as well as participate in their development in practice. This can be done independently of how many experimental projects that actually produce data for use in the development of parameterizations because the point is to establish differences in (idealized) practice perspectives, not between individuals.

The views on parameterization diverge also among modelers (see also Wynne 1996: 369). For example, according to one modeler,

\begin{quote}
It’s a bit more arbitrary how to do parameterizations. Different researchers can have very different opinions about what he or she thinks is the best and it is hard to say who’s wrong, but everybody is right in some way. It’s not until you show that a parameter is clearly superior to another that you change it. (…) Parameterizations of how you treat clouds, everyone makes it different. There’s no general way to do it. The same thing with boundary layer parameterizations, they are very different.
\end{quote}

Furthermore, in a textbook on dynamic meteorology it is stated that parameterizations are probably “the most difficult and controversial area of weather and climate modeling” (Holton 1992: 462, see also Edwards 2001).\textsuperscript{292} Nonetheless, researchers who are involved in constructing parameterizations mention similar characteristics of parameterizations that they consider to be important. For example, parameterizations should be computer-efficient, numerically stable, portable, and representative, as mentioned earlier.

However, there seem to be different understandings of how to achieve this, primarily among modelers and experimentalists. This is in line with the idea of parameterizations as boundary objects. It is also consistent with the analysis of model evaluation. This suggested that modelers and experimentalists have different expectations about the model as a numerical laboratory with its own value and shortcomings (modelers) or as primarily a

\textsuperscript{291} This possibility is also hinted at in the application mentioned above in terms of the reference that the climate motivation “is natural in the struggle for funding”.

\textsuperscript{292} Data agreement is also an important issue in terms of the implementation of new schemes and parameterizations. Modelers have opinions about whether one scheme is better than the other, but the final judgment depends on the type of modeling in question, whether it is predictive or representative.
replacement of the real world, and as such with higher demands on its realism, regarding similarity to measurements (experimentalists).

In the following, I show the interpretative flexibility of parameterizations and thereby further illuminate their character as boundary objects. I do so by discussing three aspects of parameterizations: direction, understandability and/or representation, and mutability.\(^{293}\)

**Directed From or Towards the Center?**

From the point of view of simulation models as centers of calculation, researchers develop parameterizations from two directions. To develop them on the basis of equations, exemplifies development from the center towards the periphery, as in the previous chapter, where validation of models exemplified a movement from inside the center of calculation towards the “outside”. On the basis of measurement data, parameterizations are developed from the periphery towards the center.\(^{294}\) Therefore, there are two ideal types of constructing parameterizations; a modeling and an experimental way. These strategies are more or less combined in practice. For instance, parameterizations derived from theory are never purely theoretical and always include some empirical elements. In constructing parameterizations based on data, on the other hand, researchers often have to use so-called process models (in addition to data). Nonetheless, there are some distinctive differences and these are described below.

The “pure” modeling way to construct a parameterization is to start with the laws of physics and move from the most general case to the more specific. They start their work from theory – thus with the centers inside of the centers of calculation (Latour 1987: 241). Sometimes the goal is to

\(^{293}\) I have primarily but not exclusively focused on parameterizations related to the description of different aerosol effects in climate models because these are the parameterizations that the researchers I have based my material on are working with. Because processes related to particles take place on such a small and detailed scale in comparison to the scale of climate models, many parameterizations are required to study the effects of particles in climate models. This also exemplifies one of the fundamental complexities of atmospheric modeling: the wide range of scales. For instance, the evolution of stratocumulus clouds depends on processes that act on a spatial scale from \(10^{-7}\) to \(10^{7}\) m (Sigg 2000: 5). This wide range of scales and interactions cannot be explicitly calculated in models and in the case of clouds processes they are represented in a simplified, parameterized form in, for example, NWP-models as well as climate models. Within meteorological research, the difference regarding micro and macro is generally also a difference between experimentation and modeling. At MISU, a majority of the modelers are interested in modeling meso or macro scale features, independent of whether it is about chemistry or meteorology. At other departments, more modeling is concentrated on for instance aerosol processes. Another difference with these models is that they are more focused on physical, as compared to meteorological, processes. There are also studies of micro-meteorological studies, primarily of turbulence, but examples of these cannot be found at MISU. However, micro-meteorology is still not concerned with processes on the scale that atmospheric physics and atmospheric chemistry measures. Thus, there is still a difference in scale albeit not as large.

\(^{294}\) These different approaches also relate to the mutability of models, which is discussed below.
produce a parameterization that will work well in general situations, primarily in research oriented towards numerical weather prediction. At other times, modelers concentrate on more specific situations, for instance processes in the boundary layer.\textsuperscript{295}

The “pure” experimental way of participating in the construction of parameterizations is to base them on the products of experimental work: data. A parameterization can be completely empirical. The result is an expression based on empirical relationships, not established in fundamental theory (cf. Winsberg 1999: 289), but it contains some idea about a relationship that is based on what has been found in (established through) data processing. This approach is necessary when the theoretical knowledge about a process is held to be deficient.

Nevertheless, a modeler points out that empirically based parameterizations are problematic:

> You have to be very careful when you have parameterizations in a model so that you don’t parameterize the same thing several times. I think that is why you go to the mathematical and physical laws and parameterize the processes [from them]. If you take the example of the very stable momentum flow, (…) it can depend on waves, it can depend on turbulence somewhere else that is transported down. Thus there are different processes that lie behind this interval having such a flow. \textit{If I only take data straight off and make a figure, all the processes will be baked into this parameterization. Then I will probably have a fairly good representation of the flows in the model. But (…) I don’t know what processes lie behind it.} (My emphasis)

If empirical parameterizations take their point of departure in field measurements, it means that the observation depends on everything that happened at the location where the measurements were made at a certain time, not only due to theoretically conceptualized and distinguished processes.\textsuperscript{296} Modelers aim to isolate all processes in the model and thereby to see how processes relate to one another. Empirical parameterizations complicate this because they include natural variability and a mix of

\textsuperscript{295} In one project where modelers from MISU participated, parameters were set on the basis of model simulations with a detailed model. During a conversation with one of the participants, he told me that the reviewers of a manuscript were “upset” and had criticized the approach of using simulations rather than observations for this purpose. In the paper, the authors defend their approach by arguing that to develop this specific parameterization would require a huge and very expensive campaign and that using a numerical model was an efficient and less costly method. The researcher said he would have liked to, but that it would have required extremely dense measurements along a coast. Besides that, he added that it is conservative to only use data when models function so well. As I noted in Chapter Six, I will not further consider this type of parameterization because of a lack of material regarding this approach.

\textsuperscript{296} In fact, there are also laboratory studies of processes. These are similar to idealized simulations in terms of the controlled environment. However, I will not consider the differences between field and laboratory experiments.
processes held to be distinct by theory. As the quotation above suggests, this makes it difficult to interpret them from a representative modeling viewpoint.\(^{297}\)

Regarding direction in the construction of parameterizations, it is also interesting to recollect that when simulation modelers who are involved in what has been referred to as theoretical construction of simulation models develop new parameterizations, it sometimes involves collaboration with theoreticians or theoretical modelers who only work with developing theories or analytical models.\(^{298}\) For instance, in one project, two simulation modelers worked with the implementation of a new analytical model that was proposed by a scientist who never works with computer simulations himself. In order to implement these into simulation models, theoretical modelers are “assisted” by simulation modelers. What this also points at is that parameterizations are not only boundary objects in the relationship between experimentation and simulation modeling, but also in relation to theoretical modeling.

**Understanding and/or Representing**

A general difference between experimental work and modeling construction is that whereas experimental work is primarily directed towards a fundamental understanding of processes, the bottom line of modeling construction is to represent the process in the simulation model, preferably in such a way that it improves the result of the simulation as a whole. Consequently, when experimentalists address the question of representation in simulation models, they address the concerns of the modeling world (cf. Clarke & Gerson 1990).

The following quotations illustrate several related points. First, regarding the design of measurements, a respondent pointed out:

> Already from the start we have to think that those parameters have to be in the model. (…) In this case for instance, the strength of wind represents both friction between water and air and the wave fields in the water and some

\(^{297}\) Further supporting this division is the following remark in a textbook on (climate) modeling where it is stated: “Modelers, of course, strive to keep their parameterizations as physical and nonempirical and scale-independent as practical.” (Schneider 1992: 18f.)

