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The Social Life of Climate Change Models

Anticipating Nature

Edited by Kirsten Hastrup and Martin Skrydstrup

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Preface and Acknowledgements

This volume is the outcome of an interdisciplinary workshop that truly deserved its name. Anthropology, geography, and theoretical physics mixed with science and technology studies and the history of science. The reflexive space between the disciplines proved to be a major resource for a rethinking of nature itself. Analyzing climate modelling as a practice by which people across the globe seek to anticipate nature's course, so as to be able to respond properly to the current challenges of global warming, the chapters below show a rare degree of self-reflection. This allows for an important recognition that whether one engages with lay people or with scientists, the modes of reasoning are comparable and may lead towards similar goals. Arctic hunters, Tibetan pastoralists, Pacific fishermen and other people engaged in forecasting the future on the ground share the wish to understand what is happening to their environment with climate scientists working in the same vein, if from a different perspective. As social agents they all attempt at defusing the major uncertainties with which the increasing numbers of people on the Earth are faced. People are placed differently in the world, and have different experiences upon which to act, but they have a common interest in establishing some sort of certainty vis-à-vis the waves of fear that follow from the process of global warming—quite irrespective of its root causes.

The chapters below are revised versions of presentations to the workshop; an additional paper was given by Myannah Lahsen, which unfortunately could not be included because it was spoken for in another context. Other participants in the workshop were Anette Reenberg, Frank Sejersen, Bruce Huett, Jonas Østergaard Nielsen, Laura Vang Rasmussen, Mette Fog Olwig, Astrid Andersen, and Christian Vium. Their genuine contributions to the conversation are gratefully acknowledged. Rasmus Hastrup is thanked for his help in streamlining the draft, linguistically and typographically. Henny Pedersen was in charge of the practical and economic organization of the event, which was carried out with her usual skill and commitment to the task.

The larger context of the workshop was a major research project at the University of Copenhagen, *Waterworlds*, financed by an Advanced Grant from the European Research Council, and studying the social responses to environmental challenges across the globe, which are more or less explicitly tied up with climate change in the minds of most. As the privileged holder of that grant, and on behalf of the entire group that works under its auspices, I want to acknowledge the debt to the ERC. The grant made new field studies possible as well as a series of workshops and conferences, which have enabled us to discuss our findings within a larger international community of scholars. Finally, I want to thank Martin Skrydstrup for his fine job as a co-reader when the first drafts came in. Beside the editorial work we shared the enthusiasm of discovering a remarkably coherent result of our interdisciplinary exercise: a shared understanding of the deeply *social* implications of climate change models.

Kirsten Hastrup
February 2012

1 Anticipating Nature

The Productive Uncertainty of Climate Models

Kirsten Hastrup

The present volume addresses the profound question of how one can anticipate nature's course, scientifically and practically; the question is answered from the points of view of several academic disciplines: anthropology, geography, science and technology studies, physics, and the history of science, brought together by a shared ambition to open up a new space for interdisciplinary discussion. This ambition is spurred by the intensified discussion of climate change and the need to rethink the entanglement of natural and social processes contained within the figure of "climate". Possibly the most comprehensive token of this entanglement is found in the notion of the Anthropocene now replacing the Holocene as the name of the present geological era. The Holocene started after the latest glacial period and thus comprises the history of humanity since the invention of agriculture and the emergence of the earliest known complex, urban societies (Anderson et al. 2007). The technological and social advancement since then has now come to a point where it is no longer possible to understand the Earth as independent of human influence, hence the Anthropocene (Ehkers & Kraft 2006). After more than 10–12,000 years of agricultural development, on top of which we have seen some 200 years of intense industrialization, an exponential global population growth, and a massive urbanization, the human fingerprint is everywhere: on the land surface, in the oceans, in the atmosphere. The Earth is so deeply marked by human activity that climate cannot be understood without acknowledging this. In that sense, we are at "nature"s end" (Sörlin & Warde 2009).

By implication, it is no longer possible to entertain a notion of a self-regenerating nature, beyond the human domain. Humans are all over the place, not only as destroyers of nature, of course, but also as providers of solutions. It is part of human and social life to take action. For social agents to act consistently and to take responsibility for their community, they need to have reasonably well-founded expectations to the future. In this volume we analyze the processes by which such expectations are established within diverse social and scientific communities. Through the case studies presented, the question of scale is linked to particular knowledge practices, and it is shown how the general human capacity for anticipation is shaped and stretched within such practices.

The practices by which people deal with the challenge of projected climate changes may be based on statistical models and computer simulations or on direct experiences of greater weather variability and intensified weather events; at both ends of the spectrum, they are dependent on records, experiences, and observations. The foundation of climate modelling is empirical across the board; some models use palaeo-climatic records, others rely on personal recollection and direct experience. Furthermore, all of the anticipated scenarios build upon a knowledge that is captured somewhere, for instance in ice cores, atmospheric compositions, geological traces, place names, memories, bodily sensations, stories, simulations, diagrams, or some other medium that may close the gap between past experience and future expectation by incorporating them into a comprehensive model. The elements of the model may then be processed and transformed into realistic climate scenarios. Although there is certainly a multiplicity of processing modalities, these may be seen as variations over the common theme of climate modelling.

Climate modelling takes place along diverse lines of reasoning and at different scales, as we shall see in some detail in the chapters below. In the process of modelling, nature is reconceptualized and the future reimagined. The volume focuses on the intricate links between the modelling, the configuration of nature, and the human anticipation. The ambition is to establish a common ground for further reasoning across disciplines and scales. This is pertinent, because on the one hand, climate change is fraught with ambiguities even as people agree upon its reality (Hulme 2009), whereas on the other there is an urgent need for mitigating actions. Speaking across disciplines, as we do in this volume, is not driven by a simple wish to mix knowledge, but by a sense of generic interdisciplinarity, as suggested by Marilyn Strathern, viz. a means to address problems seen to lie athwart specialisms (Strathern 2005: 127). This certainly applies to the problem of climate change.

Anthropologists have addressed local implications of climate change all over the world and contributed to the discussion of the perceived turning points between ordinary weather variability and permanent climate change (Strauss & Orlove 2003; Orlove et al. 2008; Crate & Nuttall 2009). For all the merits of local ethnography, anthropological studies of climate change of this kind have been sidelined as more or less irrelevant outside of the discipline itself (Strathern 2005). To talk across disciplinary boundaries, anthropologists need to cultivate a more comprehensive interest in the interpenetration of local and global climate issues and of different registers of knowledge. This would link up anthropology with recent developments in other social sciences such as sociology, political science, economics, and science and technology studies. In view of the fact that the climate uncertainties are multiplying around the globe, everybody has to negotiate the boundary between manageable risks on the one hand, and fears that are unknown both in origin and scope on the other (Bauman 2006, 2007; cf.

Douglas 1985; Douglas & Wildavsky 1982; Beck 1992). This distinction between known or at least identifiable *risk* and unknown and maybe even unknowable sources of uncertainty and *fear* (partly owed to Bauman 2006: 100) is important for our concern with anticipation. Among the unknowns on the ground, so to speak, are the economic consequences of shifting environmental conditions and new energy scenarios (Stern 2006). Basic food security is at stake in some regions (Lobell & Burke 2010), and this again has severe implications for political stability and international security concerns (Vanderheiden 2008).

At the other end of the scientific spectrum, the natural sciences have produced a vast amount of knowledge of the current climate trends. There is no absolute consensus about the details or about the root causes of the development, but the larger trend is well established: the globe is warming (IPCC 2007; Richardson et al. 2011). Not least because of this remarkable agreement, large groups of people all over the world are worried; when the scientific projections feed into current experiences of extreme weather variability, risk may turn into fear. As Mike Hulme has suggested, climate might also be acknowledged as a resource for intellectual creativity, however (Hulme 2009). Either way, there is little doubt that the liquidity of the climate scenarios is itself a social driver; it infiltrates the perception not only of the environment, but also of social life and knowledge. The anticipated climate change seems to affect our total "social imaginary", comprising the social, political, and moral order (Taylor 2004). If nothing else, then because the social is now so manifestly entangled with the natural, and therefore increasingly difficult to control.

This is the basis for the question posed in this volume of how one can anticipate nature, practically and scientifically, so as to make the world *work*. Climate change belongs to the interface between natural and social histories and highlights the necessity of establishing a closer relation between diverse disciplines if we are to create a common ground upon which we can reimagine a shared world and rethink received notions of nature and culture.

Human agency is based as much upon future expectations as upon past experiences (Hastrup 2007). Expectations about natural developments are not outside of the human and social realm. To understand how weather variability is incorporated and projected into a horizon for the future, a diversity of perspectives is called upon to creatively explore the processes of reasoning by which people envisage the future, and which may link models of multiple scales to each other. This is the objective of the volume.

To open up a space for reflecting upon this objective I shall, first, present the general *process of modelling* in terms of some basic components. Second, I identify some *modes of configuration*, i.e. ways of capturing and disseminating the knowledge about climate change gained through the process of modelling. Third, I shall discuss some *modalities of nature* with a view to elucidating the destabilized ontology implied in the new knowledge. Fourth and finally, I shall return to the *practices of anticipation* as embedded within different modes of reasoning, and as presented in the individual chapters.

THE PROCESS OF MODELLING: FIVE COMPONENTS

Evidently, scholars belong to the same earthly world as other people, and although climate change may mean different things to different people, these have a lot in common at the level of reasoning itself. Talking across different meanings and vocabularies is a basic anthropological enterprise, serving also as a model for interdisciplinarity. Both "offer diversity as at once foundational to an enterprise *and* as innovative exploration" (Strathern 2006: 198). Before we can substantiate the merits of this claim, we must centre our attention on the process of climate modelling itself, by identifying some components that connect rather than separate the various disciplines and scales, and which make the conversation possible. These components are observation, formalization, experimentation, projection, and action. They shall be presented here in the attempt to itemize and generalize the process of modelling as such. As will transpire, the components are not totally separate. Rather, they are part of a comprehensive, iterative process of making an argument about something, which cannot be known, but must be intimated to allow for some kind of action.

Observation and experience are cornerstones in all learning processes. People from all walks of life know their surroundings from paying attention to them, from moving within them, and from talking about them. Among people in general, scientists stand out not by their being fundamentally different, but by their attention being more methodological and more systematically recorded. With respect to climate change, the attention may focus on holy mountains, ice cores, cyclones, or atmospheric turbulence, but wherever the observational or experiential material is produced the implicit point is an acknowledgement of the agency of matter (Latour 2005). From each their perspective, or on each their scale, the observers are implicitly singling out what matters, and how it matters in the larger climate change equation. One basic question is how things are made to matter within the model in focus, and by which computational or conceptual tools the case is made. In the process of dealing with this question, new tools are explored and hierarchies of understanding established. It is also shown how modelling is in fact part of everyday attempts at understanding and anticipating nature.

Formalization of climate change observations implies an establishment of rules and regularities implicit in the material. Such regularities may be established on the basis of diverse mechanisms, such as enumeration or mapping, leading to statistical correlations or (mental) diagrams. We should note here that rules in the social domain *are* regularities (not laws)—possibly also in the natural domain, even if the rhythm may be slower. To establish solid rules in both domains, now recognized as mutually implicated, one must identify convergences and confluences in the material at hand. In some parts of the material, events abound and may form long series of observations; in other parts, events are few and far between, and direct observation must be supplemented by proxy data, identified as such because of other observed regularities of relations. Event richness and event poverty may be equally important to note, when formal pictures of regularities are established and used as the baseline for experimentation.

Experimentation implies some kind of manipulation with forms, computationally, mentally, or experientially. Once a form has been established that depicts the regularities, experimentation allows for trying out the not-yet-realized, the possible; this takes place in a social realm, populated by other people, other scientists, other manipulators. One could say that experimentation allows the objects to "talk back", or the matter to really matter—at least within the model. Experimentation is a means of testing some of the established regularities, and possibly to revise the rules. It allows for trying out when and where something matters more or less. Different kinds of experimentation may be juxtaposed, revealing different qualities of the matter, in the manner known from e.g. the complementary perspectives on light as either particles or waves. In the process of experimentation, which is of course a social practice, the limits of one's knowledge become conspicuous. Although it seems precluded to experiment with climate in real life, experimenting with models is possible, as is the experimentation by comparison in real-time experience. Walking on the sea-ice in the High Arctic and reading it for clues about thinning and cracking is in itself a practical experiment, akin to a peer-review of different scenarios.

Projection of probable futures is the principal outcome of such experimentation; in some cases the projections are expressions of probability, in others of well-established rules. Implicitly they are often seen as predictions, yet this is an entirely different matter (in principle), applicable only within a limited range of mechanical systems. As Edwin Ardener so convincingly argued, predictivity fails at the only moment at which it is truly important (1989: 53). Because prediction always rests on repetition, it will of course flounder when repetition does not occur. Although, clearly, social and environmental (so-called) systems may tend towards repetitive inertia for some time and along measurable parameters, and thus allow for a degree of probabilistic reasoning, it is difficult to know when inertia is overturned, and repetition fails—and with it: prediction. This said, the strength of models is measured by their capacity to project probable futures on the basis of experimentation and simulation.

Action, finally, is a possible outcome of the projections, whether in terms of everyday or political action; but it is also a function of one's understanding of the plot in which one takes part, including its temporal and spatial extension (Hastrup 2004a). Climate actions take multiple shapes; they may comprise a rerouting of well-established trails, a reshaping of local drainage systems, new measures of governance, mitigation or education, new demands for political action, or for new scientific knowledge. This again may lead to new observations or to the incorporation of new externalities into the models that may alter the projections. When projections are translated into actions, they will be subject to a negotiation of multiple interests, and to a highlighting of particular projections at the expense of others. The confluence of particular models in various centres of action and authority in itself may result in widely variegated actions and measures of mitigation.

With these five components in mind, it should once again be stressed that they are intimately linked. They constitute an iterative process of arguing about nature and social action that is never devoid of interest; politics and policy are at work throughout (Giddens 2009). The process of modelling itself implies a particular point of perception. Although at first sight this may be seen as detracting from the validity of models, this is in fact also part of its strength. The models may be trusted because the process of modelling is open to both new observations and to experimentation. This is where multiple forms of climate modelling emerge as a sound response to the converging uncertainties about the global environment, and as constructive ways of making past and present observations and experience relevant to the unknown future, to which there seems no direct access by way of the inertia of repetition.

In many ways, the main purpose of climate modelling is arguably to substitute for missing empirical data, mostly because the data are literally beyond reach in both time and space. In the process of substitution, selections are made as to what matters most. It is therefore worth stressing once again that climate modelling is socially embedded, as is the interpretation of the models, once they are set free and get a social life of their own. Models are therefore never allowed to stand still, even less so because they are by definition exclusive whereas climate is not. This is why there is a built-in humility in most processes of climate modelling; they can never stand alone, and never claim to be more than approximations.

MODES OF CONFIGURATION: NARRATING, COUNTING, SIMULATING

The processes of modelling lead to particular configurations of climate knowledge. The intensity of the climate change debate over the past few years has served to reconfigure the thinking and understanding of "weather". Until recently, this would be the main point of reference when people would discuss the unprecedented downpour or the unusually fine moments of spring. Weather variability was a common enough feature, and seen as a function of a seasonal cycle; extreme weather events were known to happen, yet they were still seen as events. Somewhere beyond local experience and public parlance, "climate" would sum up larger regularities as established scientifically. As shown by Matthias Heymann (2010), in the 19th century when climate patterns were first scientifically acknowledged, they were established geographically, and only later in the mid-20th century were they seen in terms of atmospheric conditions.

The distinction between climate patterns and weather events is no longer clear-cut, however. Although analytically we may distinguish between them, even events are experienced within a larger pattern of regularity, framing both the average and the extreme. In an analysis of the English weather, Golinski (2003) shows not only how the weather is part and parcel of modern life, but also how the way people talk about it reflects a deep-seated uncertainty about a historical development that refers to much more than the weather as such. With more extreme weather events in recent years, the English claim that the weather becomes more and more "continental"—possibly reflecting scepticism about the European project, according to Golinski. He goes further and shows how the weather has generally been portrayed as unstable and unpredictable in periods that have also been marked by other kinds of historical turbulence, be it social, political, or economic. A transgressive moment followed the Great Storm in England (and elsewhere in the surrounding countries) in 1703, which provided the inducement to start making systematic records of the weather, in order to find a pattern. The event has remained in national memory ever since, and we shall hear why:

> On the night of 26–27 November 1703, an especially violent tempest tore across southern England and the Low Countries, uprooting trees, tearing down houses, and sinking ships. Hundreds of thousands of trees were uprooted, and hundreds of houses destroyed; estimates of human fatalities ran into the thousands, most of them lost at sea. The storm forcefully impinged upon the lives of the entire population and demanded some kind of explanation from intellectuals. In the whirlwind of printed pamphlets that followed, the central question was whether the event was to be seen as a divine admonition or punishment, or as the result of regular natural causes. (Golinski 2003: 19)

There are two reasons for bringing the Great Storm into the picture. Firstly, we hear a faint echo of the contemporary concerns in present-day climate change debate, where multiple cosmological models compete for authority. The why and how of extreme weather events are as pertinent today as they were in 1703; and like then, new scientific questions are posed, and the demands for swift answers are intensifying. Secondly, the story of the Great Storm reminds us how weather and climate are always configured in particular ways of narration. Telling about both patterns and anomalies is probably the most common way of configuring climate.