\(^{298}\) However, in Chapter Six, I do not make this differentiation because theoreticians are sometimes parts of the *ensemble* of people participating in theoretical construction and therefore not clearly distinguishable from simulation modelers. Although theoretical practices are beyond the scope of this thesis, it is interesting to briefly mention this collaboration in line with the question of relationship between practices. While validation and parameterization create a dependence on data from the point of view of modeling, theoretical working practice and simulation modeling are also tied together in the sense that theoretical modelers in meteorology use simulation modelers as support personnel (cf. Becker 1982). By implementation in simulation models, they get the opportunity to “test” theories in numerical experiments.
other details that you have skipped. Then you only have the wind speed. We
know that it is not only the wind speed that’s decisive, but it’s dominating
(…) There are no wave fields in the model to use anyway. Then you make a
parameterization that isn’t possible to use, there’s nothing in the model to
connect it to. (…) If a parameterization contains a parameter that no climate
model (…) has inside or that they can’t get in any other reasonable way, it is
meaningless.

If one attempts to produce an empirically derived parameterization, it has to
be taken into account when planning the measurements. Producing useful
empirical parameterizations therefore includes taking the structure of climate
models – with a meteorological core – into account and developing
parameterizations that are suitable from this point of view. This restates the
fundamental position of meteorology in climate research in general and
climate modeling in particular and it reconnects to the questions discussed
earlier in the chapter regarding the use of measurements in constructing
parameterizations.

[Experimentalists] may have the goal to only understand reality. Perhaps they
didn’t have the purpose from the beginning to make [their results] easily used
in a climate model or diffusion model. That’s a good purpose too, but I think
that today, people who are working with understanding their data,
correlations, they should learn before what’s in the large models. But that’s
not always the case.

The respondent, a researcher who works both with measurements and with
developing parameterizations, contrasts the core of experimentation – “to
understand reality” – with an important aspect of modeling: “making
something useful” in a model, in this case a climate model. These two
interrelated points exemplify a case of translation whereby experimentalists
start to look at the world from the perspective of modeling as well as direct
their work towards the same. It is also important to note that while
parameterizations aim to take processes working at smaller scale into
account, they do so from the point of view of large-scale parameters.299
Parameterizations thus per definition have to translate processes into a
modeling framework if they are to be implemented.

When a modeler who works with the construction of parameterizations
explained her process of work, she emphasized that the first thing to do is to
understand the full process, not just the variables in the model. However, if
experimentalists direct their measurements towards large-scale variables of
interest in modeling, they are less concerned with exploring possible
correlations, thereby making it difficult for modelers to understand the full

299 This approach embodies the closure assumption, which is the assumption that small-scale
processes can ultimately be represented accurately in terms of the large-scale variables
available to the model (Edwards 2001: 56).
process through studies of measurements. Consequently, it is not necessarily an advantage for modelers if experimentalists only attempt to supply data for climate model development (cf. Shackley et al. 1998). In the long run, it makes it impossible to find new ways of constructing a parameterization (and hence, develop models) if both modelers and experimentalists direct themselves toward improving existing models and forget about (model-) unbiased studies of the material atmosphere. This is a sensitive question because it suggests a fundamental conflict. On the one hand, it is of benefit to modelers if experimentalists produce useful measurements and it is an advantage for experimentalists to use this as a motivation for funding. On the other hand, in the long run, this potentially undermines the production of alternatives and the possibility for modelers to “pick and choose”.

The Mutability of Parameterizations

Mutability of parameterizations can be divided into several aspects. First, if modelers change the equation or the coefficients of the equations and second, if this is done before or after running the model simulation. Parameterizations include coefficients and constants and researchers generally determine the values of constants and coefficients in their mathematical expressions by studying data. Whereas the equations in terms of the relation between parameters are (principally) immutable mobiles (i.e. fixed), some empirically determined constants, threshold values, and coefficients, are more easily adjusted.

As soon as you have a parameterization of something you have to have constants. You have this long mathematical problem and then you cut it here [the modeler shows a distance and cuts off two-thirds], then you have to say, this problem, what is it? Is it 3.5 or 7.1 or something? And you can switch or decrease or increase the value of constants and then get different answers from the model. For instance, at what temperature should you get condensation in a numerical model? Well, you can change that a bit.

Thus, every number introduces (more) mutability to the model because they facilitate tuning. I will briefly discuss the numbers of coefficients before returning to tuning.

There are no inherent differences between theoretically or empirically derived parameterizations in terms of numbers of coefficients. However, it is likely that experimentalists and modelers view the numbers of coefficients differently because while modelers both have to construct and implement

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300 One researcher also expressed his worries about a streamlining of climate research: “In a pluralistic world, there would be lots of money for different types of research, different ways of looking at things and alternative hypotheses. I think that I work with those types of large climate models but some of those streams [within climate modeling] view it as the ultimate truth. ‘We know what it will be like and therefore we should have more money.’ There should be more alternative models and measurement programs.”
parameterizations, experimentalists only provide the coefficients. For example, one experimentalist told me about a manuscript reporting on a new parameterization.

We had some trouble with one of the referees who thought we had too many coefficients. We had 25 coefficients and the old parameterizations had about eight, ten coefficients and he thought it was too tough with all those coefficients, but we were convinced that it was good and I think that the computers have become faster than when the competing parameterizations were published. So they should be able to handle a few more coefficients.

This experimentalist noted the problem of computer resources but she did not seem to be concerned about other possible reasons for reducing the number of coefficients or other possible reasons behind the critique that it was “too tough”. A contrasting point of view is exemplified by a quote from a modeler. He commented upon (previous) limits of computer power and also pointed at problems regarding the adjustment of constants.

The problem is that all those constants that have to be decided. It was harder to do them in the 1970s because the computers were slower. Today there is no problem to make those. But the problem is that if you make something wrong in one constant, then another has to compensate to make the value correct. At last, you don’t really know… It’s like the chef in the Muppet show, he presses somewhere and it comes up somewhere else.\(^\text{301}\)

Consequently, if the number of coefficients is reduced, it is likely that it is easier to avoid what is considered the risky operation of changing the values of coefficients in order to improve general model behavior (cf. Randall & Wielicki 1997). This leads back to the question of tuning.

Tuning involves adjusting parameters \textit{after} a model has been run in order to improve simulation output in terms of correspondence with observations. Representative modelers reject tuning because it makes them unsure about the cause behind the result. The quotation below illustrates this view.

These parameterizations have been developed and then you run the model and check the parameterization. If we twist this parameterization and change this thing, the result is much better. Then you run it. But in these models there are loads of these things, which are adjusted and tuned to make them work and give good forecasts for Sweden. The problem with those things is that they are not always physically correct. You can get the right answer for the wrong reasons.

\(^{301}\) During the same interview, the modeler also compared an old way and his new way of parameterizing a process, and notes another advantage of having fewer coefficients: “In some, there are six, ten, eleven constants you have to decide but in this one, we only have three. It makes it much easier. It becomes much simpler to make something precise.”
In predictive modeling at the weather service center, tuning is more common and is accepted as something unfortunate but necessary. The quote below exemplifies this, but also an important general consideration in modeling – the model as a whole.

When you look at it from the model viewpoint, the most important is that the model as a whole works well. It means that when you parameterize, you use some dirty tricks which makes the model work but which do not necessarily fit with all measurement data. And you have to do that.

From a predictive point of view, it seems as though more physics – i.e. theoretically based parameterizations – makes it more difficult to use the model because it becomes more sensitive to changes, and thus less easily adjusted to improve output. A modeler working at the weather service center said that the more “physical” the model is, the more difficult it becomes to implement new parameterizations. According to him, simple models are “more robust” and one can, for example, change a coefficient in a radiation parameterization to compensate for something related to the ground. Consequently, this is a sign of increased immutability because more “physics” increases the internal consistency of the model, even if increased sensitivity may seem to imply the opposite. Nonetheless, the modeler added that the basic philosophy behind parameterizations is that more physically based parameterizations should lead to better models, i.e. better results. But he also asked: who says that the parts are better than the model as a whole?

This is an important issue. Although some modelers are especially interested in the representation of a certain process, they cannot neglect the function of the model as a whole when they implement the parameterization in an existing framework. Consequently, there are generally trade-offs between the parameterization and the existing model structure at this point (see also Chapter Six). The experimentalist, on the other hand, tends to be interested in a particular process. S/he can concentrate on representing this process and does not have to consider all the problems of integrating this into a model. This is not about ignorance, but a result of different perspectives that emerge out of different practices.