One of the characteristics of narration, so admirably analyzed by Paul Ricoeur, is its capacity to make a whole out of individual episodes through a particular emplotment (Ricoeur 1984). The succession of events is integrated and transformed into a configuration. Climate stories, too, depend on a particular plot for them to be convincing, meaning comprehensive and sufficient for people to take them at face value. All people, and not only the English, recount weather and climate on a regular basis; the configuration by means of narration is shared, but the plots will differ according to place and circumstance. In other words, different plots are construed with the local episodes and realities in mind. In the American Arctic, the glaciers are playing an active role in the emplotment of past and present climate (Cruikshank 2005), whereas elsewhere it may be the rain that constitutes the main plot-maker (Sanders 2003). Sometimes, the weather is configured in the condensed form of proverbial lore representing a small-scale narrative, as will be well known to most. Recently, a case has been reported from the Swiss Alps, where a saying goes: “If the rooster crows on the dung pile, the weather will change—or it won’t” (Strauss 2003: 48). Such ironic comments to the unpredictability of the weather come along with more trusted observations: “First love and the month of May seldom pass without a frost” (ibid.: 49). It is a way of configuring shared knowledge and concerns and to stress the sensory experience of being there, which no modern weather forecast can sustain.

As demonstrated by Golinski (2007), the keeping of weather diaries became a popular accomplishment in enlightenment Britain, and the noting down of extraordinary “meteoric events” as well as ordinary features of wind and weather as related to household economies brought the weather to public attention and into the domain of news. “These diaries, so often kept by clergymen, represent a reformation of an older, more communal and flexibly chronological culture of weather lore in which church bells were tolled and prayers offered to appease God’s wrath during storms and drought, if the magic of omens and prognostications, of saints days and astrological conjunctions persisted in popular culture, including mass circulation almanacs” (Daniels & Endfield 2009: 221). At the same time, a new science of climate dawned and systematic measurements of temperatures began. My point is that the enlightenment concern with weather and climate operated at the interface of local, personal observations of smaller or larger irregularities of nature, and a growing quest for an identification of patterns beyond the local.

This is still very much the case, even if—narratively—the focus has moved from weather to climate. The path from local weather diaries towards a (tenuous) global agreement about global warming has not been a straight one. Climatologists have fought over the truth, and political and military interests have infiltrated the “pure” science of climate along the way (Sörlin 2009). Yet, it is safe to say that climate change “is presently a Big Story, as both a world-wide chronicle of rising cultural consciousness among political elites and the population at large, as well as the grand, often crisis narratives of environmental change itself, notably those aligned to the graphic rising curves of global warming" (Daniels & Endfield 2009: 215). The crisis is often depicted in evocative notions as "burning embers" and "tipping points" (Liverman 2008). Along with it come other narrative constructions of individual responsibility, international carbon trading, and new markets for investment that will thwart the direst consequences (ibid.). The general point is that climate change becomes clad in particular rhetorical figures, producing and produced by a linear, narrative reasoning where the apocalypse can only be stalled by heroic intervention.

The substantiation of some of the narrative figures is delivered by scientific models; these, it should be noted, are often based on proxy data of past climates that are open to alternate interpretations, and which despite all their merits are "a little undersensitive" to the possible abrupt changes (Alley 2003: 1843). The models are fed by physicists (among others), working to understand the climate system in terms of energy flows that can be measured and represented mathematically (Ditlevsen 2009). Thus, the large-scale models often are twice removed from climate as such, first because they are based on proxy data, second because they have been transformed into mathematics for them to be open to experimentation. The mathematical representations of climate change rest upon a numerical reasoning, which leads us towards the second mode of configuration, that of counting—as distinct from the mode of narration.

By way of introducing this, I shall recount an example from my own field in North Greenland, where I work with a community of hunters (Hastrup 2009a, 2009b, 2010). I thus resort to ethnographic description to convey a particular encounter between modes of numerical reasoning, an arch-anthropological manoeuvre but also an illustration of larger epistemological schisms, equally found in interdisciplinary practice. My case concerns the number of narwhals, which somehow feeds into the comprehensive understanding of present climate and other concerns. The narwhal is the dominant cash-crop in the region. Both the tusk and the *mattak* (the thick layer of blubber and skin) are sold at a high price, and it is no surprise that the hunters are keen on the narwhal, arriving in numbers in late July, early August and populating the fjord running eastwards from the village for some weeks. The outer fjord has appropriately been named Whale Sound since the first Europeans reached its shores in the 19th century. In 2001, the narwhals of the fjord were counted from the air by biologists from the Greenlandic Institute of Natural Resources, using new digital photographic equipment and a digitally superimposed fine-meshed grid. The count—corrected for estimates about submerged whales, etc.—arrived at a little over 4000 narwhals in the fjord during the selected weeks (Heide-Jørgensen et al. 2002).

In 2007, I conversed with a local inhabitant who told me about the count and who had been allowed up in the aircraft to see how the counting was done. I was curious, not simply about the biological monitoring per se (of which I knew nothing at the time), but also of its level of convergence with other local estimates. I asked whether the assessment made by biologists matched the hunters' own idea of how many whales there were. My friend took some time to answer: "It is difficult to say; how many is many? The hunters sometimes say that there are many, but they cannot know for certain how many there are. If they just get one, and there are more left, then there are many". The point is that there seems to be *enough*. This feeling also applies to polar bear and to walrus, otherwise threatened by the warmer weather, because in the far North there is still no concrete indication of a lack of animals (Asvid-Rosing 2002; Born 2005; Born et al. 2011). The Greenlandic stock may be shrinking in the eyes of biologists, who assess the numbers from above; but seen eye-to-eye with the prey, there is still more than enough. In other words, the stock is virtual and rather irrelevant in relation to the hunters' experience.

In 2008, when I was back during the whaling season, the hunters caught approximately 70 narwhals, I was told—just about the allotted quota. Compared to the estimated number of the pack in fjord, this does not seem excessive, and the hunters would have liked to go on. Although not exactly sub-standard in terms of actual catch, there was a sense that the hunt was somewhat circumscribed by the quota. This feeling was further aggravated by the glacier meltdown; the thundering and rapidly calving glaciers in the bottom of the Whale Fjord produced giant waves, underscoring the fragility of the kayaks, the basic technology of the hunt. In general, there was a sense of unsettlement comprising several factors, intertwined in the practice of hunting.

The hunters' unease about the quota was not simply related to the actual number as allotted to them, but also to the externally induced obligation to count what should not be counted, because nobody would ever hunt in excess of human needs. The key issue, as I first saw it, was the absence of any explanation as to how the c. 4000 estimated narwhals were translated into the number (70) of permitted catches. Only later, when I myself had a chance to ponder the biological report, did it dawn upon me that the problem was related to numbering as an epistemological rather than a numerical tool. Numbers can be seen as relational in two distinct ways; they may reflect either a relation between *one and many*, or between *a whole and its parts* (Verran 2010: 173). To attribute numbers to a virtual stock is to simulate a *whole* out of which every narwhal is part and every catch therefore diminishes the whole. By contrast, to think of the *many* narwhals in the fjord is to open up for seeing one narwhal at a time. Both hunters and biologists resort to a kind of numerical reasoning to assess nature's potentiality, but it results in different measures.

This observation from North Greenland goes to show how counting and numbers may configure nature in unequal ways. Of course, numbers may still be a very useful way to reach some kind of agreement about reality, which cannot be obtained by means of stories, but they must be qualified as either indexical or iconic—to add another set of words to the one/many and the part/whole figurations of numbers. Furthermore, the case of narwhal counting in North Greenland shows how both the counted and the uncounted, or the figured and the unfigured, enter into the equation that results in an iconic number upon which to act. The general challenge is not only to explain how one gets from the count to the action, but also to explain how the unfigured, and even the *unfigurable*, may gain authority, as it does in statistical models. Let us see how:

> Statistics depends on the unfigured in the sense that its characteristic tables always include the uncounted; the figures imply, if they do not in fact record, not exact counts but *estimates*. The pretense of statistical representation to coverage—to record a totality—is always a pretense, in other words. No census-maker ever counts every individual in his or her district; no social scientist ever records every suicide; no schoolmaster ever knows about every boy who masturbates. The very idea of an aggregate implies generalization, but it also reflects or records generalization. (Poovey 1994: 420)

Statistics itself has evolved in the borderland between *being* science and *servicing* science, allegedly limiting itself to the recording of facts (Poovey 1994). This self-imposed limitation makes claim to a transparent relation to the objects represented, while masking the meanings that are thereby put into play. "Largely though not exclusively an effect of the categories by which statistical representation organizes materials, these meanings are being constructed before the statistics are compiled; they then radiate from the starkest tables. It is partly because such statistical representation—even if it is nowhere acknowledged—that theory and legislation *can* be generated from numbers" (ibid.: 420). Although this may not come as a surprise to category-conscious anthropologists, the implications are profound, because they touch upon the problem of evidence that anthropologists often have a hard time disentangling from description (Hastrup 2004b). Statistics not only depends on the *unfigured*, because its general estimates are inferred from more limited counts, as Poovey suggested and as the biologists practiced, it also importantly derives its significance from the *unconfigured*, i.e. that which lies outside the scope of the intended generalization—beyond the categories that count. This is where counting parts company from narration, and where we need to ask new questions also of the large-scale mathematically based models.

It is also where we may take the next step in our discussion of configuration by counting, now in the mode of simulation. Above, I identified a built-in humility in climate models owing to the fact that they could never be more than partial and temporary. Yet, they are also configurations in their own right, operating on the basis of calculations that are then fed into machines that may simulate all sorts of future scenarios. By ever-evolving technological means, it is possible to simulate the complex interactions of various elements and processes in the Earth system; they depict "time-dependent three-dimensional flows of mass, heat, and other fluid properties" (Lahsen 2005: 898). In that way the models can couple atmospheric, oceanic, and land-surface processes. Some may even include human impact in their simulations, although in most cases this relates primarily to large-scale carbon emission, rather than everyday practices, including land use. The models fill an important gap, and their power over the human mind is related to their technological sophistication:

> In recent decades, our understanding of the climate has been revolutionized by the development of sophisticated computer models, known as general circulation models (GCMs). GCMs are a representation of the physical laws . . . expressed in such form that they are suitable for solution on fast super-computers. (Williams 2005: 2932–33)

Note how "representation of the physical laws" and "suitable for solution" indicate an implicit acknowledgement of a selective editing process being part simulation. Even so, the models often take on a reality of their own; this has been documented ethnographically, researchers mistaking the representation for the real thing (Lahsen 2005). This has a lot to do with forceful colour markings, and with the sheer technical possibilities of showing the flows of mass and temperature. What meets the eye, when looking at the screen, is the ocean, the thermohaline circulation, the radiation, the albedo, and so forth; yet all of these fluid or invisible elements of climate take shape only on the screen and as modelled.

Along with the simulations, graphs have come to play an increasingly dominant role in the climate change debate and have become objectified to the point where the visual configurations of the numerical simulations are often presented "in the manner of exhibits at a trial, credited with the irrefutability of unmediated information" (Hamblyn 2009: 232). This simply goes to further emphasize how various modes of configuration may become credited with truth-values that are at best approximations. As Peter Ditlevsen has it, after having observed that the palaeo-climatic records show both remarkable climate stability on geological time scales and dramatic changes between different climate states:

> The current state-of-the art general circulation climate models do a fair job in integrating the flow equations for the atmosphere and oceans, admittedly at a coarse resolution. They also incorporate many physical and chemical interactions involving the cryosphere (ice masses), the lithosphere (the land masses) and the biosphere (vegetation) and give a realistic representation of the present climate. However, the models are far from being able to simulate the observed past climate transitions . . . The presence of a fat tailed noise component could imply that the

> triggering mechanism for climatic changes are rare extreme events. Such events, being on the time scale of seasons, are fundamentally unpredictable and never captured in numerical circulation models. The lack of dynamical range might be due to underestimation of internal variability in too coarse resolution, thus the climate noise is too weak to induce transitions from one stable climate state to one another. This could be part of the explanation why these models have yet never succeeded in simulating shifts between climatic states. (Ditlevsen 2009: 530)

Here, the modern physicist struggles to integrate the long-term pattern with the rare event, in much the same manner as enlightenment Englishmen and present-day hunters in Northwest Greenland. Although numerical models are apt at rendering regularities and virtualities, they are far less suitable for the actual singularities, whether manifested in rapid climate switches or in changing affordances for hunters hit by new uncertainties. This problem has sometimes led to the suggestion that because of the lack of full access in time and space to the phenomena of interest, the simulation models may look like fiction:

> There are certain similarities between a work of fiction and a model: Just as we may wonder how much the characters in a novel are drawn from real life and how much is artifice, we might ask the same of a model. How much is based on observation and measurement of accessible phenomena, how much is based on informed judgment, and how much is convenience? (Oreskes et al. 1994; quoted in Lahsen 2005: 901)

Although this observation provides us with a convenient bridgehead between the different modes of configuring climate change dealt with in this section, I would suggest that the most significant commonality between the configurative modes of narrating, counting, and simulating lies in their unified concern with the challenge of configuring the long-term regularity and the one-time event within one and the same image. Whether the latter are clad in the words of "canaries and whistleblowers" (Hamblyn 2009), "strange meteoric events" (Golinski 2007), or "tipping points" (Liverman 2009), they stand out on a background of presumed regularity, as does any *climate change* narrative, by implication. Whatever the configuration, it is of course always an approximation—in words, numbers, or graphs—of the elusive phenomenon of climate.

MODALITIES OF NATURE: ELUSIVE PLACES, FLUID OBJECTS, AND UNRESOLVED PROCESSES

The versatility of climate configurations reflects the nature of weather and climate itself; before we can proceed towards a comprehensive discussion of the anticipation of nature, we must therefore question *nature* itself—which quickly leads us to a point where it is the idea of nature *itself* that dissolves. Since the enlightenment, nature has been perceived as increasingly open to intervention and control, and nature has lost its foundational properties (Strathern 1992). In the course of this process, science and society have been shown to be indivisible (Haraway 1991; Keller 1992; Latour 1993). This does not imply the demise of science, only that both natural and social scientists must work with unstable entities, and hybrid categories. Looking towards our predecessors in climate research, we realize that they have always had to deal with a rather elusive natural fact, all while they sought to capture it in scientific terms.

We shall start by looking back to Alexander von Humboldt, who is regarded as one of the 19th-century pioneers in scientific climatology and whose definition of climate became crucial (Heymann 2010: 587). For Alexander von Humboldt climate meant "in the most general sense all changes in the atmosphere which noticeably affect the human organs", including temperature, humidity, barometric pressure, or wind (ibid.). This definition links climate to both location and human experience, and it presupposes a composite view of climate.

> Fundamental to Humboldt's conception of climatology was the role of space. Although Humboldt linked climate to individual locations, he considered at the same time spatial relations. His view of climate thus proved holistic in two different modes. Climate presented the whole of atmospheric phenomena at a defined location (synthesis of phenomena) and the whole of climates in different locations (synthesis in space) . . . In physical geography as in climatology Humboldt tabulated different sorts of data from many observation sites and then correlated the distribution of types of vegetation and climates, respectively. To facilitate these correlations, he developed the isoline technique of cartography. (Heymann 2010: 587–88)

One could argue that the "isolines" (e.g. isothermal, isobaric) were spatial forerunners of the computerized graphs we talked about above. Yet they can also be seen as intrinsically destabilizing the idea of climate as linked to specific location.

Before I develop this, I shall relate an early anthropological observation on the "acclimatization of man" that links the spatial identification of climate to the constitution of the human races (Hunt 1863). Although humankind as such may live all over the globe, Hunt questions the idea that the races are equally fit to do so and asserts that too little is known about the actual influence of climate upon individuals and races to portray humans as truly cosmopolitan beings.

> No one will attempt to deny that, physically, mentally, and morally, there does exist a very considerable difference between the denizens of

> different parts of the earth; and it is not proposed to inquire whether the various agents which constitute climate, and their collateral effects, are sufficient to produce the changes we find in physique, mind, and morals; but, simply taking the various types of man as they now occur on the earth, we have to determine whether we are justified in assuming that man is a cosmopolitan animal, and whether the power of acclimatization be possessed equally by all races of man known to us. (Hunt 1863: 51)

The emerging anthropology of the 19th century was deeply influenced by Johann Gottfried Herder, whose major work on the history of humankind had provided the earliest notion of culture as a collective term for groups of people (Herder 1784–91). Until then, culture had denoted individual accomplishment and polish. In the present connection, Herder's work is particularly interesting for its stress upon the ways in which cultures grow out of nature and remain intimately linked with the different continents, including their long-term natural histories and resources. Both Humboldt and Herder stressed the intertwinement of humans and nature, if from each their vantage point, and it was only in late 19th century that culture and nature were finally split institutionally. Hunt's work clearly operates on the edge between them, which is evident from his suggestion that the natural climate variability incorporates many variables, such as the landscape itself, its vegetation, available food-items, and so forth.

> In speaking, therefore, of climate, I use the word in its fullest sense, and include the whole cosmic phenomena. Thus, the physical qualities of a country have an important connection with the climate; and we must not simply consider the latitude and the longitude of a given locality, but its elevation and depression, its soil, its atmospheric influences, and also the quantity of light, the nature of its water, the predominance of certain winds, the electrical state of the air, etc., atmospheric pressure, vegetation and aliment, as all these are connected with the question of climate. (Hunt 1863: 52)

All these variables contribute to an overturning of climate as a feature of simple geographical location and it is in their various and variable combinations that they influence the individual races and differentiate their capacities for adaptation to the prevailing climates. By providing evidence "from that most valuable of all modern sciences, statistical science", mainly as applied to British military forces in India, Hunt goes on to substantiate his claim about different powers of acclimatization. This, again, provides the bottom line for evaluating the degree of cosmopolitanism inherent in the different races. While still implicitly adhering to the paradigms established by Humboldt and Herder, and while addressing the actual influence of climate upon humans, Hunt actually contributes to a destabilization of the spatial notion of climate on the one hand and to a disintegration of the human/nature unity on the other.

In the anthropology of climate another step in that direction was taken by Gladwin (1947) just before the new climate science took off. He concluded that "the cultural evidence has been more consistent and conclusive than the physical or physiological insofar as we are dealing with purely human adaptations to varying climate conditions" (Gladwin 1947: 609). By contrast to Hunt's speaking of acclimatization, Gladwin's notion of adaptation actually presupposes an objective distinction between culture and nature, even if tempered by his claim that it is also "evident that the wisdom of man and the wisdom of his body are constantly supplementing each other, although contributing in different proportions to the total adjustment under different conditions" (ibid.: 611).

The challenge of locating climates is related to the question of time scales, implicit also in the development from Humboldt's long-term perspectives to the later anthropological concern with the "ethnographic present" (Hastrup 1990). The question of time scale has been forcefully raised by Doreen Massey, who has shown how on a geological time scale, the present rock-formations in the Lake District in Great Britain must truly be seen as immigrant rocks; they derive from the Cambrian continental drift, thus defying any facile classification by their present location (Massey 2005: 130ff). Later geological ages have left other traces upon the landscape and on the whole, the natural landscape may not be the most obvious mental foundation of place, and hence of climate in the Humboldtian sense. Massey's work suggests that places are heterogeneous associations, configured in as many sciences, stories, and maps, which for all their authority are integrations of space *and* time. This, of course, does not imply that the one may be substituted for the other.

The heterogeneity of places transpires also from an analysis of the mapping exercises carried out by the Ordnance Survey in the Scottish Highlands (and elsewhere) in the 19th century, showing how in the process of eliciting traditional proper names, local authority structures and community relations became part of the map (Withers 2000). Thus even the most authorised of representations of place are subject to configurations that have relatively less to do with geography in the strict sense than with human history in the broad sense. This observation is now the backbone of "critical cartography" (Crampton & Krygier 2006). No less interesting, geography has now been depicted in the plural as "hybrid geographies" highlighting that the thinking of space cannot be detached from thinking through the body (Whatmore 2002).

Just like weather and climate are configured in particular ways, so (now) are also places. Places may be named and marked by completely different time scales, but they are always the result of emerging stories, accidental encounters, and movements along lines of promise. As Massey elaborates: "What is special about place is precisely that throwntogetherness, the unavoidable challenge of negotiating the here-and-now (itself drawing on a history and a geography of thens and theres); and a negotiation that must take place between both human and non-human" (ibid.: 140). Again we sense an echo from the intertwined notion of climate discussed above. Place itself is an event of configuration. It is "the coming together of the previously unrelated, a constellation of processes rather than a thing. This is place as open and multiple. Not capturable as a slice through time in the sense of an essential section. Not intrinsically coherent" (ibid.: 141). Places are thus intrinsically elusive, and this is one of the challenges that may gain from an interdisciplinary investigation into global climate in its many variants and locations, and on multiple time scales.

From the elusive places we shall move on to the fluid objects, which constitute the elements of climate and which can also not be disentangled from our experiencing of it, and which cannot, therefore, be seen as pure nature. If climate adheres to elusive places, the weather is no more substantial, and never outside of human life, as suggested by Tim Ingold's notion of the weatherworld (Ingold 2007, 2010). The weather is the medium within which humans live and breathe, rather than something we observe or simply experience; we are forced to "recognize that for persons, or things to interact at all they must be immersed in the flows, forces and pressure gradients of the surrounding media. Cut out from such currents, they would be *dead*" (Ingold 2010: 132). Ingold's notion of flow reminds us that the nature in which humans are immersed comprises the elements of air, wind, and water as well as earth; the general point is that all of these media are not so much perceived, as they are perceived *through*.

Even the ground upon which people move is never simply seen, but perceived kinaesthetically and thus internalized (Ingold 2010: 125). When we say that the ground rises up or that the surface of the ice is rough, it is related to our experience from traversing it; it is our own bodily sensation that determines the phrasing—and enters our muscular consciousness—to borrow a wonderful term from Gaston Bachelard (1964: 11; Hastrup in press). In itself, the ground actually *consists* of surfaces that are very far from the evenness that governs our *idea* of the surface of the Earth. Much to my initial surprise when I first went dog sledging some years back, this also appertains to the sea-ice. It is far from even all of the time; it may have refrozen after a storm that broke up the first new ice, and the drift ice from glaciers, etc. may have occasioned other irregularities. It may look smooth from above; but from within the experience of moving about it is rarely so, and even the passenger must look out, lest the feet dangling over the side should hit an ice floe. The sensation of irregularity along the path settles as a muscular consciousness, and after a while one just knows when to flex the muscles or move the limbs.

In her study of rock climbing, Penelope Rossiter suggests an even closer connection between the surface traversed and the person traversing it. In the course of negotiating the rock a peculiar entanglement of the rock, the technologies of climbing, and the person takes place. Through their intermingling, "cliffs become climbs, and humans become climbers" (Rossiter 2007: 293). This mutual definition of the rock and the person rests within a larger idea of being immersed in an animated world: "The micromoments of a climb, and in the pre- and post-climbing ruminations and gazing on cliffs, there is sense of a dialogue with an animate entity" (Rossiter 2007: 294). We are forced to consider the mutual agency of the surface and the person, flowing together as it were. The climber no more defines the climb than the rock.

These observations highlight the sense of dialogue and mutual definition of humans and their environment, including its diverse elements of wind and weather. Such elements may play significant parts in the definition of social spaces. Thus, to take one small example, in the Andaman Islands the invisible force of the winds define both the seasons and the concomitant changes in social and bodily practices (Pandya 2007). Society and seasonality are very much a function of the flows of time as embodied in the shifting winds. In the North Greenland district where I work, the sub-districts are named by the predominant winds, which again are proxies for assembled features of the landscape, the sea, and the resources (Gilberg 1986).

Another stark example of the socialization of the winds is found in the development of wind technologies in the 20th century. Although, evidently, it is a matter of capturing winds for the benefit of society, this is not the only way in which we may speak of wind-power as socialized. As Matthias Heymann has shown, the major distinction between wind technologies developed in Denmark, Germany, and the United States is owed to "different technological styles, relating not only to the form and characteristics of the technological artefact but also to local processes and conditions" (Heymann 1998: 666). One of the major distinctions relates to the professional backgrounds of the actors: "Reliable and successful wind turbine designs have mostly been developed by non-academic engineers, technicians in Denmark, while the designs proposed by academic engineers in the 1970s and 1980s mostly failed" (ibid.). The wind turbines originally designed and tested by individual craftsmen, rather than by well-funded engineers responding to research and development plans, have something in common with the Zimbabwe bush pump, analyzed by de Laet and Mol (2000). The quality and sustainability of this particular village water pump rest with its fluidity and its entanglement in a variety of worlds. The pump *works*, because the fluidity is built into the technology itself. Both the turbines and the pumps are of course solid pieces of technology, but they matter within a comprehensive set of practical, social, and political relations.

I would venture that technologies of weather and wind, whether designed to harness, to measure, or to mitigate their potential, are fluid objects in the sense described here. They will not work if too rigid, because the weather-worlds in which we live are not rock-solid, but the opposite. The fluidity of the objects is remarkable also when we consider the computer technologies, now capturing the fluidity and complexity of the climate system. The way of dealing with this complexity is to feed its elements into the machines and study the convergence (or divergence) of isolated phenomena, such as land surface temperatures, sea rise level, and ice cap reduction. For the complexity of the climate system to be manageable, climatologists must therefore begin by slicing it up.

> It is difficult to think of a more complicated physical system than Earth's climate. Governed by a combination of the laws of fluid dynamics, thermodynamics, radiative energy transfer and chemistry, the climate system is composed of the atmosphere, the oceans, ice sheets and land. Each of these four subsystems is coupled to each of the other three, through the exchange of immense quantities of energy, momentum and matter . . . Nonlinear interactions occur on a dizzying range of spatial and temporal scales, both within and between the subsystems, leading to an intricate and delicate network of feedback loops. (Williams 2005: 2931)

We note how the larger climate system is here sub-divided into four subsystems, to allow for measurements of exchange. Any model that is made to represent the complexity would have to break it down into components at one level or other; we are faced with a case of what Edwin Ardener called the collapse of measurement into definition (1989: 149). This collapse is further underscored when climate modellers go native to the simulation, and conflate it with scientific observation (Lahsen 2005: 909–11). Realism is at stake, and the uncertainties of measurement are buried within the (alleged) certainties of the components classified as such.

A major uncertainty in the modelling practice stems from the limited amount of data available that fit the computerized thinking. As we saw above, the models are representations of physical laws "expressed in such form that they are suitable for solution" (Williams 2005: 2933). This gives rise to yet another source of uncertainty about the truth-value of the projections, stemming from the blurred boundaries between models and observational data (Edwards 1999). The simulations smooth out the variation of data, and thus co-produce circularity between model production and validation. For the fluid nature to deliver evidence, it must be classified in manageable elements, yet the fluidity persists and destabilizes evidence.

This is pertinent in the study of the so-called "unresolved processes", related to the inherent complication in the fact that climate models deal with "the coexistence of climatological phenomena of a vast range of scales" (Williams 2005: 2931). In Williams' thinking, scale is mainly a matter of quantification and measure, and it spans from a planetary scale to a question of kilometres. Even so, something falls out of view; the scale of data needed to feed the machines apparently leaves out important features of the climate system as envisaged in real time. The grid system used to produce comparable data makes significant small-scale processes and mechanisms disappear, such as for instance gravity waves, convective clouds, and small-scale turbulence.

> All of these features are known to be key aspects of the climate system, owing to their non-linear interactions with the resolved scales, and yet they are too small to be explicitly modelled. The presence of such critical unresolved processes must surely be one of the most disheartening aspects of climate modelling. (Williams 2005: 2933)

Several ways of dealing with these "unresolved processes" in the modelling practice have been suggested. One suggestion is particularly interesting. It has actually been recommended that random noise be added to the models in an attempt to mimic the impacts of the unresolved processes. The stochastic or non-linear phenomena in climate change remain the main challenge in climatology, and adding random noise is known to have improved the performance of the model. In the process, of course, climate itself has become further de-objectified, and the simulation more detached from the measurable. With the climatologist I would like to note "that it is truly remarkable that random noise—the very epitome of the unknown and the unpredictable—can actually increase the performance of models" (Williams 2005: 2933–34).

This method of adding random noise is apparently in use already in relation to the small-scale weather forecasts (simulations) that we all know from television, and this has allegedly improved their accuracy. There are some sound theoretical arguments for this, but it is still intriguing that unresolved processes may be mimicked arbitrarily and still make the projections more accurate. Because of this and because models are produced, used, and recycled in multiple ways according to a variety of interests, measurement and representation are not the sole loci of uncertainty. There is another one, which relates to the deeper uncertainty of understanding, and to the wider use and transfer of knowledge.

As recently pointed out by Reiner Grundmann, in the received view scientific uncertainty has a negative impact on its regulatory effects (Grundmann 2006: 75). This appears not to be the case, and part of the explanation lies with the false assumption of a linear knowledge transfer from science to policy. This process must be seen within a social and political context, within which "scientific uncertainty becomes a rhetorical resource which can and will be employed by different actors in different ways" (Edwards 1999: 465–66). Uncertainty and even major disagreement in the scientific community may actually lead to the same political conclusion, if for different reasons, as pointed out by Grundmann, citing how in the United States the otherwise incongruent Republican administration and Democratic Congress agreed on the need for more climate research, "one side looking for reasons to do nothing, the other seeking justification for action" (Grundmann 2006: 80).

We can see how the climate models, including their inherent uncertainties, take on a life of their own, once they have been unleashed into society and politics. There is a pervasive element of knowledge politics in all climate change models (Grundmann 2007). This not only reveals the limits of the linear model of knowledge transfer, it also shows how people make a deliberate use of the uncertainties for their own strategic purposes. This adds another set of reasons for talking about the climate models as fluid objects—mirroring the non-deterministic nature of the climate system. And this is of course the main point: there is no one nature of climate change, but through modelling exercises we may approximate some of its modalities and thus extend the knowledge by which we may anticipate nature's future course.

PRACTICES OF ANTICIPATION: REASONING ABOUT CLIMATE CHANGE

Anticipation is part and parcel of human agency; without some sense of the future, it is impossible to act responsibly. Also, without a larger moral horizon, there can be no sense of self (Taylor 1989). Of course, all of us have met apparently erratic human behaviour, but once we speak of human agency in general we presuppose a social, moral, and temporal horizon beyond both the individual and the moment. The notion of anticipation is thus a comprehensive term, within which people may attempt at predicting, projecting, or forecasting both immediate and more distant futures.

In anticipating changing climates, people are faced with uncertainties of an unprecedented magnitude; one reason for its being unprecedented is the high impact of international climate science in public debate. In science, this has led also to a new set of models in the wake of the climate models proper, namely the IAMs—or the integrated assessment models—linking climate change policies and costs to particular mitigation targets (Neufeldt et al. 2010). Here, anticipation is harnessed in instruments of projection, incorporating adaptive and economic measures and striving towards optimal adaptation. The delight in such political instruments should be tempered, however.

> Given the current impossibility to construct plausible scenarios that consider all aspects that determine the costs and benefits of an adaptation option, the normative assessment of "optimal" adaptation strategies may well be a step too far. Instead, a positive analysis of the full range of possible and appropriate adaptation options—with their costs, benefits and other implementation considerations—could be at least informative to decision-makers. (Klein 2003: 11)

In view of what we are discussing in this volume, the mechanistic view of adaptation and policy-making falls short of a proper acknowledgement of the conceptual and imaginative surplus inherent in human reasoning. The capacity for anticipation has a lot in common with the capacity for conceptualization, which "outruns the concepts it produces" (Strathern 2004: xv). Thus, once a new model is in place, the human mind is already moving beyond it, addressing new fears arising at its boundaries and its built-in uncertainties about that which remains unconfigured in the model. Also, we should not forget that the climate models (i.e. the general circulation models) that are taken as the most vital sources of information for policy-makers are not simple representations; 'such models do not merely represent nature, they are also ways of creating "other" simulated natures at the same time as "naturalizing the social world"' (Rayner 2003: 282).

Climate change resists domestication and reminds us that "fear is at its most fearsome when it is diffuse, scattered, unclear, unattached, unanchored, free floating, with no clear address or cause; when it haunts us with no visible rhyme or reason, when the menace we should be afraid of can be glimpsed everywhere but is nowhere to be seen" (Bauman 2006: 2). The multiple ways of configuring and modelling the nature of current climate concerns are means by which the fears are transformed into manageable risks; but they are operative only so far as there is agreement about the social and moral horizon by which people orient themselves in the world. And as all scientific understanding, climate knowledge is stabilized through a non-obvious process of circulating reference (Latour 1999), which cannot but reproduce the fluidity of the object itself. This further underscores the fact that "climate" is deeply marked by humanity, whichever way we look at it (Hulme 2010).

In everyday life, anticipation implies a day-to-day forecasting of practical possibilities on the one hand, and a concern with more distant futures and possible scenarios on the other. With the Big Story of climate change now being known to most people, the daily weather forecast and the extreme weather events are read as indications of unknown scenarios of a nature out of bounds. This complicates anticipation, which by definition always requires new knowledge, because the concepts are always out-manoeuvred by new climate events, stirring the capacity for further conceptualization. An example of such conceptual work is found in a study of fishermen in Tamil Nadu, who were severely hit by the tsunami in December 2004. Frida Hastrup has shown how the initial "sense of having lost the ability to predict natural conditions in the days and weeks following the tsunami was gradually countered by a subtle casting of unpredictability as the expected and future order of things within specific and limited periods of time. The point is that this was a recovery of the ability for forecast and not a discarding of it" (Hastrup 2011: 77).

In the present context this goes to stress that conceptual work is a significant part of anticipatory practices that are so much more than "mere" predictions. The conceptual work reflects various styles of human reasoning, as we shall see in the chapters below, demonstrating how climate is negotiated in social practice, ranging from everyday strategies of harvesting nature's resources to highly technical modalities of long-term prediction. If in the large-scale climate models the (unresolved) processes of intermediate range tended to disappear from view, this is in stark contrast to reasoning about climate modelling in the everyday, where people are actively seeking to anticipate future scenarios—including middle-range phenomena that may bridge weather and climate. This is where the present volume, through its interdisciplinary effort at addressing a problem that lies athwart the individual disciplines, offers a genuine contribution to knowledge. In each their way the chapters below address the key question of the volume, viz. how people within particular settings anticipate future environmental scenarios and transform them into shared images and expectations that make social action possible.

In *Chapter 2*, Mike Hulme raises the important question of how climate models gain and exercise authority and become endowed with the status of bearing witness to the climate change. The authority is both epistemic and social, and through an investigation of the interaction between scientific models, cultural performances, and political interests, the varying relations between the two forms of authority are unfolded. This leads to a deep-seated recognition of their entanglement and to a call for incorporating the social and human sciences when addressing climate change.

In *Chapter 3*, Frida Hastrup explores the figuration of knowledge among environmental experts in coastal Tamil Nadu, India, and shows how this figuration paradoxically contains its own admission of the unknowable. Through a detailed attention to the use of percentages in the assessment of environmental knowledge she shows how the unknowable may still be counted and thus become figured. This way, knowledge remains authoritative, and no one seems to question the figures as comprehensive, even when acknowledged as partial.

In *Chapter 4*, Cecilie Rubow deals with the multiplicity of responses to new climate uncertainties in the Cook Islands, the South Pacific, where the recurrent cyclones are read as signs of climate change. On this background she explores the practical life of climate models and the multiplicity of anticipatory responses that results. Through her discussion it becomes clear that there is no single kind of knowledge that may stabilize the visions of the future, but rather a whirling of concepts and understandings that mirror the turbulent times and serve each their purpose in different contexts.