There is another difference between experimentalists and modelers. During a discussion about climate modeling at a meeting attended mostly by experimentalists, one of them criticized climate modeling. This researcher maintained that climate models are like weather predictions and when climate models are developed the modelers “play around” with parameterizations. The interesting point here is that because experimentalists working on parameterizations leave them in the hands of

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302 The experimentalist also added that they “work in the same way because they come from the same community” (thus supporting the view of simulation modeling as one subworld) and stated, ”I would like to know more about the process before I formulate something.”
modelers after they have constructed them, every adjustment may appear as “playing around” or have the same negative connotation as “tuning”. This means that from an experimental perspective, all changes involve changing the final products of experimental research. Furthermore, it suggests that the features of simulation models as materialized models, consolidating computer programs and mathematical models, do not only pose difficulties in grasping simulation modeling practices. It is also a cause of problems in the (cooperative) relationship between modelers and experimentalists.

Beyond the (Discursive) Climate Network

The experimentalists’ attempts to problematize their research in relation to climate are the primary basis of the foregoing discussion. However, there are also modeling efforts that are not directed towards climate modeling and that do not attempt to be labeled climate research. Climate models are based on numerical weather prediction and thus on meteorological (dynamic) modeling, but all attention has not turned to climate. Why do some modelers translate their research into climate terms and why do others choose not to?

Since climate modeling requires increasingly more descriptions of processes, empirical research is required to understand processes about which little is known. A consequence is that experimentalists have good opportunities to take research initiatives in this area. At the same time, the growing importance of models makes it even more difficult to change the basic structures. This is not only a consequence of the turn to climate but it also points to a difference in the opportunities that different groups of researchers have to jump on the “climate change bandwagon” (cf. Fujimura 1988).

In this chapter, I have discussed climate research from the point of view of the researchers. This is in line with the focus on the subworlds of

303 All experimental project applications appealed to climate whereas a few modeling proposals did not. Although these have not been included in the presentation, this is also notable in applications from MIUU: Whereas experimental proposals to some extent refer to climate, modeling proposals do not.

304 It is crucial to distinguish which translation process is at issue here. While the experimentalists’ interests have been translated into climate modeling in one sense (they motivate their research in relation to it), there are differences concerning the translation mode of interessement: whether their concrete measurements are directed by climate modeling or not. At the same time, experimentalists translate themselves in relation to funding agencies (problematicizations in grant proposals). Consequently, there are several translation processes, from different actors, involved in the construction of climate research (cf. Star & Griesemer 1989). The process of interest in this section is how research is problematized. Furthermore, it should be clear that this section only seeks to offer some suggestions about and not a complete analysis of why researchers resist climate research problematizations on the basis of the analyses of previous chapters.
meteorological research that informs the thesis as a whole. Meteorological research has played a fundamental role in establishing and formulating the threat of anthropogenic climate change as we know it today. Consequently, it has played a central role in the growth of the climate change problem from a micro-scale problem in the hands of a few devoted researchers to a macro-scale problem – global in all its aspects (cf. Callon & Latour 1981). It is unnecessary for meteorological researchers today to problematize climate change from scratch, but they do need to adjust to the existing climate network. This is exemplified by the structure of the problematizations discussed above, where I also noted how experimental commitments are not always compatible with the development of climate modeling as practiced by (climate) modelers.

However, climate modeling is not always compatible with other modeling commitments either. For example, the more routines and sub-routines a model contains, the more difficult it is to use and develop. In line with this, several modelers generally recommend that solving (theoretical) problems is best approached by using models that are as simple as possible. These suggestions are in line with the practices I have previously called theoretical construction and explorative use. In contrast, climate models become more and more complex, in part because of the position of climate modeling as an obligatory point of passage in climate research, and in part because of the desire to include more descriptions of processes.

The differences between modeling and experimentation in terms of the material and human resources these practices require may also affect the tendency and need to apply for funding for every new line of work or small project. Since models are often for “free”, at least in terms of monetary costs, whereas measurement equipment is expensive, modeling research is in principle possible with no more additional resources than a salary and a computer. Given powerful computer systems, modelers have principally less need to apply for external funding compared to experimentalists and thus less reason to formulate their research in climate terms for funding purposes.

In this sense, climate modeling appears as more oriented towards prediction. It also does so from the perspective of organization. Predictive modeling involves a formal social network surrounding one model when representative modeling tends to take place in informal collaborations with a larger degree of independence between the activities of the participating modelers. Nonetheless, climate modeling cuts across predictive and representative modeling in the sense that climate modeling does not necessarily presuppose a commitment to results. This is shown in Shackley’s (2001) analysis of different epistemic lifestyles of modelers, where the constructor roughly corresponds to the practice of representative modeling and the seer to the practice of predictive modeling (in this respect). While there are problems with this typology (see Chapter Six), together with my own findings it suggests that climate modeling shares several features with other, closely related simulation modeling practices. Partly therefore, I suggest that more detailed investigations of simulation modeling practices per se are required in order to understand not only questions related to climate science, but also in relation to other scientific simulation work, not least science-policy relationships where simulations are involved.

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Concluding Remarks

In this chapter, I have argued that climate change is an arena where negotiations about the meaning of climate research take place. The chapter shows how the climate change question as an arena is both a source of potential cooperation as well as competition within meteorological research.\textsuperscript{306} In addition to participant observation and interviews, funding applications represent a fruitful point of departure for the study of negotiations, not only between experimentalists and modelers, but also in relation to the general “funding climate”. Thus, the chapter showed how researchers translate their problems into climate terms by relating their research to climate models, preferably by relating research to the development of parameterizations. It has been suggested that climate modelers have translated the interests of other scientists (e.g. Shackley et al. 1998) and that climate models have become obligatory points of passage for truth claims about climate (Edwards 2001). However, as I have pointed out, none of these suggestions have been accompanied by, or based on, more in-depth empirical studies on how the interests of other scientists have been translated, nor the meaning and consequence of climate models as obligatory points of passage in research practice. This is a gap that this chapter has endeavored to fill. In the final chapter I will also attempt to develop the consequences this has for scientific production.

Because of the question about the relationship between modelers and experimentalists, the mutual interest in parameterizations was particularly attended to. Parameterizations function as flexibly interpreted boundary objects in this relation and it has been shown how they serve different translation purposes. At the same time, their features appear differently from the point of view of experimental work and modeling respectively. I also explicitly but very briefly touched upon why some meteorological researchers do not problematize their research in order to participate or appear as participating in the climate arena. However, this should be distinguished from what happens later in the process because while many experimental projects are formulated in connection to climate change and climate modeling, the analysis cannot show how their outcomes are in fact used in the development of climate models.

The different translations of interests in relation to changes in commitments discussed in Chapter Five are also of importance in understanding the problems in the relationship between simulation modeling and field experimentation. While experimentalists sometimes address the explicit interest of simulation modelers, they do not change their commitments. In other words, interest (as an actor-network concept) is

\textsuperscript{306} This may not be the only arena where subworlds of meteorological research meet and where there is competition and/or cooperation. Nevertheless, it is the arena I have focused on and about which I seek to draw my conclusions.
volatile and cannot explain the more robust perspectives and commitments based on practices. Perhaps this is also related to the discrepancy between discourse and practice in the sense that while the actor-network perspective attempts to incorporate the materiality of social life, it nevertheless more adequately addresses the formation of discursive interests, i.e. accounts of interests through negotiations rather than practical commitments grounded in activities (and the shared understanding of these) (cf. Shapin 1988: 543f.).

More generally, this could be connected to the actor-network view of the world as a “seamless web”, where everything is connected to everything else through heterogeneous associations that result in different actor-networks (see e.g. Latour 1987). While we can study the threads of this web and (re-)produce piles of actor-network stories by network-followers, as Shapin (1988: 547) points out, there is little to be said from within this web. Consequently, a consistent actor-network analysis (in a sense) exemplifies what Gubrium and Holstein (1997) refer to as a postmodern idiom, in which there is nothing more to analyze than descriptions and representations.307

As a final, more empirical point, an interesting question in connection to related research on the relationships between scientific knowledge and political decision-making as well as politics more generally, is the mediating role of the IPCC. Rather than focusing on the political “side” and ready-made science – consequently the second and third volumes of the assessment reports (e.g. Mattson 2005) – this chapter suggests that the first volume of the IPCC assessment reports also functions as a mediator from the point of view of research – i.e. science in action (cf. Latour 1987). Not only do the reports privilege results of climate modeling. They also serve to re-affirm their importance in enabling scientists to relate their research to an established problem formulation and through the emphasis on parameterizations (the uncertainties in process descriptions), reproduce the meteorological core in climate models and in the climate question more generally.

307 See also Gubrium & Holstein (1997: 218, n.6). However, Latour (e.g. 1991/1993) has explicitly rejected postmodernism on an epistemological level in part because it separates nature, society and discourse rather than bringing them together. Nevertheless, as implied above, the seamless web has similarities with a more Foucaultian view on discourse but includes material agents in it.
This thesis concerns science as a production process. More specifically, it has addressed this topic from the point of view of science worlds, what they are like and how they are constructed on the basis of a study of meteorological research practice. My decision to focus on meteorological research is especially motivated by its role in the climate change question, which has formed an arena where several actors meet. An additional interest of the thesis is therefore the construction of climate research and how it has affected the two subworlds of meteorology that I have focused on: field experimentation and simulation modeling.

The thesis is based on a qualitative study of meteorological research practice, with a specific focus on climate related research. Data was collected at a meteorological research department and the methods used were participant observation and interviews. In addition, it included observations made during field expeditions, interviews at different departments, and text analysis. Different theories bring different things to the foreground during fieldwork as well as in the subsequent analytical process. I have used concepts related to social worlds in combination with actor-network ideas, in part by incorporating material objects into the analysis. Thus, the thesis has focused on the production and articulation work in the social world and to some extent in arenas, and on the networking that takes place inside and between subworlds. It has also discussed the role of things in the subworlds, particularly measuring instruments and simulation models as inscription-devices (cf. Latour & Woolgar 1979/1986) that are further characterized as epistemic objects or technical artifacts (Rheinberger 1997; cf. Knorr Cetina 2001). More specifically, the study focused upon the practice of field experimentalists and simulation modelers as representing two subworlds, based on common activities through which a shared perspective and commitment arise (cf. Strauss 1978b; 1993). These worlds were analyzed both separately and in relation to each other, specifically in terms of how their relationship is affected by the climate change question as a common arena.

In this chapter, I will present and discuss the main results in four thematic sections structured according to processes of importance in relation to social worlds: segmentation and subworlds, legitimation, arena processes, and intersections. These sections also include discussions of questions brought up by the use of actor-network ideas as well as methodological
considerations. The chapter concludes with a summary of the contribution made by this thesis.

The Subworlds of Field Experimentation and Simulation Modeling

In the first two empirical chapters, I focused on field experimentation and simulation modeling separately. Because of their differences the chapters emphasize different aspects in order to highlight what I found most characteristic and fruitful to analyze, both from an empirical and a theoretical point of view.

To establish successful production networks, scientists make choices about which combination of scientists and instruments will provide the most productive coalition and organize projects that incorporate the resources of many scientists with different equipment and skills. The articulation work that is required to organize central activities and the types of practices that the organization of activities includes was presented in the chapter on the experimental subworld. The chapter shows how the incorporation of practices depends on different types of relations. The concerns involved in integrated experimentalism on the one hand, and instrument expertise on the other, are different. Consequently, interessement and subsequent enrolment differ. Whereas entrepreneurship and integrated experimentalism link at an early stage, instrument expertise enters later into the collaborative network – the arc of work. Instrument expertise includes participation in a broad range of fieldwork and since the researchers in this practice participate as pure “measurement guys”, their concern (or sub-program) does not have to be translated in the same way nor to the same extent as for the researchers in integrated experimentalism, who are more bound to one field because of their research interests and their commitment to and use of conventional instruments.

Simulation modeling practices are also presented as a typology but these have a more pronounced focus on production work as compared to the description of the experimental world. The subworld of modeling consists of practices involving theoretical construction, explorative use, pragmatic construction, and applied use. I concluded that the differences between result-oriented modeling practices, which strive to produce the best possible output in comparison with observations, and modeling practices focusing more on the representation of “physics”, was a consequence of boundary work for the purpose of distinguishing between weather forecasting and meteorological (modeling) research.

A major theme of the chapter, especially in relation to model construction and development, is the relation between the computer program and the
theoretical model in simulation modeling. In the modelers’ accounts, the mixing of scientific discourse with practical, technical considerations becomes salient, but the relation between computer program and theoretical model is difficult to grasp, partly because modelers themselves appear to perceive it as either science or technology, not as technoscience (cf. Latour 1987). The tension between physical representation and “good” results is an effect of this separation of the simulation model into either a theoretical model or a computer program, or machine that produces “data”. But virtual atmospheres are creations of materialized models and this is an important lesson for studies of simulation modeling for at least two reasons. First, it highlights that simulation modeling involves both conceptual and hands-on work (cf. Star 1995b; Schild 1996). Second, it suggests different constraints (and possibilities) compared to work with either conceptual models or computer machines in isolation from each other rather than in combination. I will return to this below.

In spite of the differences between the subworlds of field experimentalists and simulation modelers, the analyses of the role of measurement instruments and simulation models as epistemic objects or technical artifacts enable comparison. From this point of view, representative modeling (theoretical construction and explorative use) shares similarities with instrument expertise whereas predictive modeling resembles the experimental focus on the (measured) atmosphere involved in entrepreneurship and integrated experimentalism.

However, there is a difference between measuring instruments and simulation models as technologies of investigation in terms of representation. In contrast to the measuring instruments used in experimental practice, simulation models are representational entities in themselves. Unlike measuring instruments, they are not only used to produce representations. Models typically represent either some aspect of the world, some aspect of our theories about the world, or both at once and hence the model’s representational power allows it to function not just instrumentally like a simple tool, but to inform about the thing it represents (Morgan & Morrison 1999: 11) and to be studied in the way that natural systems might be (Sismondo 1999: 256). This is the role the simulation model plays in exploratory simulation modeling. Hence, these artifacts can be used for different things and the various practices that they are incorporated in require different resources, including resources provided by other production practices. This gives rise to the character of the relationship between the subworlds and the production network that connects them. This was analyzed in Chapters Seven and Eight. Before turning to these chapters, I would like to briefly discuss a question concerning methodology.
Reflections on How to Study Simulation Modeling

The ethnographic approach was better suited for the study of field experimentation than for simulation modeling. As was mentioned in the chapter on methods, in the analysis of modeling practices I relied more on what people told me about what they were working with. However, because I was not only interested in how modelers perceived their own work, it was also important to get some insights into the actual working tasks. During both informal and formal interviews, my informants and respondents often demonstrated different things for me, for example by writing equations, drawing pictures, showing how parts of a computer program worked or the results of a model simulation. This was especially evident during field campaigns, but was also of great importance for the study of modeling where I had less opportunity to observe continuously.

In spite of the emphasis on the study of technical and not only “social” aspects of research work (e.g. Latour & Woolgar 1979/1986: 24), it is evident that “social events” in research, such as when scientists meet other scientists and talk about their findings or the hands-on work carried out by technical staff in chemical laboratories or instrument work shops, are activities that are much more easily observed than the activities involved in simulation modeling. To register activities and events free from the participating actors’ subjective understanding and interpretations of what is going on presupposes the ability to observe those activities and events. This problem is encountered when studying simulation modeling because it is difficult to see what the scientist is really doing. If the sociologists are only interested in this, and not in what the scientist thinks and makes of his/her work, a possible solution is to study the written results of modeling practice (cf. Latour 1987: 246). However, science is not only writing practice and reports and journal articles always reconstruct, rationalize and omit many features of scientific practice. They do not tell us much about how science is done (as opposed to re-presented).  

One way to solve part of this problem is to use research protocols and laboratory notes. These enable us to follow the process of work as it happened, not as it was presented afterwards – a method that has been used by some social world scholars (see e.g. Star 1989a; Clarke 1985). The problem in this case is where to find these types of inscriptions in simulation modeling, which is practiced by researchers who mostly work alone and who do not seem to leave many written traces besides the articles they produce.

How then should we get at simulation modeling practices? It is time-consuming not only for the observer but also for the scientist being observed if observation demands repeated demonstration and constant verbal reports of what particular working task the scientist is involved in. How much does

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308 Since interviews are also about how scientific practice is told, this is also a problem with this technique. See also the discussion on questions and interview guides in Chapter Three.
the sociological observer have to know about simulation modeling and how (and when) should that knowledge be achieved? Of course this question could be asked in relation to any study of scientific practice and the answer, at least in part, depends on the theoretical approach and questions.