In *Chapter 5*, Kirsten Hastrup analyzes the process by which hunters in Greenland assess the quality and potentiality of the sea-ice, upon which their livelihood depends. With the melting ice, the future seems increasingly uncertain in the North, yet the hunters still have to procure food for their families and need to navigate the ice whenever possible. It is shown how they manage by way of a diagrammatic reasoning, which is seen as a particular kind of modelling that establishes qualitative relations between disparate observations and indicate a way forward.

In *Chapter 6,* Hildegard Diemberger explores the interface between local and scientific knowledge in Tibet, where the snow-clad mountains have always been indicators of the moral climate. She shows how the local observations in many ways complement the scientific models, and makes a strong case for collaboration to strengthen the knowledge base for deciding about the future in a troubled region. It is not simply that local knowledge may be called upon as empirical evidence for the changes that climate models may simulate, it is also and very importantly a rich and underexplored source of proxy data for modellers.

In *Chapter 7*, Ásdís Jónsdóttir shows how notions of "local" and "global" are configured within a coastal adaptation project. She discusses how the different scaling practices used in various contexts influence the space for agency. Although the project and the joint workshops it generated were designed to minimize uncertainties, they actually generated a whole new set of them. Also, in the process of translating between allegedly local and global forms of knowledge, certain forms of agency became marginalized. Policies thus deeply influenced the sense of the possible.

In *Chapter 8*, Anders Kristian Munk analyzes two field experiences with hydraulic modelling practices and asks the question: what happens when we flood the future? In a straightforward sense, of course, flood modellers are concerned with anticipating nature—they try to predict its likely course under given circumstances. In a less obvious way this also implies that flood modelling works on a perception of nature as a bounded domain of its own, even though it has become abundantly clear that the determination of flood risk is bound up with social and financial forces.

In *Chapter 9*, Martin Skrydstrup presents an ethnographic description from the ice core drilling community on the Greenlandic ice cap of how agreement about nature is reached by way of a mixture of negotiation, calculation, and experimentation. It is shown in detail how expectations solidify as certainties in the micro-sociality of the science trench, where the signals from the bottom of the ice cap are read and interpreted. The general point is that the reading of the ice and the gleaning of its hidden truths gradually open nature up for the scientists. The act of anticipation is skilfully plotted by the leader, who manages to build up to its conclusion, to be reached at the absolutely right moment.

In *Chapter 10*, Peter Ditlevsen discusses some of the implicit uncertainties of general circulation models and alerts us to different strategies of prediction. It is argued that the success of numerical weather predictions has induced the belief that climate and the working of nature can be anticipated and is accessible through calculations. The development of climate models over the past three decades has been toward including more and more processes and components of Nature. This has been the standard solution to correcting for insufficiencies or inaccuracies in the model simulations when comparing with observations. The question is if this process ever stops, or if, at any point, the models are accurate or detailed enough.

In *Chapter 11*, Matthias Heymann investigates the emergence and construction of confidence and trust in early climate simulation from the mid-1950s until about 1980. The investigation focuses on an analysis of the writings of distinguished climate scientists like William Kellogg, Stephen Schneider, and James Hansen. From the presentation, formulation, and justification of findings based on climate simulation typical sources of confidence in climate models can be inferred.

Finally, in *Chapter 12*, which is a brief afterword, Martin Skrydstrup links the chapters together with a view to the interdisciplinary transaction in which they are rooted and to the natures that emerge in the process.

As will have transpired from this brief presentation of the individual chapters, they not only illustrate the *problem* inherent in the anticipation of nature, they also show how people across the globe and across a wide spectrum of modelling and reasoning practices seek to diminish the uncertainty about the future. This is what still makes it possible to act; nature must be anticipated at some scale or other for people to be able to take responsibility for society—so deeply implicated in the forces of nature.

In this introductory essay I have wanted to open up a space where the uncertainties adhering to the future climate may be put to productive use in knowledge-making. It seems that along with climate change comes a whole new range of conceptual challenges that call for a new interdisciplinary effort. The diversity of chapters and the multiplicity of perspectives offered bear witness to a scholarly diversity as *both* foundational to our enterprise *and* as innovative exploration. This exploration is of vital importance not just in the world of scholarship, but for the whole of the world we may anticipate in the Anthropocene.

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REFERENCES

Alley, R.B. 2003. "Palaeoclimatic Insights into Future Climate Challenges." Philosophical Transactions: Mathematical, Physical and Engineering Sciences 361 (1810), Abrupt Climate Change: Evidence, Mechanisms and Implications, 1831–49.

Anderson, D.G., K.A. Maasch & D.H. Sandweiss, eds. 2007. Climate Change and Cultural Dynamics: A Global Perspective on Mid-Holocene Transitions. Amsterdam: Elsevier/Academic Press.

Ardener, E. 1989. The Voice of Prophecy and Other Essays. Oxford: Blackwell.

Asvid-Rosing, A. 2002. The Polar Bear Hunt in Greenland. Nuuk: Greenland Institute of Natural Resources, Technical Report 45.
Bachelard, G. 1964. The Poetics of Space. Translated by M. Jolas. Boston: Beacon.
Bauman, Z. 2006. Liquid Fear. Cambridge: Polity Press.
Bauman, Z. 2007. Liquid Times. Cambridge: Polity Press.
Beck, U. 1992. Risk Society: Towards a New Modernity. London: Sage.
Born, E.W. 2005. An Assessment of the Effects of Hunting and Climate on Walruses in Greenland. Oslo: Natural History Museum, University of Olso.
Born, E.W., A. Heilmann, L. Kielsen Holm & K. Laidre 2011. Polar Bears in Northwest Greenland: An Interview Survey about the Catch and the Climate. Copenhagen: Museum Tusculanum Press (MoG).
Borroughs, W.J. 2005. Climate Change in Pre-history. Cambridge: Cambridge University Press.
Crampton, J.W. & J. Krygier 2006. "An Introduction to Critical Cartography." ACME: An International E-Journal for Critical Geographies 4 (1), 11–33.
Crate, S. & M. Nuttall, eds. (2009) Anthropology and Climate Change: From Encounters to Actions. Walnut Creek, CA: Left Coast Press.
Cruikshank, J. 2005. Do Glaciers Listen? Local Knowledge, Colonial Encounters, and Social Imagination. Vancouver: University of British Columbia Press.
Daniels, S. & G.H. Endfield 2009. "Narratives of Climate Change: Introduction." Journal of Historical Geography 35, 215–22.
Ditlevsen, P.D. 2009. "Climate Transitions on Long Timescales." Contemporary Physics 50 (4), 511–32.
Douglas, M. 1985. Risk Acceptability According to the Social Sciences. London: Routledge.
Douglas, M. & A. Wildavsky 1982. Risk and Culture: An Essay on the Selection of Technological and Environmental Dangers. Berkeley: University of California Press.
Edwards, P.N. 1999. "Data-Laden Models, Model-Filtered Data: Uncertainty and Politics in Global Climate Change." Science as Culture 8, 437–72.
Ehkers, E. & T. Kraft, eds. 2006. Earth Systems Science in the Anthropocene. New York: Springer Press.
Giddens, A. 2009. The Politics of Climate Change. Cambridge: Polity Press.
Gilberg, R. 1986. "Polar Eskimo", in W. Sturtevant, ed. Handbook of North American Indians 5, Washington: The Smithsonian Institution (p.577–94).
Gladwin, T. 1947. "Climate and Anthropology." American Anthropologist 49, 601–11.
Golinski, J. 2003. "Time, Talk, and the Weather in Eighteenth-Century Britain", in S. Strauss & B.S. Orlove, eds. Weather, Climate, Culture. Oxford: Berg (p.17–38).
Golinski, J. 2007. British Weather and the Climate Enlightenment. Chicago: Chicago University Press.
Grundmann, R. 2006. "Ozone and Climate: Scientific Consensus and Leadership." Science, Technology & Human Values 31, 73–101.
Grundmann, R. 2007. "Climate Change and Knowledge Politics." Environmental Politics 16, 414–32.
Hamblyn, R. 2009. "The Whistleblower and the Canary: Rhetorical Constructions of Climate Change." Journal of Historical Geography 35, 223–36.
Haraway, D. 1991. Simians, Cyborgs and Women: The Reinvention of Nature. New York: Routledge.
Hastrup, F. 2011. Weathering the World: Recovery in the Wake of the Tsunami in a Tamil Fishing Village. New York: Berghahn Books.
Hastrup, K. 1990. "The Ethnographic Present: A Reinvention." Cultural Anthropology 5 (1), 45–61.

Hastrup, K. 2004a. Action: Anthropology in the Company of Shakespeare. Copenhagen: Museum Tusculanum Press.
Hastrup, K. 2004b. "Getting it Right: Knowledge and Evidence in Anthropology." Anthropological Theory 4, 455–72.
Hastrup, K. 2007. "Performing the World: Agency, Anticipation and Creativity", in E. Hallam & T. Ingold, eds. Creativity and Cultural Improvisation. Oxford: Berg, ASA Monographs 44 (p.193–206).
Hastrup, K. 2009a. "The Nomadic Landscape: People in a Changing Arctic Environment." Danish Journal of Geography 109 (2), 181–90.
Hastrup, K. 2009b. "Arctic Hunters: Climate Variability and Social Mobility", in K. Hastrup, ed. The Question of Resilience: Social Responses to Climate Change. Copenhagen: The Royal Danish Academy of Sciences and Letters (p.245–70).
Hastrup, K. 2010. "Emotional Topographies: The Sense of Place in the Far North", in J. Davies & D. Spencer, eds. Emotions in the Field: The Psychology and Anthropology of Fieldwork Experience. Stanford: Stanford University Press (p.191–211).
Heide-Jørgensen, M.P., N. Hammeken & P. Hollebeek 2002. Optælling af narhvaler i Qaanaaq Kommune august 2001. Nuuk: Grønlands Naturinstitut. Teknisk Rapport nr. 50.
Herder, J.G. 1784–91. Ideeen zur Philosophie der Geschichte der Menschheit, Repr. Leipzig 1877–1913.
Heymann, M. 1998. "Signs of Hubris: The Shaping of Wind Technology Styles in Germany, Denmark, and the United States, 1940–1990." Technology and Culture 39, 641–70.
Heymann, M. 2010. "The Evolution of Climate Ideas and Knowledge." WIREs Climate Change 2010 1, 581–97.
Hulme, M. 2009. Why We Disagree about Climate Change: Understanding Controversy, Inaction and Opportunity. Cambridge: Cambridge University Press.
Hulme, M. 2010. "Cosmopolitan Climates: Hybridity, Foresight and Meaning." Theory, Culture and Society 27, 267–76.
Hunt, J. 1863. "On Ethno-climatology: Or the Acclimatization of Man." Transactions of the Ethnological Society of London 2, 50–83.
Ingold, T. 2007. "Earth, Sky, Wind, and Weather", in Wind, Life, Health: Anthropological and Historical Perspectives, special issue, Journal of the Royal Anthropological Institute, 19–38.
Ingold, T. 2010. "Footprints through the Weatherworld: Walking, Breathing, Knowing", in The Objects of Evidence: Anthropological Approaches to the Production of Knowledge, special issue, Journal of the Royal Anthropological Institute, 121–139.
IPCC 2007. Climate Change 2007, Fourth Assessment Report, Working Group I: The Physical Science Basis. Cambridge: Cambridge University Press.
Keller, E.F. 1992. Secrets of Life, Secrets of Death: Essays on Language, Gender and Science. London: Routledge.
Klein, R.J.T. 2003. "Adaptation to Climate Variability and Change: What Is Optimal and Appropriate?", in C. Giuoponi & M. Schecter, eds. Climate Change in the Mediterranean: Socio-economic Perspectives of Impacts, Vulnerability and Adaptation. Cheltenham: Edward Elgar (p.32–59).
Laet, M. de & A. Mol 2000. "The Zimbabwe Bush Pump: Mechanics of a Fluid Technology." Social Studies of Science 30, 225–63.
Lahsen, M. 2005. "Seductive Simulations? Uncertainty Distribution around Climate Models." Social Studies of Science 35, 895–922.
Latour, B. 1993. We Have Never Been Modern. Translated by C. Porter. Cambridge, MA: Harvard University Press.

Latour, B. 1999. "Circulating Reference: Sampling the Soil in the Amazon Forest", in Pandora's Hope: Essays on the Reality of Science Studies. Cambridge, MA: Harvard University Press (p.24–79).

Latour, B. 2005. Reassembling the Social. Oxford: Oxford University Press.

Liverman, D.M. 2009. "Conventions of Climate Change: Constructions of Danger and the Dispossession of the Atmosphere." Journal of Historical Geography 35, 279–96.

Lobell, D. & M. Burke, eds. 2010. Climate Change and Food Security: Adapting Agriculture to a Warmer World. New York: Springer.

Massey, D. 2005. For Space. London: Sage.

Neufeldt, H. et al 2010. "Climate Policy and Inter-Linkages between Adaptation and Mitigation", in M. Hulme and H. Neufeldt, eds. Making Climate Change Work for Us: European Perspectives on Adaptation and Mitigation Strategies, Cambridge: Cambridge University Press.

Oreskes, N., K. Shrader-Frechette & K. Belitz 1994. "Verification, Validation, and Confirmation of Numerical Models in the Earth Sciences." Science 263 (4), 641–46.

Orlove, B., E. Wiegandt & B.H. Luckman, eds. 2008. Darkening Peaks: Glacier Retreat, Science and Society. Berkeley: University of California Press.

Pandya, V. 2007. "Time to Move: Winds and the Political Economy of Space in Andamese Culture." Journal of the Royal Anthropological Institute special issue, S91–S104.

Poovey, M. 1994. "Figures of Arithmetic, Figures of Speech: The Discourse of Statistics", in J. Chandler, A.I. Davidson, & H. Harootunian, eds. Questions of Evidence: Proof, Practice and Persuasion across the Disciplines. Chicago: Chicago University Press.

Rayner, S. 2003. "Domesticating Nature: Commentary on the Anthropological Study of Weather and Climate Discourse", in S. Strauss & B.S. Orlove, eds. Weather, Climate, Culture. Oxford: Berg (p.277–90).

Richardson, K., W. Steffen & D. Liverman 2011. Climate Change: Global Risks, Challenges and Decisions. Cambridge: Cambridge University Press.

Ricoeur, P. 1984. Time and Narrative. Translated by K. MacLaughlin & D. Pelleauer. Chicago: University of Chicago Press.

Rossiter, P. 2007. "On Humans, Nature, and Other Nonhumans." Space and Culture 10 (2), 292–305.

Sanders, T. 2003. "(En)Gendering the Weather: Rainmaking and Reproduction in Tanzania", in S. Strauss & B.S. Orlove, eds. Weather, Climate, Culture. Oxford: Berg (p.83–102).

Sörlin, S. 2009. "Narratives and Counter-Narratives of Climate Change: North Atlantic Glaciology and Meteorology." Journal of Historical Geography 35, 237–25.

Sörlin, S. & P. Warde, eds. 2009. Nature's End: History and the Environment. New York: Palgrave/Macmillan.

Stern, N. 2006. The Economics of Climate Change. Cambridge: Cambridge University Press.

Strathern, M. 1992. After Nature: English Kinship in the Late Twentieth Century. Cambridge: Cambridge University Press.

Strathern, M. 2004. Partial Connections. Revised edition. Walnut Creek, CA: Altamira Press.

Strathern, M. 2005. "Anthropology and Interdiscplinarity." Arts and Humanities in Higher Education 4, 125–35.

Strathern, M. 2006. "A Community of Critics? Thoughts on New Knowledge." Journal of the Royal Anthropological Institute 12, 191–209.

Strauss, S. & B.S. Orlove, eds. 2003. Weather, Climate, Culture. Oxford: Berg.

Strauss, S. 2003. "Weather Wise: Speaking Folklore to Science in Leukerbad", in S. Strauss & B.S. Orlove, eds. Weather, Climate, Culture. Oxford: Berg (p.39–60).
Taylor, C. 1989. Sources of the Self: The Making of the Modern Identity. Cambridge: Cambridge University Press.
Taylor, C. 2004. Modern Social Imaginaries. Durham: Duke University Press.
Vanderheiden, S., ed. 2008. Political Theory and Climate Change. Cambridge, MA: MIT Press.
Verran, H. 2010. "Number as an Inventive Frontier in Knowing and Working Australia's Water Resources." Anthropological Theory 10, 171–78.
Whatmore, S. 2002. Hybrid Geographies: Natures Cultures Spaces. London: Sage.
Williams, P.D. 2005. "Modelling Climate Change: The Role of Unresolved Processes." Philosophical Transactions: Mathematical, Physical and Engineering Sciences 363 (1837), 2931–46.
Withers, C.W.J. 2000. "Authorizing Landscape: 'Authority', Naming and the Ordnance Survey's Mapping of the Scottish Highlands in the Nineteenth Century." Journal of Historical Geography 26, 532–54.

2 How Climate Models Gain and Exercise Authority

Mike Hulme

INTRODUCTION

Numerical climate models have become central to the unfolding story of climate change. Climate models underpin the knowledge claims and risk assessments of the Intergovernmental Panel on Climate Change (IPCC), claims and assessments which powerfully shape political narratives of climate change (Manuel-Navarette 2010) and animate new social movements (Jamison 2010). Climate models seem essential for the detection and attribution of anthropogenic climate change, heavily informing iconic expert judgements such as: "Most of the observed increase in global average temperatures since the mid-20th century is *very likely* due to the observed increase in anthropogenic greenhouse has concentrations" (IPCC 2007a: 10; emphasis in the original). Climate models are also being deployed to attribute extreme weather events, such as individual heat waves or flooding episodes, to human influences (Pall et al. 2011). And numerical climate models offer novel access to the distant future by simulating the climatic consequences and their impacts of different development pathways being chosen around the world: "Anthropogenic warming could lead to some impacts that are abrupt and irreversible" (IPCC 2007b: 53). By anticipating the future in this way, climate models have become a prosthetic to human moral and ethical deliberation about long-term decision-making.