Symbolic interactionism emphasizes the creation of meaning in social interaction but the sociological view of face-to-face interaction is perhaps too narrow to capture important and indeed social processes in scientific production (cf. Latour 1996b, see also 1992a). Although the social interaction between simulation modelers involved in developing and using models seemingly independently of each other is not the same as between players in a computer game, computer games are nonetheless good examples of how artifacts mediate social interaction and social relations more generally (cf. Sverrisson 2005). While these types of practices cannot be observed in the same way as face-to-face encounters, sociological theory can nonetheless avoid trying to grasp practices that cannot be observed independently of the stories the participants tell us about them. This is the case with both theoretical work and computer work; both of which constitute simulation modeling. 309 Due to the difficulties involved in observation per se, these are important considerations for further studies of simulation modeling practice in general, but also for any study with a focus on a scientific practice that is not included in traditional experimental or field experimental practices, i.e. the type of research which studies of scientific practice have mostly neglected.

Legitimation Processes in Meteorological Research

Among other things, the process of legitimization includes distancing a social world or subworld from others, setting standards, embodying them and evaluating them (Strauss 1993: 217). Science worlds need to legitimize their work as scientifically reasonable and their methods as technically respectable. It is also inherent in this work to set and maintain the boundaries among and between disciplines. Questions of legitimation were addressed in Chapters Six and Seven.

While it was not a major focus, Chapter Six illustrated how simulation modelers aimed to establish legitimacy for meteorological representative simulation modeling as scientific practice, as opposed to weather

309 See also Merz and Knorr Cetina (1997) discussion of similar issues in their study of theoretical physics as a "thinking science" in which they assert that due to the obdurateness of the field "the study is based rather less on the observation of physicists’ activities than on one analyst’s capability to exploit her physics training and interact with participants as a member of their culture" (Merz & Knorr Cetina 1997: 74). See Gale & Pinnick (1997) for a discussion and critique. See also Knorr Cetina & Merz (1997) for a reply. See Bowker et al. (1997a) for an important contribution regarding cooperative work and computer systems.
forecasting. The history and origins of social worlds are important in order to understand them (cf. Strauss 1978b). Thus, it is valuable to reflect on the results in a historical perspective in relation to the development of meteorology as a discipline, both in terms of its theoretical basis and institutional formation. Meteorological research has developed in close relation to weather forecasting (see e.g. Friedman 1989) and the history of classic (dynamic) meteorology to a large extent overlaps with the history of weather forecasting (see e.g. Nebeker 1995). Theoretical development did not proliferate until the early 20th century and meteorology is thus a relatively young science. Perhaps this is why more theoretically oriented modelers attempt to distinguish their work from weather forecasting as a less scientific meteorological practice?

Every new science world must establish itself as legitimate in the larger disciplinary world (Gerson 1983: 366f.). I do not suggest that meteorology has not been established as a legitimate science, but that my results are in line with Gerson’s (1983: 367) assertion that various segments of a discipline (to some degree) are always engaged in competition for recognition and prestige both within and outside the discipline. Furthermore, I have identified meteorology as an important subject for such competition, including participating segments.

It is also notable that while Chapter Seven primarily illustrated how the legitimacy of simulation modeling is more generally established and maintained, this analysis also strengthens the conclusion that meteorological simulation modelers express themselves in such a way as to legitimize meteorology as a scientific discipline. For example, the appeal to the “physics of the model” is a way to associate meteorology with physics, probably the most prestigious of scientific disciplines (cf. Becker 1967).

In addition, the practice of meteorological simulation modeling also has to prove itself as a legitimate scientific practice. In Chapter Seven, I showed how questions posed by the philosophy of science can be addressed in a sociological way, and, furthermore, how the notion of social worlds and subworlds can help us to understand what happens when the products of two separate production processes, driven by different logics, are brought together. I analyzed modelers’ argumentation, and some experimentalist views in relation to this, as part of a negotiation about the practical significance of the falsification principle. This revealed the practice perspective in both experimental and modeling production and the argumentation is also a sign of how the worlds strive to achieve legitimacy for their activities and their production processes. The arguments can also be connected to the legitimization of the product, in this case scientific results. One important aspect of scientific legitimacy is to connect results to the empirical world, i.e. to produce reference. In line with this, the arguments in relation to the principles of model evaluation can also be seen as part of the production of reference in simulation modeling. Thus, by combining a social
world perspective with actor-network ideas in general and Latour’s work (e.g. 1987; 1999a) in particular, questions of legitimacy can be addressed both in terms of social worlds more generally as well as with a point of departure in the special characteristics of science worlds.

Meteorological Research and Climate as an Arena

In Chapter Eight, I argued that climate change is an arena where negotiations about the relationship between modelers and experimentalists take place. The chapter illustrated how the climate change question as an arena is both a source of potential cooperation as well as a conflict within meteorological research. It showed how researchers translate their research into climate terms by linking their research problems to climate models, preferably by relating these problems to the development of parameterizations. The construction of parameterizations represents an intersection between modelers and experimentalists and this intersection is strengthened as a consequence of how the climate change question enables parameterizations (for climate models) to function as boundary objects.

One of the motivations for this thesis was the role of meteorology in the climate change question. I argued that studies of local practice and small-scale climate research are required in order to understand how climate modeling has translated the interests of different social worlds and subworlds. It is therefore important to note that my own work is a result of the interest that the climate change question has generated and the important role I assumed that meteorology played in this. In a way, my study participates in supporting this hypothesis. As I have turned to researchers with a more or less explicit interest in climate research, I have also given them the opportunity to further emphasize the relevance of climate in their research and the role of meteorology in this. If the purpose had been to establish the importance of meteorology, this would have been a self-fulfilling approach.

As it is, the aim has been to analyze these accounts from a symmetrical and constructivist analytical stance, in which the researchers’ statements become both resources and topics. Thus, I have focused on how relevance is

310 This can be compared to Clarke and Montini’s (1993: 69) conclusion that in arena analysis, there is no assumption that the analyst is not part of a community of practice (a social world) that is more or less active in the arena. The selection process of the research problem rather makes it likely that this is the case (in some way).

311 However, even if I claim neutrality, the act of research is consequential and my study may have consequences for meteorological researchers in the arena (Scott et al. 1990; cf. Becker 1967). While this question is beyond the scope of my purpose, an awareness of it is still important in relation to my result. For discussions about neutrality in studies of science, see the Special issue on “The Politics of SSK: Neutrality, Commitment and Beyond” in Social Studies of Science, Vol 26, No 2.
made and have not assumed that anything is climate research. Nevertheless, I have drawn upon earlier research in discussing climate models as obligatory points of passage for claims about climate. This role of climate models results from a gradual stabilization, but I do not analyze its origin in detail due to my focus on contemporary processes. The analysis of recent grant proposals further supports the view that climate models play a central role in the climate change discourse and specifically in defining what climate research is. Yet the chapter showed differences at the discursive level, manifested in funding applications, and at the practical level, where the translation of interests is not as obvious.

The difficulties in combining experimental work and modeling were traced to their different practice perspectives. This suggests that it is through a social world analysis of how field experimentation and simulation modeling work and relate to each other that we can come to understand not only how and why climate simulation modeling has grown in importance, but also what is hindering the climate network. Using only the concept of translation as an analytical tool in combination with other concepts drawn from the actor-network approach cannot address this adequately. Actor-network researchers suggest that a full description is the only type of acceptable explanation (e.g. Latour 1995; Callon & Latour 1992, see also Law 1994) but this “explanation” can only tell us how translations occurred, not why some did not (cf. Fujimura 1991: 222). I have shown how the interests of experimentalists come to be translated as a result of climate change being a common arena and the role of parameterizations as boundary objects. Furthermore, I have also shown how commitments “stabilize” practices and therefore constitute a form of resistance towards the formation of heterogeneous networks in practice.

To summarize, I have argued – in line with the study by Shackley et al. (1998) referred to in the introduction – that climate models have translated the interests of meteorology, but I have also attempted to show how this process takes place and to some extent, why it happens in the way it does. At this point, I would like to look deeper into what consequences this translation process has for scientific production. First, if climate models have a central position and parameterizations are produced in line with the requirements of these models, how can we understand this in relation to the organization of scientific practices? It is fruitful to relate this question to the notion of center of calculation, which will be discussed in the next section. Second, I will discuss of the role of things in different scientific practices.