Numerical climate models[1] have therefore acquired significant authority in the contemporary world—if by authority we mean "the power to determine, adjudicate, or otherwise settle issues or disputes".[2] They exercise this power and influence over the academy, over policy debates, and over the human imagination as the following quotations show (emphases added):

> (from scientists) "*Climate models* will . . . play a . . . perhaps central role in guiding the trillion dollar decisions that the peoples, governments, and industries of the world will be making to cope with the consequences of changing climate . . . adaptation strategies require more accurate and reliable predictions of regional weather and climate extreme events than are possible with the current generation of climate

models" (World Modelling Summit for Climate Prediction 2008; quoted in Goddard et al. 2009: 343).

(from campaigning organisations) "But, with the advancement of global *climate models* to three-dimensional coupled entities, with ever increasing spatial resolutions, it is now known that the impacts of climate change will manifest in more extreme local changes in temperature" (nef 2008: 3).

(from religious organisations) "The aims of the Church of England's Shrinking the Footprint campaign rely on the accumulated weight of evidence from scientific observation and modelling. The campaign will continue to maintain awareness of . . . projections from *climate models* of the climate system" (Church of England 2009: 8).

(from public intellectuals) "The relentless logic of the *[climate] models* proves over and over that the poor and vulnerable will be hardest hit by climate change" (Hamilton 2010: 201).

How can it be that climate models are able to exert authority over trillion-dollar decisions, over religious organisations, and over the human imagination of the future? What sort of authority is it that is being exercised? How do climate models gain this authority, and how do they retain it? And in what ways is this authority differently recognized between cultures?

The UK's Royal Society's motto famously asserts "*nullius in verba*"—"on the word of no-one"; i.e. accept nothing on authority. The corollary of such scepticism is carefully to observe, test, and experiment. This challenge to received wisdom was characteristic of the cultural shifts in Europe of the late 17th- and early 18th-century Enlightenment which gave birth to the Royal Society. Yet it is a scepticism that human beings find difficult always to practice. Deference to the elder, the priest, the celebrity, or deference to the claims of science itself is difficult to eradicate. We want to be reassured about the future, to establish some authority which can tame and manage our fears about it.

Although of necessity we accept many things on authority each day, in the case of climate models is this deference warranted? The IPCC Fourth Assessment Report claimed in 2007: "There is considerable confidence that [climate models] provide credible quantitative estimates of future climate change, particularly at continental and larger scales" (IPCC 2007a: 591). Is there considerable confidence? For whom and for where? And for what purposes is such confidence claimed?

The question, therefore, I wish to address in this chapter is: "How do climate models gain and exercise authority?". There are two interrelated dimensions to the authority of climate models which need examination: the source of climate models' epistemic authority and the source of their social authority. *Epistemic authority* arises primarily from models using mathematical expressions of physical laws to represent reality. And yet climate models remain significant abstractions and simplifications of reality. On the other hand, climate models' *social authority* resides in the interactions between scientific practices, cultural performances, and political interests, interactions which endow models with the status of trustworthy "witnesses" to the truth—or not.

These two dimensions of authority relate in complex and varying ways. Understanding this relationship—and hence understanding the authority exercised in society by climate models—requires critical philosophical, sociological, and anthropological analyses. As Hastrup (Chapter 1, this volume) observes, climate models and modelling "have a social life of their own" and the practices of design, communication, and interpretation of climate model simulations are always socially embedded.

EPISTEMIC AUTHORITY

The epistemic power of climate models comes from their being rooted in strong physical theory and from their deployment of mathematical expressions of such theory to represent the physical dynamics of oceans, atmosphere, and ice sheets. For example the Navier-Stokes equations describing the motions of fluid substances are central for all advanced numerical weather and climate models.

And yet climate models remain significant abstractions and simplifications of reality. Wherever one looks in the representational structures of climate models, one finds exclusions, approximations, and parameterizations of observable physical processes. Paradoxically perhaps, the greater the number of physical processes that are represented in a climate model, owing to the expanded degrees of freedom the greater are the uncertainties in projections of future climate states made using that model. As a leading American climate modeller has expressed recently when reflecting on this paradox in the context of the next IPCC assessment due in 2013/14: "The spread in initial results is therefore bound to be large and the uncertainties much larger than for the [climate] models in the last IPCC assessment. There are simply more things that can go wrong" (Trenberth 2010: 20–21; see also Knutti 2010). Models such as these with (too) many degrees of freedom may almost be thought of as "nervous models".

These epistemic characteristics of climate models leave us with an unresolved tension. Do climate models provide answers to questions such as "how large will be human influences on the climate system during the next century?", or, rather, "do climate models generate proliferating data from which more questions emerge?" (Overpeck et al. 2011)? What exactly is the purpose of climate models: heuristic tools (metaphors even; Ravetz 2003) for understanding climate processes, or truth machines for predicting future climates? Naomi Oreskes and colleagues in their famous 1994 paper on climate model verification argued that "the primary value of [climate] models is heuristic . . . useful for guiding further study, but not susceptible to proof" (Oreskes et al. 1994: 644). Others may suggest that both functions are valid (see Heymann, Chapter 11, this volume), but if so then the relationship *between* the heuristic and predictive roles of climate models requires us to consider the social life of models.

SOCIAL AUTHORITY

The social authority of climate models emerges from the interactions between scientific principles and practices—those that give rise to their epistemic authority, as we have just seen—and the public visibility and performances of these models in the social sphere. As with Steven Shapin and Simon Schaffer's idea of socially validated knowledge through "public witnessing" of the performance of Boyle's air-pump in the 17th century (Shapin & Schaffer 1985), climate models need to be "seen" to be performing credibly and reliably. They need to be "made" trustworthy—worthy of the trust of the public. To earn their social authority climate models therefore need to inhabit public venues, displaying to all their epistemic claims of offering credible climate predictions.

These forms of "public witnessing" of climate models may include displays of computational power (images of powerful computers with captions such as "The supercomputer Tupã aims to take the world by storm"; Tollefson 2010), colour-rich animated displays of simulated virtual climates (Schneider 2012), and public endorsements from powerful (political) or trusted (celebrity) actors, as in some of the quotes listed in the introduction. Many of these forms come together in the authorisation of climate models through the cultural idiom of computer gaming. For example, the computer game *Fate of the World* released in 2010 by the Red Redemption team (http://fateoftheworld.net/) defers to climate modelling in this way—as the source of "realistic data" through which "opportunities for learning about climate change available for players are huge" and which "can have a positive impact, especially on younger players". And as an expert witness to the credibility of climate models the IPCC itself has been particularly important.

Such varied forms of public witnessing endow climate models with social authority. But note—and I shall return to this later—the particular forms and statuses of social authority acquired by climate models are culturally conditioned and therefore can vary, sometimes very substantially, both within and between societies.

CLIMATE MODEL RELIABILITY

Keeping in mind these opening considerations about the relative roles of epistemic and social authority of climate models, I will structure the following exploration in terms of the specific question: "Are climate models reliable?"

As later explained, I do not mean "reliable" in the narrow sense of whether or not models offer accurate representations of reality, but rather the broader question about the "reliability" of a climate model for particular purposes and within particular cultures. To assist in this investigation, I draw upon the work of Arthur Petersen in the Netherlands by adding two further dimensions of "reliability" to Petersen's original two-fold typology (Petersen 2006). I suggest here a four-fold typology of climate model reliability: coding precision (Reliability 0; henceforth R_0); statistical accuracy (Petersen's R_1); methodological quality (Petersen's R_2); and social credibility (R_3). We look briefly at each of these in turn.

R_0 Coding Precision: Is Mathematical Representation of Physical Theory Converted into Stable Computer Code?

This is perhaps the narrowest and most technical definition of model reliability. How well are the physical-mathematical relationships in a conceptual climate model converted into computational algorithms and thence executable computer code? Imprecision (through the choice of numerical solutions to differential equations) and errors (in often millions of line of computer code) are inevitable in this process, but is the resulting code stable? And is it portable across computational platforms and useable by others outside the original design group?

R_0 is usually assessed internally by climate modelling teams, but there may be occasions when this element of model reliability becomes contentious and demands are made to "open up" the model. Indeed, in recent years the "open-source movement" (e.g. Bradley 2005) has spread into climate modelling with organisations like Climate Code Foundation and Clear Climate Code seeking to bring greater professional scrutiny and quality control to bear on climate model codes. Pipitone and Easterbrook (2012) analyzed software from several leading climate models claiming that "in order to trust a climate model one must trust that the software it is built from is built correctly" (p.348). Their conclusion was that climate models have "very low defect densities" relative to other similar-sized open-source projects. Even so, this commitment to open-sourcing climate model code is a time-intensive task and modellers themselves may be reluctant to commit to it even in the cause of public trustworthiness. As NASA climate modeller Gavin Schmidt remarks: "Of all of the things that I can do that are important, is allowing reproducibility of my code on somebody else's computer important? No, that's not important" (reported in Kleiner 2011: 12).

Although calls for greater accountability and transparency in climate modelling are likely only to increase in the future, there may be both practical and theoretical limits as to how far the millions of lines of climate model code can be perfected. Which leads us next to consider reliability R_1.

R_1 Statistical Accuracy (or "Realism"): Do Model-Simulated Climates Bear a Resemblance with Observed Climates?

It was this aspect of climate model reliability which first brought me into direct contact with climate models. In 1988 I arrived at the University of East Anglia, hired to work on a research contract concerned with model validation funded by the UK Department of Environment (DoE). The UK DoE desired an independent analysis of how well the climate model which they funded—at the UK Met Office, later the Hadley Centre—simulated observed and palaeo-climates. My earliest work in this area was published in Hulme (1991).

Evaluating how well models simulate reality sounds relatively straightforward, but this is far from the case. There are both philosophical and practical (technical) problems involved with this task. These have been well rehearsed so I will not dwell on them here, but in summary the following points need emphasis. As pointed out by Oreskes et al. (1994), model verification is only possible in closed systems. In contrast, models of complex natural systems such as climate can never be fully verified because such models always require input parameters that are incompletely known. A second philosophical problem with climate model verification is that of underdetermination or equi-finality: differently designed and configured models may yield the same result and so model results are always underdetermined by the available data.

From a practical perspective the problems of evaluating R_1 are greater still (Lane & Richards 2001; Shukla et al. 2006; Stainforth et al. 2007; Gleckler et al. 2008). The observed data against which model simulations are verified are never fully independent of modelling assumptions: they are "model observed" data rather than "purely observed" data. Then there are the large number of performance indices against which a model can be evaluated. How do we judge which of these are most important for establishing the reliability of a model? And, thirdly, how much similarity between a model simulation and observed reality is deemed enough to establish reliability? Different levels of statistical confidence imply different levels of trust or belief in the veracity of the model (Valdes 2011).

There is the further difficulty in that model predictions of long-term (multi-decadal) climate change are impossible to verify—in the direct sense that would be used, for example, to verify daily weather forecasts. Only with the benefit of 20 or more years of observations after the prediction was made could such verification be possible. One rare example of climate model multi-decadal forecast verification is of the predictions made in 1988 by the NASA GISS climate model led by Jim Hansen. Hargreaves (2010) contrasts the 20-year predicted global warming trend (0.26°C/decade) from this climate model with that observed (0.18°C/decade), but concludes that the model prediction demonstrated substantial statistical "skill"; i.e. the model performed better than chance. Yet this type of climate model verification is rare and very limited in scope. And as argued by Oreskes et al. (1994), the underlying climate model (as opposed to the model prediction) cannot be validated by such an exercise.

R_2 Methodological Quality: Are Climate Models Well Constructed?

If R_1 is focused on the reliability of climate model *outputs*, then R_2 focuses on the quality of what we might call climate model *inputs*, namely: model structure, boundary conditions, simulation design, levels of expertise, external collaborations, and so on. Petersen and Smith (2010: 5) describe this aspect of climate model reliability thus: "That which derives from the methodological quality of the different elements in simulation practice, given the purpose of the model".

There are a variety of ways of assessing the reliability of climate models in these terms. We might assess model design and structure: is it simple, elegant, or overly complex? We might ask whether or not the modelling team followed appropriate professional standards in software design and documentation (Lane & Richards 2001). Or we might consider the levels and ranges of expertise which have contributed to climate model design. For example, should physical oceanographers rather than marine biologists design the ocean biogeochemistry module of a climate model? This was the essence of criticism levelled at the Hadley Centre modelling team by a UK House of Commons 1999 enquiry into scientific advice on climate change: "While the Hadley Centre is very expert in climate modelling and in the physics and mathematics of climate change, *it lacks expertise in other disciplines*, notably the biological sciences . . . We strongly suggest that it might benefit from more in-house staff with expertise outside meteorology, including the biological sciences" (House of Commons 1999: para 12; emphasis added).

Underlying much of this R_2 evaluation of climate models is the thorny question about whether or not different models do—or should—converge on the same simulation or prediction results. Climate models are rarely independent of each other (no one yet knows how to establish "degrees of independence" of climate models; Pirtle et al. 2010) and prediction convergence may simply imply that all models are equally wrong. Is prediction convergence across the population of climate models therefore a sign of reliable physical theory and well-designed models? Or is it merely a sign of a high level of model interdependence: the same experts, using the same algorithms, calibrated against the same data? Should we trust models more or less when they yield similar results—what does it mean when climate models agree?

The IPCC has adopted an approach which uses multi-model ensembles to quantify the range of uncertainty in climate model predictions. Each model is treated as an equally valid representation of reality and hence given equal weight in the ensemble-mean. One model, one vote. But is such a "democracy of models" the right form of representative politics when seeking the truth? Climate modeller Reto Knutti has warned against complacency here:

"There is a real danger of model convergence as a result of tuning, consensus on metrics and peer pressure, rather than improved understanding and models . . . The benefit of a more narrow projection must be compared to the potential damage of overconfident projections and wrong adaptation decisions resulting from it" (Knutti 2010: 401). Paying careful attention to the methodological quality of the "inputs" into climate models and model simulations—what is meant here by R_3—is therefore one way of warding off such unwarranted overconfidence.

R_3 Social Credibility: Are Climate Models Socially Authorised to Speak?

Considerations of R_2 are still largely contained to practices internal to climate modellers and their scientific networks (although external public scrutiny through regulated modelling and professional standards may begin to enter). But does coding precision in climate models (R_0) combined with "adequate" statistical accuracy in their simulations (R_1) and suitable methodological quality in their design (R_2) automatically generate trustworthy models? My argument is "no, it doesn't" and so a fourth aspect of climate model reliability—R_3—requires careful scrutiny, namely how climate models exist and operate as social objects.

To scrutinize climate models according to this criterion requires examination of the networks that allow models to enter, endure, and travel in society. These include the following networks with their attendant investigations:

- epistemic networks (as we have seen above): studying which experts are enlisted in model design and the (often implicit) hierarchies of expertise involved;
- financial networks: the majority of (large) climate models are funded by national government agencies and the politics of model-funding are important to unveil;
- political networks: climate change mobilizes a wide array of interests and actors and it is important to understand how climate models are deployed in the politics of climate change knowledge;
- discursive networks: language and rhetoric are used powerfully in the communication of climate model outputs and careful attention should be paid to the representations of certainty, uncertainty, and ignorance in such communications;
- performative networks: climate models claim to capture and simulate reality in virtual form and so making such realities visible requires sophisticated and subtle visualizations through animations, colours, virtual globes—these require critical scrutiny.

Through attaching themselves to and exploiting such networks climate models compete for and acquire social authority—the right to "determine, adjudicate, or otherwise settle issues or disputes". To illustrate some of these aspects of R_3, I draw upon the work of Martin Mahony and his examination of the UK Met Office's Hadley Centre's PRECIS model (Mahony & Hulme 2012).[3] PRECIS is a regional climate model of high (25 kilometres) spatial and temporal (daily) resolution, which over the last decade has been made available to over 100 countries worldwide. It is a modelling system, which has been designed to assist adaptation and development planners in the "global South". Through investigating how this one model has managed such extensive geographical reach we can see how these different enabling networks work to establish its social authority.

PRECIS carries with it the pedigree of the Hadley Centre's Earth system modelling enterprise. This pedigree of being bred from one of the world's leading climate modelling centres is jealously guarded. Although PRECIS has been distributed to over 100 countries, it is done so on certain conditions that tie the model back to its epistemic parentage. PRECIS also extends its reach across the world through the financial backing of the British Government. It has received either direct or implicit support in its developmental trajectory from three national government departments: the Ministry of Defence, the Department for Energy and Climate Change, and the Department for International Development.

PRECIS has been able to exploit international political and diplomatic climate change networks and thereby further extend its reach and authority. As one of the PRECIS development team remarked, "'It was also useful for us to have the UNDP [United Nations Development Programme] seal of approval on it' to lend credence to the chain of translation" (Mahony & Hulme 2012: 201). Working through such overtly political networks grants additional authority to PRECIS and so enables users to justify political action on the basis of PRECIS' outputs. For example, this PRECIS user from the Caribbean reflected, "We were able to convince the international audience that even though they're talking about 2[°C], 2 would be extremely detrimental to us. So . . . outputs from models like PRECIS help us in terms of our convincing of policy makers that they should take a stand" (Martin Mahony personal communication, September 2010).

The authority of PRECIS is also illustrated through its ability to engage with the discursive networks of climate change and development. In particular, its promotional material has been able to deploy the language of social vulnerability combined with scientific prediction thereby making the model "useful". In so doing PRECIS' authority is lent in support of this particular framing of climate change adaptation. As Mahony and Hulme (2012: 208) conclude, PRECIS facilitates interaction between scientific and political worlds:

> in support of a particular political sagacity. This is achieved through the . . . deployment of normative discourses of vulnerability and scientific realism, the consequence being a community pursuing [climate

> change] knowledge which possesses high spatial resolution and precision. This pursuit is facilitated by the rendering of planned adaptation as captive to, or an ancillary of, the ability to predict future climatic changes on the scales that most interest decision-makers.

Finally in my list of five enabling networks of authorisation, the epistemic authority of PRECIS is displayed performatively by showing visually its "realism" in comparison with other lower resolution models. Credibility for PRECIS is therefore established visually, as much as it is established statistically (R_1), through frequent use of coloured graphics. These emphasize the difference made by high resolutions to the representation of familiar geographic forms, whether they be coastlines or familiar meteorological features such as tropical cyclones. As Mahony and Hulme (2012: 202) explain:

> A comparison is presented of the representation of the Philippines at various spatial resolutions (400, 50, and 25km). As the resolution of the model increases, the shape of the coastlines becomes more detailed, more isles appear on the map, and the overall picture becomes one of topographical clarity with the islands recognisable to anyone familiar with the geography of the western Pacific.

This performative demonstration of PRECIS' epistemic authority suggests that the model can tell us something about the *real* world and the *real* atmosphere and is not merely a heuristic tool.

As PRECIS has moved around the world in recent years, the model has gained social authority by imposing itself on distant cultures through a process Mahony and Hulme (2012) describe as "epistemic hegemony". The PRECIS model is a good example of the co-production of scientific knowledge and social order at work (Jasanoff 2004), in this case mediated by a climate model. This extended example illustrates the many different functions this "mobile model" has secured through it global passage, functions that go well beyond Oreskes' notion of a climate model as a heuristic or Ravetz' suggestion of climate model as metaphor. PRECIS has both gained and exercised authority in society.

Further insight into the social authorisation of climate models comes from considering how climate models have been deployed in two different science-policy cultures: the UK and the Netherlands. In recent years both countries have created sets of national climate scenarios of the future, commissioned by their respective central governments. Both countries have strong scientific traditions and have valorised evidence-based policy. And yet in the design of these respective climate scenarios climate models have been granted different degrees of authority over the (climatic) future.

In the UK, the national scenarios developed in 2009 (Murphy et al. 2009) drew almost exclusively on climate model simulations and in particular on one model hierarchy from one modelling centre (the Hadley Centre). This was justified through claims that it offered the world's most advanced climate modelling system. Various sophisticated statistical techniques were used to convert model output into probabilities of future weather outcomes at very fine temporal (hours) and spatial (5 kilometres) scales. In the Netherlands, however, the four national climate scenarios were developed using a greater diversity of methods and techniques than in the UK (KNMI 2006). Although climate models and their simulations remained important, the exercise sampled a wide spread of model hierarchies and combined model simulations with historical evidence, local meteorological reasoning, and expert judgement. Lesser authority was granted to a single climate modelling system and its simulations than in the UK case.

It is enough for my purpose here to show that R_3 can vary radically across different cultures and decision-making practices, even if climate models are adjudicated to possess similar degrees of reliability across the other three levels of assessment. In this example of climate scenario construction, climate models are granted very different authorisations to create and guide descriptions of future climates, which may then be used to inform public (or private) decision-making. There is a note of caution here for the way in which the IPCC conducts its work and establishes its universal knowledge claims based on models. Its authoritative deployment of climate models with their representation of putative future climates becomes potentially dangerous if in so doing the IPCC erases, or is oblivious to, differences between cultures in the social authority that is granted to these climate models (Hulme 2010).

CONCLUSION

This chapter has explored how climate models gain and exercise authority in society. There is no doubt that climate models offer a powerful way—the single most powerful way—for scientists to organize their knowledge about the physical Earth system, to understand the material interconnections between different parts of that system, and to help identify key sensitivities within it. To construct, maintain, and use a model implies at least a minimal level of understanding of physical causation in the complex Earth system, and an ability to re-create features of that reality in a simulation machine. Climate model simulations must have some correlate in the observable physical world. If they do not, then as much effort must be invested in understanding the behaviour of the climate model as in understanding the physical Earth system. It is the model that is deficient in some respect, not reality (Lahsen 2005). Climate modelling has in many ways therefore become a behavioural science: a science which studies the behaviour of climate models.

Whether the public, and the politicians they elect, should trust climate models when they are used to prognosticate about the far future—and hence whether they should defer to decision-making calling upon the authority of models—requires an additional set of questions to be answered. It is not enough for climate modellers to speak about the stability of their code (R_0), or about the fidelity of their simulations (R_1), or about the quality of the underlying model structures and design processes (R_2). Even less is it enough to be told that all climate models (broadly) agree. To gain authority within certain forms of democratic life it is important that the networks and practices that support and authorise the social life of climate models are subject to critical scrutiny. As with other authoritative voices and institutions in society (Brown 2009), climate models and their networks must be held accountable to broader sets of public norms and standards.

These norms are socially constructed and they will therefore vary between cultures and nations. It is insufficient to assert that climate models possess universal and uniform authority simply on the basis of their epistemic power. Such claims are common in the world of climate modelling—just as they are often also subliminal, as in this recent example commenting on new developments in Brazil's modelling community: "The [new] supercomputer could help to earn Brazil a place in the small club of nations that contributes global climate-modelling expertise to the IPCC" (Tollefson 2010: 20). The implication here is that a new generation of climate models operated through a new powerful supercomputer will not just enhance Brazil's scientific modelling capacity, but will also enhance Brazil's political authority in the "club of nations".

Beyond such superficial claims, it is understanding the social credibility of climate models—what I have termed here R_3—that is critical. For climate models to gain the status of "trustworthy witnesses" it is necessary but insufficient that they be evaluated against the reliability criteria R_0, R_1, and R_2. Rather, R_3 has to be evaluated, case-by-case, keeping in mind the distinct "civic epistemologies" of different political cultures (Jasanoff 2005). And ultimately it is the ways in which the claims of epistemic authority are socially validated that yield greatest insight into how climate models gain and exercise authority in society.

Climate models "take on a life of their own once they have been unleashed into society and politics" (Hastrup, Chapter 1, this volume, p. XX). They need to be studied not merely as tools of scientific enquiry, but as powerful social objects. Such study cannot be left to climate modellers. We need the insights and tools of philosophy, sociology, and anthropology to understand how climate models gain authority and how this authority is exercised differently around the world.

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NOTES

1. In this chapter I use "climate models" as the generic term to describe the whole family of numerical climate models, which includes simple one-dimensional models, intermediate complexity models, general circulation models, and Earth system models.
2. www.dictionary.reference.com.
3. PRECIS stands for: *P*roviding *Re*gional *C*limates for *I*mpacts *S*tudies.

REFERENCES

Bradley, D.A. 2005. "The Divergent Anarcho-utopian Discourses of the Open Source Software Movement." Canadian Journal of Communication 30, 585–611.

Brown, M.B. 2009. Science in Democracy: Expertise, Institutions and Representation. Cambridge, MA: MIT Press.

Church of England 2009. Church and Earth 2009–2016: The Church of England's Seven Year Plan on Climate Change and Environment. Church of England, Lambeth Palace, London. http://www.shrinkingthefootprint.org/misc_lib/14.pdf.

Gleckler, P.J., K.E. Taylor & C. Doutriaux 2008. "Performance Metrics for Climate Models." Journal of Geophysical Research 113, D06104. doi:10.1029/2007JD008972.

Goddard, L., W. Baethgen, B. Kirtman & G. Meehl 2009. "The Urgent Need for Improved Climate Models and Predictions." EOS 90 (39), 343–44.

Hamilton, C. 2010. Requiem for a Species: Why We Resist the Truth about Climate Change. London: Earthscan.

Hargreaves, J.C. 2010. "Skill and Uncertainty in Climate Models." Wiley Interdisciplinary Reviews Climate Change 1 (4), 556–64.

House of Commons 1999. Scientific Advisory System: Scientific Advice on Climate Change. Third Report of the Science and Technology Select Committee, London.

Hulme, M. 1991. "An Intercomparison of Model and Observed Global Precipitation Climatologies." Geophysical Research Letters 18, 1715–18.

Hulme, M. 2010. "Problems with Making and Governing Global Kinds of Knowledge." Global Environmental Change 20 (4), 558–64.

IPCC 2007ab. Climate Change 2007: The Physical Science Basis. S. Solomon, D. Qin, M. Manning, M. Marquis, K. Averyt, M.M.B. Tignor, H.L. Miller Jr. & Z. Chen, WGI Contribution to the Fourth Assessment Report of the IPCC, Cambridge: Cambridge University Press.

IPCC 2007b. Climate Change 2007: Synthesis Report. Cambridge: Cambridge University Press.
Jamison, A. 2010. "Climate Change Knowledge and Social Movement Theory." Wiley Interdisciplinary Reviews Climate Change 1 (6), 811–23.
Jasanoff, S., ed. 2004. States of Knowledge: The Co-production of Science and the Social Order. London: Routledge.
Jasanoff, S. 2005. Designs on Nature: Science and Democracy in Europe and the United States. Princeton, NJ: Princeton University Press.
Kleiner, K. 2011. "Data on Demand." Nature Climate Change 1 (1), 10–12.
KNMI 2006. Climate in the 21st Century: Four Scenarios for the Netherlands Royal Netherlands Meteorological Institute. Netherlands: De Bilt.
Knutti, R. 2010. "The End of Model Democracy?" Editorial comment. Climatic Change 102 (3), 395–404.
Lahsen, M. 2005. "Seductive Simulations? Uncertainty Distribution around Climate Models." Social Studies of Science 35, 895–922.
Lane, S.N. & K.S. Richards 2001. "The 'Validation' of Hydrodynamic Models: Some Critical Perspectives", in M.G. Anderson & P.D. Bates, eds. Model Validation: Perspectives in Hydrological Science, Chichester: John Wiley & Sons Ltd.
Mahony, M. & M. Hulme 2012. "Model Migrations: Mobility and Boundary Crossings in Regional Climate Prediction." Transactions of the Institute of British Geographers 37(2), 197-211.
Manuel-Navarette, D. 2010. "Power, Realism and the Ideal of Human Emancipation in a Climate of Change." Wiley Interdisciplinary Reviews Climate Change 1 (6), 781–85.
Murphy, J.M. et al. 2009. UK Climate Projections Science Report: Climate Change Projections. Exeter: Met Office Hadley Centre.
nef 2008. 100 Months Technical Note, 4pp. http://www.neweconomics.org/sites/neweconomics.org/files/100_Months_Technical_Note_1.pdf.
Oreskes, N., K. Shrader-Frechette & K. Belitz 1994. "Verification, Validation and Confirmation of Numerical Models in the Earth Sciences." Science 263, 641–46.
Overpeck, J.T., G.A. Meehl, S. Bony & D.R. Easterling 2011. "Climate Data Challenges in the 21st Century." Science 331, 700–702.
Pall, P., T. Aina, D.A. Stone, P.A. Stott, T. Nozawa, A.G.J. Hilberts, D. Lohmann & M.R. Allen 2011. "Anthropogenic Greenhouse Gas Contribution to Flood Risk in England and Wales in Autumn 2000." Nature 470, 382–85.
Petersen, A.C. 2006. Simulating Nature: A Philosophical Study of Computer-Simulation Uncertainties and Their Role in Climate Science and Policy Advice. Amsterdam: Het Spinhuis.
Petersen, A.C. & L.A. Smith 2010. Reframing the Reliability of Models Moving from Error to Quality for Use. Draft paper for the ESF workshop Exploring Epistemic Shifts in Computer Based Environmental Sciences, 10–12 June, Aarhus University, Denmark.
Pipitone, J. & S. Easterbrook 2012 "Assessing Climate Model Software Quality: a Defect Density Analysis of Three Models." Geosci. Model Dev. Discuss., 5, 347–382.
Pirtle, Z., R. Meyer & A. Hamilton 2010. "What Does It Mean When Climate Models Agree? A Case for Assessing Independence among General Circulation Models." Environmental Science & Policy 13, 351–61.
Ravetz, J. 2003. "Models as Metaphors", in B. Kasemir, J. Jaeger, C.C. Jaeger, & M.T. Gardner, eds. Public Participation in Sustainability Science. Cambridge: Cambridge University Press (p.62–78).
Schneider, B. 2012. "Climate Model Simulation Visualisation from a Visual Studies Perspective." Wiley Interdisciplinary Reviews Climate Change 3(2), 185–193.

Shapin, S. & S. Schaffer 1985. Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life. Princeton, NJ: Princeton University Press.
Shukla, J. et al. 2006. "Climate Model Fidelity and Projections of Climate Change." Geophysical Research Letters 33, L07702. doi:10.1029/2005GL025579
Stainforth, D.A., M.R. Allen, E.R. Tredger & L.A. Smith 2007. "Confidence, Uncertainty and Decision-Support Relevance in Climate Predictions." Philosophical Transactions of the Royal Society A 365, 2145–61.
Tollefson, J. 2010. "Brazil's Climate Modellers Are Set to Go Global." Nature 468, 20.
Trenberth, K. 2010. "More Knowledge, Less Certainty." Nature Reports Climate Change 4 (February), 20–21.
Valdes, P. 2011. "Built for Stability." Nature Geoscience 4, 414–16.

3 Certain Figures

Modelling Nature among Environmental Experts in Coastal Tamil Nadu

Frida Hastrup

FIGURATIONS OF KNOWLEDGE: MODELLING REAL ENVIRONMENTS

Among other ventures undertaken at the Center for Advanced Studies in Marine Biology in the town of Perangipettai in Tamil Nadu, India, scientists, including biologists, aquaculturalists, and resource managers, are currently involved in a so-called Potential Fishing Zone Forecasting Project. In collaboration with local fishermen working the waters off the coast of the neighbouring Bay of Bengal, and in response to recent problems with excessive fishing, changes in seasonal regularity, shifts to the seabed and currents, and other such trying characteristics of the Tamil Nadu marine environment, the researchers have initiated a project of trying to systematically take stock of the immediate natural surroundings with a view to ensuring a sustainable fishing trade in the future.

Explaining to me that the coastal area is considered vulnerable both in itself as an ecosystem susceptible to even minor shifts in natural cycles and human intervention, and as a means of livelihood which feeds thousands of people, the researchers consider it necessary to, quite literally, get to the bottom of things and map the local underwater topography, water depths, the stock of fish and other aquatic resources, and so on, in order to try to shield and sustain both the marine ecosystem and the people depending on its yield. As the title of the current marine biology project indicates, such mapping is an exercise of forecasting, of charting a potential, or, in the terminology of this volume, of anticipating nature.

On the basis of my talks with some of the involved scientists during fieldwork in coastal Tamil Nadu in the spring of 2010, my overall analytical aim in this contribution is to explore how environmental expertise emerges as a particular figure in the words and world of the marine researchers—a figure that is articulated as both numerically accurate and inherently vague.

Science studies have long since shown that (natural) scientific facts are not free-floating universals to be harvested by a detached observer, but produced in the practical settings where the scientists pursue them (see e.g.

Latour 1999). Although this insight is crucial, what I am after here, however, is not to go backstage and unveil how scientific facts, such as those seen as characterizing the marine environment in Tamil Nadu, are created in, say, social, political, or institutional practices. Instead, I dwell on a specific conversational situation that occurred during my fieldwork and I bring it to bear as a general qualification of how anthropology might address the anticipation of nature and, more broadly, the production of objects of knowledge in analyses. As I will show in the following, the scientists at the marine biology centre outlined a complex model of environmental insight, which has an inbuilt paradox: all while being boldly anticipatory from the outset, the model modestly points to the limits of anticipation by stating that part of nature will always remain an inaccessible mist outside of our grasp. By thus explicitly modelling what in a way cannot be known and positing a whole that remains elusive, the scientists combine measures of courage and humility in a manner that I see as exemplary for how we might imagine the craft of anthropological analysis.

Through pursuing, during my fieldwork as well as in this chapter, the question of what the marine scientists claim to be doing when they engage in a forecasting of the potential held by the marine environment, I investigate a specific process of modelling, in which nature and knowledge about it are co-produced in collective actions and articulations. Emerging from my talks with the marine scientists is a particular figuration of the world, knowledge and knower, in which these are articulated as completely entangled so as to refuse that justification of a claim can be provided from outside of the specific situation at hand (see Verran 2001: 33ff, 2010). In this light, both nature and the theories about it are things in the world (Helmreich 2011: 138).

In the following I thus call upon the Tamil marine scientists to develop with me the overarching argument that there is no point from where to grasp the environment except from inside of it, and that this makes anticipatory modelling of nature appear as a kind of hyper-real exercise—situational, experiential, and empirical at the core, even when it invokes numerical abstraction or explicitly grapples with what remains vague. What the scientists offer, as do responsible anthropologists committed to the creativity of people in collective life, is a brave kind of theorization about the world that makes a point out of its analytically inexhaustible nature and takes into account that objects of knowledge are not pre-arranged given entities, but subtly modelled in conversational life by way of a synthesis of data and theory, figure and ground, fact and configuration, observation and perspective that are always co-produced and complicit in each other (cf. Brichet & Hastrup 2011; Verran 2001: 35).

To trace how the marine scientists and I talked such complex modelling of environmental expertise to life, I now turn to the actual scene of our encounter in the Tamil coastal environment and at the biology research centre and to the different stages in our conversation. Along the way, I also turn to

Anna Tsing (2010) for her to help me conceptualize the scientists' paradoxical flipping back and forth between boldness and modesty in their figuration of environmental knowledge. In the final section, I discuss how the marine researchers' theorization can speak more generally to the making of anthropological objects of study and to practices of modelling in the world.