312 In addition, to analyze funnels of interest based on the problem formulation of knowledge brokers or intermediaries (cf. Liftin 1994; Sverrisson 2001) is also a way to address the question of co-production of knowledge (Jasanoff 2004a). There are numbers of co-production activities in different arenas and between different types of actors. The above mentioned is one of them, approached from the point of view of research and the production of knowledge, rather than scientific knowledge.
Climate Modeling as Centers of Calculation

The perspective of experimental practice generates the idea that because experimentalists use measuring instruments to capture the features of the “real” atmosphere, their work is also closer to the “real” atmosphere as compared to modeling. However, it is precisely because experimentalists work in this way that their power to explain and represent the atmosphere remains (more) insignificant. Experimental work aimed at parameterizing is part of the translation of the material world towards the (formalist) center. In simulation modeling however, a parameterization that describes a single process replaces the measurements of a whole field campaign (cf. Galison 1996: 137) and thus also the work of many experimentalists. Simulation modeling not only represents more processes (and thereby things), but replaces more people as well: the experimentalists that would otherwise study these processes with measuring instruments.

Modelers gain additional strength by working inside these centers with inscriptions of a higher order that draw many more different things together (cf. Latour 1987). However, the growth of centers of calculation creates a drift towards an increasingly mathematical shape on all things involved in work. Growth makes centers (and networks) more heterogeneous and this requires more formalism to keep them together (Latour 1987: 245). Whereas experimental work (data) primarily represents the world in (its random) detail, simulation modeling represents the world as an ordered and coherent system both by drawing upon formalism and by drawing more things together through the use of experimentally produced inscriptions. Since recent climate models aim to represent more processes and more spheres of the climate system, for example the biosphere, the hydrosphere, and ice, in addition to the atmosphere, this type of modeling is an example of how the center grows.

Thus, I argue that using the concept of center of calculation in empirical studies of simulation modeling contributes to the explanation of how simulation modeling in general, and climate modeling in particular, becomes powerful (cf. Latour 1990: 60) in the sense that they replace increasing numbers of both people and things.

Nevertheless, the usefulness of the center of calculation concept in explaining some features of climate modeling and simulation modeling more generally is limited and the concept is vague. One of the few criteria of centers of calculation that Latour (e.g. 1987; 1990) has defined is that centers of calculation calculate only with inscriptions. This implies mathematical formalism and conceptual work. Pickering (1995: 113f.) analyses (theoretical) modeling as conceptual practice and in terms of what he refers to as disciplinary agency, and suggests,

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313 In fact, explanation and representation are two sides of the same coin from an actor-network viewpoint (cf. Latour 1988b; Callon & Latour 1981). See also Fuller (1996).
Conceptual systems (…) hang together with specific disciplined patterns of human agency, particular routinized ways of connecting marks and the symbols with one another. Such disciplines – acquired in training and refined in use – carry human conceptual practices along, as it were, independently of individual wishes and practices. The scientist is, in this sense, passive in disciplined conceptual practice (Pickering 1995: 115).

In a way, this passage is also applicable to simulation modeling since it captures how simulation models structure work and how they are to some extent path-dependent. However, the importance of conceptual discipline depends on whether modeling is predictive or representative and, in any case, to accept an explanation of conceptual practice as sufficient would be to neglect my previous conclusion that simulation models are material entities. Modeling work is also structured because of the models’ character as assemblages of the routines of computer programs (cf. Star 1991a: 275). It is the double character of simulation modeling practice as not just conceptual paperwork, as in case of theoretical modeling (and in centers of calculation [Latour 1987]), but also as computer programming, that makes the study of simulation modeling practice particularly challenging. Furthermore, while tuning simulation models may be part of routine work at the weather service, the practice of tuning itself as resulting from the practical difficulties with implementing new parameterizations, shows how modeling work cannot be characterized as routine – the opposite of articulation work (Star 1991a: 275). What is special with the ways in which simulation models direct the work of modelers is how they appear to be, to some extent, self-articulating since simulation modeling structures work in line with what makes the model work as a whole.

In fact, this helps us to understand why collaboration around a simulation model draws things and people together in a different way than field campaigns, which enable multiple agendas to exist side by side. In practice, a simulation model used for theoretical purposes metamorphoses from conceptual to technical forms within its role as an epistemic object. As discussed in Chapter Six, the form does not determine the role – epistemic objects exist in multiple forms (Knorr Cetina 2001). As an epistemic object, a simulation model tells us what the world is like but at the same time, it is the world. In making the “outside” fit into this world, there is little room for flexible interpretations, given the aim to achieve both theoretical stringency and “good” results. Facilitating collaboration (interconnection) between simulation modeling and field experimentation therefore requires a reliance on parameterizations as boundary objects, which provides the flexibility that enables the center to grow by structuring (without coercing) the practices drawn into it. I will return to this after the next section.

An important point at this stage however, is that the discussion above also illustrates how a focus on practice (simulation modeling) rather than on the
object (simulation model) brings questions of sociological interest to the fore, a difference I briefly discussed in Chapter Two in relation to the presentation of the concepts epistemic object and technical artifact. From the point of view of practice, it becomes impossible to (implicitly) suggest that simulation models are essentially “multiplex” and “unfolding” when these are characteristics they achieve only when they play the role of epistemic objects (see Merz 1999 cf. Knorr Cetina 1999). What becomes important is to look at practice in relation to the object in order to see the social consequences of this in terms of, for example, the organization of practices.

The Politics of Simulation Modeling

Let me briefly return to the question of the role of things, which I discussed in the introductory chapter. Although I find the inclusion of material objects into sociological analysis important, not least in sociological studies of science (and technology), I have discussed in previous chapters some of the reasons why I do not follow exactly the program proposed by Latour (e.g. 1991/1993; 2003a), and the actor-network approach more generally. At this point, I want to discuss some of the ways in which this program itself has difficulties in addressing some important questions about the role of things, in particular natural things, in scientific practices.

A fundamental question for actor-network stories has been to investigate how the division between humans and nonhumans – Society and Nature – is settled (see e.g. Latour 1987; 1991/1993). As a consequence of this quest and the view on explanations as such, actor-network researchers can only study one fact construction project at a time and should not seek to generalize from several projects. They cannot say anything about the role of things in the machineries of knowledge production (cf. Knorr Cetina 1999), i.e. about possible patterns of how the divide between Society and Nature settles in different, sometimes partial, types of scientific production processes. Furthermore, and as I have indicated on a number of occasions throughout the thesis, there are various problems with analyses of scientific practice that are exclusively based on its products (articles, reports). In this context, an important question concerns the consequence of a semiotic approach to scientific writing in terms of understanding different types of practices in relation to the role of natural things. With a semiotic understanding of material agency, is there any difference between post-structuralist (postmodern) semiotic analysis and sociomaterialist semiotic analysis from the point of view of material agency in practice? Thus, to conclude, studies of the written products of science
from an actor-network perspective in principle fail to address the roles of material agency in different scientific practices, in other words, the translations and transformations in configurations of (natural) things and artifacts taking place inside scientific production processes such as field experimentation and simulation modeling.

As simulation models shape interaction to an increasing extent, they grant increasingly less influence (“agency”) to the natural things they represent and replace – the clouds, air masses, droplets, and aerosols. In the climate arena, climate modeling influences the organization of scientific practices. Considering that both modelers and experimentalists shape our view of reality with their practices, what happens if one of these practices starts to shape the others? What happens if experimental practices are translated in order to exclusively produce data for the purposes of simulation modeling? While questions about the consequences of this for the subworlds await the following section, questions of material agency are important, especially from the point of view of Latour’s (2003a) discussion of the “politics of nature” and the extended view of democracy in the collective – consisting of both people and things. However, questions about the role of things in the (interconnected) production machineries of science are beyond the scope of what an actor-network analysis can answer. I argue that my focus on the artifacts in fact construction, their use, and role in practices – which have been referred to as intermediary aspects of knowledge production (cf. Knorr Cetina 1999; 2001) – lead to important questions also about the role of natural things in these practices. While this study does not provide answers to these questions, I agree with Pickering (1995) that there is a need to conceive of (some form of) material agency, without retreating to a semiotic understanding of it, if these questions are to be addressed in more depth.