COUNTING THE ENVIRONMENT: ACCURACY AND ACCUMULATION

In recent years, concerns of various kinds and intensity have visited coastal Tamil Nadu, and all of these challenges make up the environment of my fieldwork as well as of the ongoing marine research. My conversation with the scientists took place against the constant beating of the waves from the Bay of Bengal and the sometimes surprisingly forceful gusts of wind. In the low-lying coastal areas of the region rapid erosion of the shoreline causes the sea to steadily encroach upon the fishing populations, cyclones regularly wreck huts and fishing gear in the settlements along the coast, and in 2004 the Asian tsunami shattered the lives of thousands in the region (see Hastrup 2010, 2011). These problems, in turn, have occasioned an assemblage of protective measures to be taken, such as coastal plantations, rock sea walls, conservation of mangrove forests, programmes of relocation of people, and the promotion of disaster-resistant building practices. Introduction of increasingly efficient fishing equipment combined with market forces have also put a strain on the marine resources in all of Tamil Nadu, calling for yet other initiatives to sustain the coastal populations (Bavinck & Karunaharan 2006).

At present, this array of pressing coastal concerns, shared by experts and lay people who each in their way strive to keep uncertainties at bay and to anticipate a likely course of events in their surroundings, characterizes life along the coast. In the case of the marine researchers I feature here, the protective work they do is abetted by intricate technology to ensure scientific and experimental accuracy. Shown around at the marine biology centre, I was introduced to a world of sophisticated machinery: this microscope could show the genetic development of a particular mangrove shrimp, that centrifuge would reveal the composition of a water sample, and a screen provided updated satellite forecasts as part of an Ocean Information System. These and other complex instruments, in which the researchers clearly trusted, testified to the point made by Latour that natural scientific facts can be seen as all the more real and reliable for their carefully fabricated nature (2005: 90).

The technology at the research centre made up a world designed, as it were, to wrest knowledge of nature free from nature. One might say that nature was both the means to and end of the exercise of mapping the coastal environment in the world of the marine researchers, who wanted to leave no stone unturned. During our talks, the scientists articulated a positive belief in natural science and in the indisputability of objective evidence; facts of nature, it seemed, were out there to be harvested little by little through natural scientific observation and systematic compilation. In recounting how more precisely they went about the mapping of the marine environment, the scientists described to me how a small team of researchers from the project would set out in their boats, joined by local fishermen, and go to a locality where the fishermen would know from experience and past observations that a particular type of fish would be likely to roam in more or less predictable quantities at specific times of day and in specific seasons. Along the way and at the chosen destination, the marine scientists would make measurements of the depths, investigate the seabed, take samples from the water and the bottom, throw out nets, and make counts of the catches, so as to make estimates of the stock, conditions, and fluctuations of the marine resources. After analyzing the collected samples and data in the laboratories or assessing the more obvious counts and results, the researchers would discuss all of the findings with the fishermen, in recognition of their long-term experience in harvesting and monitoring the sea and its resources. Through this collaborative procedure the people involved in the project would try to ascertain whether and to what extent changes in the marine environment had appeared in recent times when compared with previous experiences and memories of yields and conditions in the past. At the time of my visit to the research centre, the scientists were in a process of repeating this procedure over and over in different selected locations along the coast and at different times, so that a body of data could gradually be accumulated. The explicit purpose of the exercise, as the researchers told me, was to collect as much field evidence as possible in order to be able to measure the resource base of the sea in the immediate coastal regions and give scientific advice about future fishing activities to the benefit of both fishing communities and ecosystem. Data collection was thus presented to me as the key activity in the effort of forecasting in the project, and accumulation of information was the desired goal. Through careful recording and an ensuing transfer of data from out there in the wild to a supposedly more controlled scientific realm elsewhere, the researchers seemed to imply that the facts collected from the sea might eventually add up to a full picture of the marine resources from end to end.

In a recent publication Anna Tsing (2010) has proposed the concept of *worlding* as a useful term to capture what she refers to as the always partial and experimental ascription of world-like characteristics to the social realm. A focus on worlding, Tsing suggests, is a tool for examining the perceived and often quite implicit relations between parts and wholes; it shows a particular—if frequently unrecognized—figuration of elements in scenes of social encounter, all the while pointing to the instability of such explanatory frameworks for whatever is under study. Writing specifically about the study of science, Tsing uses worlding as a means of capturing dynamic forms of contextualization of scientific data. As Tsing notes, "All researchers, including both natural and social scientists, use worlding to assess their research materials, that is, to place them in what seem to be relevant webs of relationality" (Tsing 2010: 50). As an activity that resides with scientists of all bends, worlding can thus show which "world" the data are thought to have come from and what accordingly makes up the relevant context in which to understand them. At the marine centre, the relevant context for the individual bits of data seemed to the researchers to be given by the shifting coastal nature as a whole—the world out there—the comprehensive account of which could eventually be provided. The marine scientists, it appeared from their remarks, collected natural pieces of an equally natural puzzle; the question and the answer were somehow the same.

Given this immediate and perhaps unsurprising trust in natural science expressed by the researchers, I was curious to know more about the role of the participating local fishermen, and I went on to probe into the matter. Because the fishermen were involved in the Potential Fishing Zone Forecasting Project in the first place, the marine researchers must be ascribing an ability to decode the natural surroundings to their amateur counterparts as well as to themselves and their laboratory machinery. What intrigued me was that the fishermen seemed to be involved in the project not just as informants but as conversational partners. They were there to *discuss* the findings with the scientists. The parties, it seemed, had agreed to speak, indicating that they had some kind of common interest at heart, even if they articulated their knowledge about their shared environment in different ways (cf. Tsing 2005: xi; Verran 2002). When I asked about the role of the fishermen in what appeared to be a collective activity of talking about the environment with the shared aim of proper forecasting, one of the researchers was very clear about the contribution by the fishermen. The researcher told me that in his view the scientists "infuse knowledge with science". Knowledge, based on previous experience, he elaborated, resided with the fishermen, whereas science, founded on experiments to be accurately repeated to form a database and a systematic future prospect, was the domain of the researchers. Knowledge, it seemed, was somehow seen as nesting within science, which, in turn, was seen as adding an extra layer of explanatory and prospective power to the fishermen's embedded knowing. The research materials to be assessed and placed in webs of relations in the world of the staff at the marine biology centre, then, was a form of knowledge of the environment, which included both classic natural scientific data such as water samples, counts of catches, etc., and the local fishermen's insights.

What is interesting here, I think, is not so much the perhaps predictable detection of an implicit, if polite, hierarchization of lay and expert knowledge, according to which the latter contains the former. On the contrary, I suggest that what is in fact important here is not really the qualitative difference between the two knowledge forms, as the ways in which these knowledge forms are seen as quantitatively contributing to a common aim of mapping the marine environment. To put it shortly, the local fishermen were involved in the project because they *also* had environmental expertise, which could bring the marine scientists nearer the goal of mapping the coastal nature. In the worlding of the marine researchers, it seemed, both parties were procuring different and particular parts to the same general whole.

So what are we to make of this modelling of the environment that combines a belief in data being out there to be gradually accumulated and systematized when transported to the realm of science with a need for a discussion of facts during and after their processing? Surely, this is not just a straightforward scientism, which posits a clear distinction between lay and expert knowledge along the lines of random observations versus scientific experimental accuracy, and which I can then critically unearth. Such a simplistic dichotomy would belittle the seemingly generative role of the conversation that took place both between the fishermen and the marine scientists, and the marine scientists and the anthropologist. If, again, we follow the lead of Tsing and the idea of worlding as an always provisional and unstable, yet necessary, form of contextualization of data, we might get a clue on this issue. One of the virtues of worlding, according to Tsing, is that it carves out an avenue to explore how figure and ground constitute each other in scientific analyses and are created simultaneously in ongoing processes of figuration. The point here is that grounds are figured. This is to say that the "natural whole", i.e. the marine environment, the anticipation of which both researchers and fishermen contribute to, emerges as a figure in the words of the researchers, rather than as a given ground on which the individual observations rest. Entities that appear natural are brought to life in collective social action, which, needless to say, does not make them any less real. To the contrary, these entities are as real as anything precisely because of their inherently situational qualities, only they are not inevitable. As Tsing remarks, the "gift of worlding is its ability to make figures appear from the mist and to show them as no more than figures" (2010: 64). This implies that even figurations of environmental knowledge that in one way or another invoke nature categories as foundational and given can be seen as generative of reality, enacting the environment in a specific way, accomplishing a world in the going-on (cf. Lavau 2011: 43; Lien & Law 2011; Verran 2001: 37).

In the worlding of the marine scientists, then, environmental expertise was a performative figuration rather than a matter of accurate representation even if they believed in the objective validity of scientific facts. The facts were contextualized as parts of a natural whole, but given that this was very much an effect of conversation, the context was no longer a stable and given ground, but appeared as a multiple figure. Paradoxically, this became clear to me when, during our talks, the researchers repeatedly referred to numerical figures and calculations in order to qualify their knowledge of the environment. In the following, I turn to look at how, curiously, ideas of percentages came to express the unstable and multiple nature of the scientists' modelling of the environment.

SCALING THE ENVIRONMENT: THE NATURE OF PERCENTAGES

At some point in the course of our talks, one of the marine scientists suggested that there is a difference between being able to manage the environment and being able to control it. This distinction intrigued me, and I asked him to elaborate. He went on to say that he and his colleagues worked on the assumption that they would never be able to reach a point where nature was comprehensively known; to assume otherwise was to entertain a mythic sense of environmental controllability that would never be obtained, regardless of the number of scientific experiments, samples, and other data including the fishermen's observations they collected and carried out. Something would always escape their scientific embrace, as the researchers told me, but what they could do was to learn to manage the environment by way of experimental procedures. Interestingly, when we discussed this further, the scientists expressed a surprisingly clear idea about this ever-elusive part of the environment: in their estimate 20% of nature would remain out of science's reach, whereas 80% of its features could be revealed through careful scientific accumulation and ensuing discussion of facts. The exhaustive 100% insight, they maintained, is unobtainable, whereas 80% knowledge is within scientific reach; the researchers did not regret this, but seemed to acknowledge as a premise and from previous experience that this was simply as close as they could get and that this was what they should strive for. In this way, the figure of 80% becomes the whole within which the marine science is performed.

What we see here is a model of environmental knowledge in which percentages are not necessarily parts of 100—a worlding where the context, the 100% enveloping frame, is explicitly made to appear as fictitious. In the light of this, one might say that the scientists at the marine biology centre somehow operated with two figures of knowledge within one seemingly contradictory model: by referring to percentages they seemed to imply a totality, all while recognizing that this totality would remain an unattainable fiction, whereby the totality was somehow downscaled to become less than itself.

What is interesting in relation to the larger question of modelling nature is that each of these two figures of environmental knowledge, that is, the whole and its parts, a controlled and a managed nature, referred to in terms of 100% and 80%, respectively, come to life through each other, the one at once highlighting and negating the other in a discussion between them. Marilyn Strathern has written about such awkward relations between phenomena and argued that the appearance of one figure through the other is a kind of mocking that shows the fragility of both (Strathern 1987). The fact that there even are two (or more) figures shows their fragility, understood as an implicit recognition of an other, a recognition of each as no more than a figure, as Tsing phrased it. The totality (100%) appears through the elusion inherent in the scientists' claim that they would only ever know 80% of nature's secrets, and the parts—the percentages—appear through the fact that they will never add up to the (whole) whole. To me, what is important here is the mutual implication of the two figures of the 100% and the 80%, and the way in which they are both somehow cut out of each other. In order to focus on one number, one must necessarily infer the other. What we see here is a peculiar kind of collaboration between the two—a mutual dependence where the one is both confirmed and negated by the other. In that sense, the co-existence of the two figures in one unstable, self-undermining yet creative model becomes a specific perspective on the world, a point of view through which nature—as controllable or manageable—can be conceptualized in the scientists' imagery. The dual model is a world-making figuration of parts and whole. As Tsing has it: "We can only identify figures to the extent that we can imagine worlds, that is, the systems of relationality through which figures emerge. Figures are relational elements of worlds" (Tsing 2010: 50).

More generally, I suggest that this (lived and apparently unproblematic) paradox of the 80/100% model, where each figure negates the other all the while making it appear, shows the inevitability of perspective or scaling in all models, whether of environmental knowledge, climate change, or what have you. It takes a concerted effort, that is, a certain perspective to make a (known or unknown) fact of nature appear as such, even when numerical calculations of, say, micro-organisms in a water sample form the scientific database. This, importantly, is not meant as a straightforward relativist position stating that people can perceive of environmental knowledge in different ways, nor is it a constructivist point emphasizing the created character of scientific knowledge. Rather, taking a cue from Helen Verran, it is a matter of recognizing realness as emergent within concrete social settings (Verran 2001). As Verran has shown on the basis of her experiences with teaching mathematics to school teachers and children in Nigeria, even something as seemingly universal and given as numbers need to be given a particular guise in particular settings of social interaction for them to work. In other words, numbering, measurement, and calculation need to be enacted in specific ways for numbers to appear as natural. Numbers, in this sense, are not representative but constitutive of entities in the world—entities such as a controllable nature and a manageable environment (cf. Verran 2010: 176). In Verran's thinking, it is only through collective actions in what she calls the "here-and-now" (2002) that we can access the world and the parts of which it is thought to be made up. In the light of this, yet another layer of conversation can then be identified as generative of and generated in reality, namely the dialogue between theory (perspective) and data (nature). Recently, Stefan Helmreich (2011) has suggested an analytical approach that operates "athwart theory", implying that both theory and what is theorized are seen as things in the world, which further suggests a flipping back and forth between seeing theory as an explanatory tool, and a tool to be explained (Helmreich 2011: 138). In the case of the figuration of environmental knowledge articulated by the Tamil marine scientists, data and theory are brought into just such athwart conversation, in that the idea of an 80% manageable nature is both a theory and a real object, as is the idea of a 100% exhaustion of nature's make-up. In the light of this thinking inspired by Helmreich, I want to point out that it is not as if the accumulation of data such as, say, a water sample and a statement from a local fisherman can be abstracted to become a theory of the 80% knowable nature conceived elsewhere, somehow outside of the situation at hand—it is not a relation of a given piece of data from the outside being theorized in science. The point made clear in the course of my being with the marine scientists is that such distinction cannot be made; both are figures that explain and must be explained. What is interesting here is that this finding of theories and data as both mocking and co-producing each other was articulated by the marine scientists themselves in their view of conversation between different figures (numbers as well as persons) as generative of their subject matter and scientific modelling ambition.

By thus exploring the peculiar nature of percentages in the environmental model of the marine researchers, we see how in social conversational life coherence is neither a pre-requisite for knowledge-making, nor the order of the day; rather, paradoxes roam freely without a paralyzing effect on people who readily live with multiplicity and a co-existence in a single moment of seemingly contradictory elements (cf. Mol & Law 2002). Returning to Tsing's concept of worlding as an implicit framing that refuses the fictitious stability of context while positing a temporary part-whole relationship, at stake in knowledge-making is not to feed more facts into models for them to work, but to recognize that the very conceptualization of some feature or other as an environmental fact—whether knowable or unknowable—is a matter of perspective, and thus of figuration of parts within a frame or within a story. In Tsing's words: "Formal methods produce data points, but they do not in themselves transform the data into a story. That work requires setting a systematic context, a "world", in which the data form part of a pattern. To understand scientific story-telling practices, it is necessary to appreciate the work of worlding, even where it is downplayed or denied in lavish descriptions of formal methods" (Tsing 2010: 50).

At the Tamil marine biology centre, the scientific story-telling practice was literally a telling of the world, in which fishermen, researchers, and anthropologist all had a say. In sum, so to speak, I suggest that the very notion of percentages as invoked by the researchers works aptly as a conversational concept, in that it inherently discusses the mutually mocking nature of parts and whole, and thereby takes the analytically inexhaustible nature of the world into account.

MODELLING REMAINS: JOINING THE CONVERSATION

Through concentrating on the situations that brought the dual model of environmental expertise to life, my analytical move has been to take "scientific accountings—including social scientific ones—as events in the world in need of examination", as suggested by Helmreich (2011: 138). The question, then, is what are we to make of the event of the scientists' accounting of environmental expertise as a dual model that is both bold and modest? What are the implications of the fact that the scientists calculate what by definition cannot be calculated and have no problem with it? One answer is to see this duality as an invitation to join the discussion and take from it a generative idea of analysis that sees it as taking place squarely in the world where all kinds of people can have a say in the simultaneous and entwined process of *creating* and *understanding* the environment. In this light, it is not a problem that models, as Tsing has noted, have a tendency to breed more models in an unfinished process of multiplication. As she has observed, the quest for certainty with regard to environmental knowledge entails a curious contradiction; as she puts it: "Models are made more reliable by incorporating uncertainties into the model, that is, by modelling them" (Tsing 2005: 104). What is at stake here, to me, is the recognition of the fact that anthropological knowledge-making can never exhaust the world analyzed. Strathern has captured this in a poignant way by identifying what she calls the "remaindering effect" of anthropological analyses (Strathern 2004); whatever parts we choose to focus on by cutting them out of the world as particular objects of inquiry, there will be an excess of other parts left out of our field of vision. This goes to show that as a particular creative cut, the 100% controllable environment had an 80% manageable nature nesting within it and—importantly—vice versa. As Strathern has it, perspectives simply produce further perspectives (ibid.: 108). The creation of some feature or other as an analytical object is thus a temporary closure—an unstable worlding—through which a figure is carved out as such of a larger whole. In that sense, any figure comprises what it is not, that is, other figures. Other cuts would have been possible and could have caused yet other figures to appear, and produced yet other perspectives in an unending process.

The biologists' thinking in terms of percentages that do not add up to 100 is in a literal or even numerical sense an articulation of figures comprising what they are not. More generally, then, the remaindering effect of figuration is a condition that scientists and lay people share, because any figuration is a creative accomplishment undertaken in scenes of social encounter and on the basis of a certain perspective, rather than a representation of given and natural entities. Interestingly, one might add, it is not necessarily the elusive 20% of nature's mystery that qualifies as the remainder; the 80% or even the unattainable totality of the 100% can equally be cast as remaining outside of the analytical embrace, because it is the very notion of percentages not adding up to 100 that entails the undermining of figures as given, while creating them.