Furthermore, I would also like to reflect on questions related to the co-production of natural and social orders (cf. Jasanoﬀ 2004a) in relation to simulation modeling. In the case of climate change, climate modeling produces a form of conceptualization and representation of the Earth as a system where processes at the molecular scale are incorporated through the representation of large-scale patterns. Climate models and the relatively new development of Earth Systems Models represent extremely complex processes. What is the consequence of this representation of the atmosphere and other parts of the climate system for the role of science in shaping our (understanding of) reality? This has implications and consequences for how we deal with what we define as reality, for instance how to reduce anthropogenic influences on the atmosphere. For example, the concept of

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The difference between working with translations/transformations from matter to form (using measuring instruments to record, for example, the behavior of bubbles) or translations from theory to code to model output should in no way be confused with (the scientists’) ideas about what is closer to or further away from “truth” or “reality”, which is an asymmetrical standpoint (cf. Wynne 1996: 379).
greenhouse gas emissions defines international priorities, both with regard to
global environmental change and related issues such as poverty, inequality,
land-use patterns, and global consumption, but is this necessarily the most
important, acceptable or effective way to solve the problem (van der Sluijs et
al. 1998: 318)? Is or was it the only way? Previous work has suggested that
orderings of nature and society – science and politics – are interdependent
and that the scientific order of global climate prediction and the political
order of global management and universal policy control mutually construct
and reinforce one another, as based on deterministic processes, smooth
change, and scientific control (Wynne 1996: 372, see also Miller 2004;
2005). How do processes like this take place in other questions and arenas?

Representations that present a smooth sequence of events cannot admit
the local, unique, and unexpected (cf. Star 1995a: 275). Without suggesting
that different methods of investigation produce realities, they do at least
shape our understandings of reality, and as a consequence, political decision-
making intended to change the conditions of our world, especially
considering how scientific knowledge is gaining increasing importance in
politics (e.g. Beck 1992). Accordingly, if the projection of ordered realities
is not a question of ontological politics (cf. Mol 1999), it is at least one of
epistemological politics.

But perhaps this differs depending on if we consider knowledge
production within the social or natural sciences? Social reality can be
affected by our understanding and knowledge of it and, according to the
extensive discussion on performativity (especially with regard to economics
and the construction of markets), is even created and shaped by it (e.g.
Callon 1998). Material reality is more conventionally conceived of as
given, unaffected and independent of our conceptions of it. Relating this to
simulation modeling, an interesting question is what happens when social
science meets natural science, for instance in epidemiology or to a growing
extent in research about environmental problems? These are generally
conceived of in terms of their natural “causes” and social “responses” but
environmental problems in the industrialized world are mainly the
consequences of how we live in it (cf. Beck 1992). Why then not speak of
“social” causes and “natural” responses? Thus, while social and natural
orders are traditionally studied as separate from each other within the
frameworks of social or natural sciences, some topics make evident how
social and natural orders cannot always (or perhaps ever) be studied in

315 The performative idea of science and society was evident in earlier actor-network accounts,
for instance in Latour’s (1996c) juxtaposition of the ostensive and performative view of
science, but it has become a more salient theme in more recent contributions, which have
gone further in acknowledging the fluidity of ontologies across networks (see e. g. Law &
Singleton 2000; Mol 1999; 2002). See Callon (1998) for a specific focus upon the
Performativity is also an aspect of feminist writing, see e. g. Butler (1990).
isolation. What is variable, what is constant and what assumptions about natural and social orders are embedded in the simulations of these types of phenomena – the hybrid objects (cf. Latour 1991/1993) of our modern world?

Intersection Processes: A Clarification and Discussion of Boundary Objects

This section attempts to move towards general sociology on the basis of an extended discussion of boundary objects. This is of particular concern in this work due to its origin in both the ideas of social worlds and translation processes, as related to the actor-network approach. The concept of boundary object was developed to understand how different groups or social worlds could cooperate while retaining their heterogeneity. This implies that the analysis starts from a heterogeneous collaboration, for instance an interdisciplinary research program, in order to see how collaboration works, rather than focus on the central product or object of an existing collaborative network. This is in line with the interest in collective practices rather than the outcome of those practices (e.g. Becker 1982) and also with the definition of boundary objects as marginal to those worlds or subworlds participating in the collaboration (Star & Griesemer 1989: 412) but, at the same, time crucial for the collaboration.

However, the notion of boundary object is not only related to social world theory. It is also an attempt to tie it to, and modify, the translation model. Thus, objects that are read differently by different people (Henderson 1999), or objects that are used differently by different groups (e.g. Kwa 2004), do not automatically function as boundary objects unless they also serve different translating purposes. It is in combination with the translation model that the concept “boundary object” acquires analytic potential. Yet the examples above suggest that multiple uses and flexibility rather than translation capabilities have come to the forefront in previous attempts to use the concept. If boundary object is used simply as a label (cf. McGee 2000), applied to objects a priori to signify objects on boundaries (or constituting the boundary), then the capacity of the concept to explain how objects achieve their role as mediators in social practice is lost.

Trading Zones and Transaction Spaces

The management of boundary objects – including their construction – is conducted (by scientists and others) only when work coincides (Star & Griesemer 1989:412). At intersections, social worlds meet through the exchange of resources that takes place. Similar to intersections are what
Peter Galison (1997) refers to as *trading zones*, boundary areas of scientific practices where interaction takes place.\(^{316}\) He analyzes the exchanges and intense collaborations between three sub-cultures of nuclear physicists – theoreticians, experimentalists, and engineers – in the 20th century. These traditions remained intact, preserved inside the collaboration, while the coordination of exchange took place around the production of two competing instrument cultures, which ultimately joined. Galison (1997) observes how their exchanges of goods can be compared to the analysis of incomplete and partial relations among people from different cultures that trade in a trading zone.\(^{317}\) Each tribe may bring to this interaction and take away objects that they interpret differently.\(^{318}\) The question of how different groups come to work together is similar to the question that Star and Griesemer (1989) seek to illuminate with the concept of boundary object. I therefore propose to connect trading zone and boundary object.

Nowotny et al. (2001: 146) seek to extend and generalize the metaphor of the trading zone to go beyond interactions among scientific subcultures or subworlds to exchanges that take place across disciplinary and institutional boundaries and propose the notion of *transaction space*. In these spaces, transitory transactions take place in which supposedly all parties have something to gain. As I have been discussing boundary objects in terms of relations both within the social world of meteorology as well as with funding agencies, I consider this to be a fruitful development of trading zones and will therefore refer to transaction spaces in the following discussion. The transaction space surrounding boundary objects is a social space that acts as a “platform” for them. At the same time, the possibility to trade depends on establishing the boundary object: The transaction depends on multiple translations (of and by experimentalists, modelers, funding agencies), which depend on a boundary object.\(^{319}\) When the intersection is asymmetrical in terms of dependency, the boundary object presupposes an arena, like

\(^{316}\) Although this notion is adopted from anthropology, I only intend to use and develop it on the basis of Galison’s (1997) definition.

\(^{317}\) For example, Galison (1997: 803f.) mentions the contact between peasants and the landowning class in the Cauco valley in Colombia, which includes many levels of exchange involving money, but the understanding each has of the exchange of money is different.

\(^{318}\) An object with a highly symbolic or even sacred value for one tribe may represent an entirely banal object for another.

\(^{319}\) Fujimura (1992a) suggests that what she calls *standardized packages* (Fujimura 1986, 1988) facilitate both collective work by members of different worlds and fact stabilization. If an interface is the *means* by which interaction and communication is effected at the places where two social worlds intersect, a standardized package is an interface that operates simultaneously in *many different social worlds*, a mechanism by which multiple intersections occur (Fujimura 1992b: 124). Thus, standardized packages appear to have some similarities with immutable mobiles (e. g. Latour 1990). However, the point here is that the present discussion is more concerned with the *transactions* and *translations* involved in the relationship between social worlds.
climate, in order to enable translation and transaction (exchange) in a transaction space.320

Transaction Spaces and Boundary Objects in Meteorological Research

Networking or cooperation in a more loose sense was discussed in three of the analytical chapters. On the basis of results presented in these chapters, I discuss the three different cases (field campaigns, model evaluation, and parameterization) from which to consider collaborative (networking) efforts across social subworlds in relation to the discussion of boundary objects.

Field campaigns constitute symmetrical intersections between subworlds of the experimental world. Everyone has something to gain. Participants produce data for their own use but also for sharing with others. On the basis of their data, participants produce articles, sometimes with the support of data produced by others, or supply others with data, and thereby add their name to the list of authors if the result is published as an article. Field campaigns become temporary transaction spaces where heterogeneity remains and where subworlds also continue to exist within the projects. In these transaction spaces, all types of measurement data – as what is exchanged – may not have the same value but they are generally interpreted in a similar way and fill a similar function for the participants. Measuring instruments however, function as boundary objects and serve slightly different purposes for the different participants and thereby constitute important elements of collaborative work.

Turning to the relationship between simulation modeling and field experimentation, I have suggested that modelers interact with experimentalists because they want data in order to test and compare with their models. Model evaluation is an aspect of most types of modeling practice. Comparison of output and data forms an important part of this as an intersection and thus a potential transaction space between modeling and experimental work. Yet experimentalists produce data largely independently of what the modelers need for these purposes. Although there may be a general aim to produce usable data for model comparison, there seems to be limited communication about what modelers need or what experimentalists need.

Boundary-objects seem to emerge from horizontal social interaction (van der Sluijs et al. 1998: 312) but I suggest that it is important to distinguish this from symmetrical (mutual) or asymmetrical resource-relationships. In symmetrical relationships, both parties depend on an exchange of resources between each other. In asymmetrical relationships, dependency is unequal, such as in the case of model evaluation, but this does not mean that an asymmetrical dependency presupposes hierarchy. Asymmetry rather corresponds to the character of the “incomplete” or “partial” relations that Galison (1997) describes. Nonetheless, there may be something to gain in an asymmetrical relation because of a boundary object and the translations it enables.

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can supply. The transaction of data is difficult since experimentalists and modelers tend to be interested in different types of data and data processing. There is little gain, and too much work, for experimentalists, if they only serve modelers with data and cannot use it for their own interpretation and production of articles. The different use and status of data is also a potential source of conflict. This relation is asymmetrical and the intersection is unsuccessful as a transaction space because modelers do not have much to offer in return for their use of experimental data. However, as we have seen in Chapter Eight, climate modeling can serve to legitimize the need for field campaigns, but translations in funding applications (writing practices) are not necessarily combined with equal changes in material practices.

Climate change as an arena was the point of departure in Chapter Eight in which I discussed the tensions involved in defining “climate research” and its use as a label to attract attention and funding. But climate change enables cooperation as well. Like model evaluation, parameterizing is an asymmetrical intersectional boundary from the point of view of modeling and experimental work but through climate as an arena, the situation changes.

Parameterizations as intersections are strengthened through the construction of climate models, which incorporate more processes than other atmospheric models. Yet this is not the most important point. Parameterizations become boundary objects because they enable multiple translations through their position in an arena where there is not only one other social world or subworld to relate to (as in the case of model evaluation). Experimentalists adapt (translate) the interests of funding agencies, from where experimentalists can hope for a potential return, based on the idea that the climate research label is a way to receive research grants. Worth noting is that it is ultimately the “climate research”-label that is at stake in this analysis. The link to parameterizations is crucial because without it, experimentalists lose their attachment to climate research (and in particular, climate modeling) and consequently their translation capability in relation to funding agencies.

Climate as an arena and climate modeling as an obligatory point of passage are key features of this situation because it is the resource that modelers make use of in transaction. While experimentalists tend to find the label “climate research” useful independently of whether their potential contribution with data is realized or not, I illustrated how this is debated. Parameterizations serve different translation purposes when they appear as boundary objects in grant proposals, and the exchange of data between experimentalists and modelers does not work equally well in practice. The

321 While simulation modeling provides a legitimizing frame in which observations become important for the production of reference, this is not the type of exchange that the discussion of transaction space refers to.
flexible meaning of data (in exchange) and parameterizations (as boundary objects) also cause tensions.\textsuperscript{322}

But what happens with the social world of experimentalists if parameterizations guide experimental discourse on paper and work in practice, in other words, if the interests of experimentalists are translated more completely? Does the social world remain intact or is it part of a segmentation process in which both experimental and modeling practices together form an organized social world of climate science? To the extent experimentalists start to follow the modeling way of organizing their practices, it signifies a changing social subworld (cf. Galison 1997). This view approximates Steve Fuller’s (1996) remark that Galison’s (1997) analysis of trading zones does not take the long-term effects of pursuing a particular trade language into account. In line with this, I suggest that stabilized boundary objects may lead to change. If the purpose of data collection changes, and thereby the meaning of data, so does the world of experimental practice. This also follows from the translation model because translation also means change. In its pure form, however, this model cannot tell us why this change came about since it does not inform us about the practices and social subworlds of field experimentation and simulation modeling and how they interact in it more generally.

What about simulation modeling? The transaction space is a space for experimentalists and the model constructors. There are also other scientists involved in climate as an arena, such as the users of climate models, who have a central position in the climate arena without participating in the trading zone. What makes some practices and their related practitioners absent from the arena by resisting translation? The relationship between modelers and experimentalists in relation to climate as an arena in general and the position of climate models as obligatory points of passage in particular does not only affect experimentalists – in the sense that their concerns are translated into (climate) modeling. An important feature of obligatory points of passage is that they affect all actors around them (Callon 1986). Thus, while this discussion has focused on what happens in the experimental world, it does not mean that the modeling world remains the same (cf. Latour 1983). A likely change is an increased focus on parameterizations and the incorporation of more process descriptions. Rather than striving towards reduction and simplification, this means more detailed model construction. Modelers working with representative modeling and predictive modeling respectively may move further apart, causing a growing divide in modeling.

\textsuperscript{322} Through the notion of transaction space, it also becomes clearer how and why experimentalists criticize the use of data in the hands of modelers: after transaction, the object – in this case data - changes its value and meaning (cf. Galison 1997).
To summarize, field campaigns enable transaction but also preserve the differences between the actors within the collaboration, including their different goals. Both field campaign projects in themselves and measuring instruments function as boundary objects in this. Climate change and climate research generate resources for various types of participants too, but these resources create various claim-making activities (cf. Aronson 1984). These claims strive towards the establishment of some kind of transaction space on the basis of the boundary object, involving both consensus (the importance of climate research) and conflict (how to do climate research and the role and features of parameterizations in this).

Contributions and Outlook

This study has aimed to empirically and theoretically contribute to sociology and social studies of science and technology. Empirically, my study has revealed the heterogeneity of the different research practices that together make up a discipline, and how these are related. Furthermore, the study has added to the understanding of scientific collaborations as being made up of networks of practice, connected as a result of translations of interest. Climate simulations have been fundamental for the understanding and conceptualization of climate change and the thesis has added to the existing sociological research about the climate change question by showing how meteorological researchers align their research problems to climate modeling and the effects this has on the relationship between different groups of meteorological researchers.

In addition, this thesis has analyzed simulation modeling practice. On the basis of my findings, I suggest that it is potentially fruitful to conceptualize simulation models as *materialized* models, i.e. as objects in which abstract models are intertwined with technology in order to produce virtual realities within a scientific framework. Recognizing this feature of simulation models is a precondition for a sociological understanding of simulation modeling practices. Although the notion of center of calculation captures the centralizing and structuring function in relation to modeling practices, it is also important to develop our methodologies and theoretical tools to approach these matters. My discussion of features of simulation modeling is an attempt to move in this direction by suggesting characteristics that should be taken into account in such developments.

This book has further demonstrated the fruitfulness, but also the drawbacks, of combining a focus on activities and perspectives with an attention to the role of material things developed in social studies of science. Through this combination, it has added to the understanding not only of how networks are built, but also how discursive networks do not always correspond to material networks. Practical commitments are not as easily
changed (translated) as interests, i.e. the way interests are defined in the actor-network approach.

Finally, an interesting topic for further investigation is to see if and how the developed concept of boundary objects discussed above could be useful in analyzing social processes in other science worlds. For example, how environmental questions – as arenas – interact with the practice and organization of social worlds of science involved in knowledge production in relation to environmental issues, and how these also interact with political actors, international organizations and industry. The occurrence of boundary objects is not limited to scientific practice but may develop in any forms of social networking/collaboration in which different social worlds or subworlds seek to participate. Thus I suggest that by developing the boundary object concept on the basis of my findings, studies of science as practice may contribute to the analysis of changes in social worlds and social life more generally.
The first time I was introduced to social studies of science and technology was during one of the classes in Modern Sociological Theory in the spring of 2001. This field has since enabled me to combine my passion for both the social and the natural sciences, which has now merged into this thesis.

Introducing me to the writings of Bruno Latour, is but one of the many things for which I owe Árni Sverrisson the greatest debt of gratitude. At the same time as Árni gave me complete freedom to pursue my own project, he has made a profound impression of my thinking. He has also made me a much better sociologist than I would have ever been without him as my supervisor. Árni has always read every draft with interest and optimism, and I praise his ability to recognize and to encourage the development of my (hopefully) incipient ideas – sometimes even before I knew how to formulate them myself – rather than pointing out what was lacking in my work. Some of the other things which I also admire and appreciate about Árni are his intellectual sensitivity and intuition, not only in terms of academic work, but also on a more personal level. With perfect timing, Árni knew when it was time to be demanding, but also when I most of all needed some cheering-up. Thank you!

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