The point is that the researchers' figuration of environmental knowledge as simultaneously incomplete and total illustrates a modelling of knowledge that has instability built in to it. The conversational figuration of environmental knowledge on the part of the marine scientists I engaged with during fieldwork is thus a paradox that is productive to think with, because it has a general bearing on the mode of anthropological analysis, where totalities (and their parts) must be perceived as figures carved out as real but not inevitable, as certain but not accurate, and where working fictions form the core of anthropological story-telling.

What we can and must anticipate, then, is perhaps not so much the course or quality of nature, but the fact that we can never catch up with the environment and analyze it as if it were a totality, which we can see from the outside, and from which we can harvest ever more data that can then be transported to a scientific realm for theorization. This goes for marine scientists, as well as for anthropologists; we are well beyond laying any claims to simply be representing an indigenous environmental knowledge collected from out there in the wild, and domesticated in scientific analyses. Fact and figuration, perspective and observation are much too intertwined for such myths. Finishing off such myth of modelling of nature as a cumulative and exhaustive activity, however, yields a vertiginous and humbling analytical possibility of modelling among the people we engage with by boldly joining in the very conversation that places anthropology in the world.

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REFERENCES

Bavinck, M. & K. Karunaharan 2006. “A History of Nets and Bans: Restrictions and Technical Innovation along the Coromandel Coast of India.” MAST 5 (1), 45–59.

Brichet, N. & F. Hastrup 2011. “Figurer uden grund: Museumsansamlinger og globale klimaforandringer.” Tidsskriftet Antropologi 64, 119–35.

Hastrup, F. 2010. “Materializations of Disaster: Recovering Lost Plots in a Tsunami-Affected Village in South India”, in M. Bille, F. Hastrup, & T.F. Sørensen, eds. An Anthropology of Absence: Materializations of Transcendence and Loss. New York: Springer (p.99–112).

Hastrup, F. 2011. Weathering the World: Recovery in the Wake of the Tsunami in a Tamil Fishing Village. New York: Berghahn Books.

Helmreich, S. 2011. “Nature/Culture/Seawater.” American Anthropologist 113 (1), 132–44.

Latour, B. 1999. Pandora’s Hope: Essays on the Reality of Science Studies. Cambridge, MA: Harvard University Press.

Latour, B. 2005. Reassembling the Social: An Introduction to Actor-Network-Theory. Oxford: Oxford University Press.
Lavau, S. 2011. "The Nature/s of Belonging: Performing an Authentic Australian River." Ethnos 76 (1), 41–64.
Lien, M. & J. Law 2011. "'Emergent Aliens': On Salmon, Nature, and Their Enactment." Ethnos 76 (1), 65–87.
Mol, A. & J. Law 2002. "Complexities: An Introduction", in J. Law & A. Mol, eds. Complexities: Social Studies of Knowledge Practices. Durham: Duke University Press (p.1–22).
Strathern, M. 1987. "An Awkward Relationship: The Case of Feminism and Anthropology." Signs 12 (2), 276–92.
Strathern, M. 2004 [1994]. Partial Connections. Walnut Creek, CA: Altamira Press.
Tsing, A.L. 2005. Friction: An Ethnography of Global Connection. Princeton, NJ: Princeton University Press.
Tsing, A.L. 2010. "Worlding the Matsutake Diaspora, or, Can Actor-Network Theory Experiment with Holism?", in T. Otto & N. Bubandt, eds. Experiments in Holism: Theory and Practice in Contemporary Anthropology. Oxford: Wiley-Blackwell (p.47–66).
Verran, H. 2001. Science and an African Logic. Chicago: University of Chicago Press.
Verran, H. 2002. "A Postcolonial Moment in Science Studies: Alternative Firing Regimes of Environmental Scientists and Aboriginal Landowners." Social Studies of Science 32 (5–6), 729–62.
Verran, H. 2010. "Number as Inventive Frontier in Knowing and Working Australia's Water Resources." Anthropological Theory 10 (1–2), 171–78.

4 Enacting Cyclones

The Mixed Response to Climate Change in the Cook Islands

Cecilie Rubow

In February and March 2005 the Cook Islands experienced a swirl of five strong cyclones having a devastating impact through high winds, storm surge, and damaging waves. Subsequently, on many occasions, residents and outside observers have linked these incidents of extreme weather with climate change. Since then it has been 'all over', an officer at the National Environment Service explained five years later. At this point, numerous NGOs working in the area had taken up climate change as a priority area, and the National Environment Service and external consultants had produced a long series of vulnerability assessments on the main island, Rarotonga, and on several of the 14 outer islands, among them several low-lying atolls, concluding that climate change is observable in a number of ways. The local newspapers had started to report from workshops, sites, and projects related to climate change, and many people on the islands had increasingly become aware of global warming as a threat to the islands and their inhabitants. In a turn, the cyclones made climate change present.

Starting from the 2005 cyclones, this chapter is taking the question of the "anticipation of nature" in three connected directions: after a short introduction to the evolving and revolving concern about climate change in the Cook Islands, first, I will show how scientific climate scenarios have been received, employed, and modified in various policy settings in the Cook Islands. Making a swift move into the wider, shifting horizons of anticipation, secondly, I will explore how cyclones and other instances of bad weather have become intertwined with tourism, traditional leadership, and emerging Christian eco-theologies. En route, in a looping way, using the cyclones as a figure for "athwart theorizing" (see Helmreich 2009), I advocate for taking the whirl, the rotating quality of the cyclone—and the horizon as a demarcation between what is within actual and potential reach (Schutz 1970: 245)—as topological ways (Mol & Law 2002: 8) to think about the mixed social-natural life of cyclones. As it turns out, the links between the 2005 cyclones (and future cyclones) and climate change grow strong, but the evolving cyclone exegesis is also seriously contested in both scientific and public domains.

As a reception study (Rudiak-Gould 2011) the aim of the chapter is to show how various groups of people receive and understand "climate change" through the cyclones. The unstable, furious winds move in different, even opposing directions and although they leave a visible trace of destruction and environmental change behind them, they are also directing multiple opportunities for the islanders' search for new fixed points in the horizon, new ways of relating to the future of the islands and the island communities. In science, policy, and everyday observations and conversations, the link between climate change and more intense cyclones is unsettled and unstable, creating a void of certainty that nevertheless makes room for diverse and conflicting forms of manoeuvring. Thus, the reception of the scientific models of climate change is situated in a turbulent environment moulding the conceptions of weather patterns in new, differentiated ways.

"CLIMATE CHANGE IS FOR REAL"

In 2000 *The Initial Cook Islands National Communication to UNCCC* states that "the Cook Islands historically has not been subject to extremes of temperature and rainfall, and interannual variations in sea level rise" and that it is "uncertain as to how these local fluctuations will be affected from the enhanced greenhouse effect" (PICCAP 2000: 28). Looking into the future on the basis of the IPCC scenarios[1] and very "limited scientific capabilities" the report assesses that increases in temperature and sea level rise will worsen the damages to the already vulnerable and sensitive coastal zones. With a few exceptions, the overall line of reasoning in the report is that climate change is a prospective phenomenon. Although observable coastal erosion and a decrease in the vitality of the reefs systems, according to the report, "indeed" could be linked with climate change, it is estimated that it is more likely to be linked with natural dynamics and human activities such as overfishing, uncontrolled pollution, removal of coastal vegetation, the blasting of reef channels, and poorly constructed sea walls (ibid.: 29–30).

Five years later the situation is changing. In February and March 2005, the Cook Islands experienced the exceptional row of five cyclones. In the wake of it, these incidents of extreme weather were linked to global warming as providing the physical basis for more and stronger cyclones. In the *Second Communication to the United Nations* on climate change, prepared in 2010/11 with more than 20 participating governmental and non-governmental stakeholders, the conclusion is taking quite another route. Here 10 years after the "first communication", it states that "the Cook Islands face severe climate impacts and higher vulnerability due to increased climate change and global warming" (Cook Islands Government 2011: 89). The report now identifies at least 24 areas of climate change vulnerabilities, including coral bleaching, coastal erosion, increased storm surge, damage to infrastructure, flooding and sedimentation, and acidification of the sea. Furthermore, the report points to many gaps and needs in terms of information and capacity building. Climate change is now conceived as an active driving force, already causing a wide fan of connected, environmental problems.

The marked difference between the two reports, the one primarily prospective and deductive on the basis of IPCC scenarios, and the second anchored in local assessments, seems to suggest that climate change in this setting began as global warming before turning into local environmental change. This is not only a remarkable shift in attention from global warming as a general and quite distant process—not least because Pacific Islanders' part in the burning of fossils has been insignificant—to climate change as an observable feature in the landscape and in the weather patterns. However, when climate change is coming to the shore from the more distant horizon it does not necessarily gain in evidence and clarity. On the contrary, a notable process of differentiation seems to have taken off. Just as a cyclones' impact is diversified—some trees fall, some roofs are secured—so is the reception of climate change.

A short look into the "National Environment Forum", a workshop over three days held in Rarotonga in July 2010 with more than 100 participants coming and going may illustrate this process of differentiation. Many of the stakeholders of the *Second Communication* were summoned to a workshop in order to prepare a new *Strategic Action Framework for the Cook Islands*. Climate change had its own section among others such as waste management, biodiversity, and land use, but it was also a recurrent theme throughout the workshop. To an audience of approximately 50 people, an officer from the National Environment Service introduced climate change as a long-term problem with short-term effects already witnessed locally and in the region. The officer listed some of the local impacts of climate change stressing coral bleaching and an increased frequency and intensity in rainfall and tropical storms (cyclones and storms of various strength). To underline the seriousness—and as a wakeup call after lunch, she said—she wore a life buoy with the inscription "Climate Change Kills. Act now. Save lives", apparently stemming from "tcktcktck", an NGO visible in the UN COP process in which the officer had participated several times. Supported by a second speaker, she strongly called on better coordination and implementation of actions, rising awareness, partnership, and education.

"Climate change is for real" was one of sentences encircling the Forum: yet this claim about reality was clearly not without uncertainties, open ends, or outspoken disputes. As humpback whales, birds, landfills, tourism, and rainfall patterns entered the discussion, it wheeled in several directions, temporarily dwelling on specific geographical locations, feedback mechanisms, or digging into legal issues. This turbulent, reverberating process of differentiation was noticeable at several occasions, not least when discussion groups were organised around selected core themes. Starting from quite well-defined points of departure the groups set out on more or less organized and coordinated tracks. In some cases long-standing issues were revisited, e.g. waste management, in other cases imminent questions of funding and organization were addressed. Sometimes climate change seemed to be an overall driver intensifying all environmental problems; sometimes its reality was disputed or neglected to the benefit of a focus on what residents on the islands might do themselves to alleviate the present and future pressure on the environment.

At one point a group was asked to discuss "what are the options (internal relocation?) we need to look at if we cannot adapt?". The participants were asked to imagine a future point (not specified) where adaptation measures were not any longer possible. Opening up the horizon anew, the future actions suggested in this group, backed up by other break-out groups (working with comparable questions), were the following:

> *rally small islands to fight together*
> *push bigger countries (the global warming is not our fault)*
> *educate political and traditional leaders*
> *work out disaster management plans*
> *plan for the worst, hope for the best*
> *go to church on Sundays*
> *appoint a climate change division*
> *map hazard areas*
> *raise awareness, educate, educate, educate*
> *wait and see*
> *charge use of air space*
> *build an ark*
> *buy a cruise ship*
> *harvest mangese nodules to help funding, and so that no one else gain from it*
> *secure drinking water and food*
> *improve communication between the islands*
> *relocate coastal communities from outer islands to Rarotonga, from the coast to inland, and from Raro overseas*

At this point of analysis, I suggest to take these suggestions as what Annemarie Mol and John Law understand as a "multiplicity", or as "coexistencies at a single moment". Obviously, many of the suggestions will not appear in the final report leading to the next five-year strategic plan for the environment in the Cook Islands. However, the open horizontal view, prompting many and varied suggestions, seemed well aligned with other responses during the workshop and among Cook Islanders outside the workshops' circle of NGOs and government offices. In the streets, at the beaches, in the shops, and in homes of Rarotongans, "wait and see" would probably be the favourite evaluation of the signs at the shore and in the horizon, quickly accompanied by selected suggestions born out of both a range of imminent concerns, general curiosity, and some unwariness about what "climate change" actually embraces. I simply propose "mixed" to label this multi-directional response, an imaginative whirlpool of suggestions, searching for the next fixed points in the horizon.

To make sense of such multiplicities Mol and Law (2002) advocate for more topological ways of thinking and writing than the vocabulary of "worlds", "cultures", "models", "discourses", or other "wholes" call on. This perspective suggests that complex socio-natural phenomena offer an anthropological analysis more than a description of, say, differences and likenesses between a scientific "worldview" and "public opinions and common sense", or between different cultural models of climate change. This move away from meaning systems and patterns of significance entails the discovering of methods for "laying out spaces, and for defining paths to walk through these" (ibid.: 8), and for identifying "the various coordination strategies involved . . . in reassembling multiple versions of reality" (ibid.: 10). In this chapter, I will suggest that one way of realizing this ambition is to tune into the practical reorientation of spatial transcendences, as outlined in phenomenological terms by Alfred Schutz: "In space, the world within my actual reach carries along the open infinite horizons of my potential reach, but to my experience of these horizons belongs the conviction that each world within potential reach, once transformed into actual reach, will again be surrounded by new horizons, and so on" (Schutz 1970: 245). A horizon is as such more than a sharp line separating a landscape and a blue sky, or the blurred space between the spraying sea and the dark thunderstorm. It is also a term denoting the wider experiential, and changing, sense of what is within actual or potential reach, and what is beyond outside any "manipulatory sphere" (ibid.). "Wait and see" is within reach, "relocation" is appearing in the horizon in many Pacific locations as a potential strategy, whereas, judging from the laughing, "building an ark" seems to instantiate a new horizon.

Taking the list of the mixed response as a valuable finding in itself, the next step is to follow how people, texts, figures, and situations in practice assemble different versions of climate change. The turbulent cyclones that triggered the reality of climate change is at the centre of my attention in order to show how facts, hopes, and landscapes are moving between what is within actual reach and what is beyond. The whirling quality is expressed in the force of "climate change" as an explanatory tool, the sense of the extraordinary, and the way in which everything on the ground potentially seems to be drawn into the storm and left in some confusion.

LINKING CYCLONES AND CLIMATE CHANGE

> Between 4 February and 8 March 2005, the Cook Islands experienced five damaging cyclones, four of which were assigned a severity

> rating of Category 5 and caused damage to homes and essential public infrastructure. The Government and its agencies provided early warning information dissemination, evacuation and emergency relief to the affected population with the support of international and regional relief agencies. Following the cyclones, the Government assessed the physical damage but it lacked all of the capacity and resources to finance the immediate recovery and reinstatement of basic services. (SPREB 2009: 25)

Tropical cyclones are extraordinary weather events linking people and islands in many turbulent ways. As a furious, condensed whirling wind system battering the land, rapidly putting the ocean into extreme motion, cyclones have a remarkable impact on the islands in the South Pacific (Terry 2007). In this region, many cyclones evolve, grow strong, and fade out, often leaving a highly visible track of destruction at the shores, on land, and through people's experiences. But cyclones are not simply arriving and leaving as if they (only) were something in themselves. Landscapes and people interact with the waves and the wind in numerous ways. Some coastal zones are damaged and some beaches are eroded; in other cases new land is created because large amounts of sediment are removed from the reef and lagoon to the foreshore (ibid.). Cyclone warnings are issued, and boats, cars, books, and people (and much more) secured when the track of a cyclone seems to approach an island. In some cases the arrival of cyclones is predicted by the curling top of young banana leaves or by meteorological boards; the storms are feared for and expected with both prayers and moorings. Cyclones are in other words becoming real in many distinct ways—or in Mol's terms—*enacted* in different, more or less coordinated *repertoires*, i.e. ways of practicing people and objects (Mol 2002). Some repertoires prominent in the media and in disaster management reports are centred on specific instances such as Cyclone Sally in 1986, the cyclones Meena, Nancy, Olaf, Percy, and Rae in 2005 or Cyclone Pat in 2010. Others deal with a certain flying roof, the overturn of a particular coconut tree, or the tricky laughter heard in the streets after a particularly nasty storm.

What follows here are guided by a tracking of repertoires establishing both strong links and less tangible interferences and frictions between cyclones and climate change. In this region cyclones are important *and* unstable weather phenomena. In the Cook Islands they may turn up in the wet season (or out of season), during El Niño events (or not), they may reach category 5 strength, and a cyclone may hit an island one time—or once again after the silent eye has passed. A strong cyclone may leave crops, houses, and people intact and safe, or as Cyclone Martin in 1997, cause death and destruction of infrastructure and result in a long period of recovery for whole communities.

THE DAY THE ISLAND WAS ALMOST WIPED OFF THE MAP

> On the afternoon of 1 November, 1997—the first day of the hurricane season—cyclone Martin smashed huge waves through the villages and lagoon of Manihiki. 19 people died and 400 were evacuated to Rarotonga by the Royal New Zealand Air Force.
>
> One report described the terrifying day: "Housing was flattened, public facilities destroyed, crushed coral roads washed-out, and virtually all of the off-shore accommodation and equipment relating to the lagoon pearl-fishing industry was destroyed. Small boats, timbers and household contents were strewn everywhere, and sheets of cast-iron roofing were wrapped like tape around high trees. Sunken debris littered the edge of the lagoon to a distance of about 30 metres. (http://www.cookislands.org.uk/manihiki.html)

If it was difficult or simply did not seem relevant for Cook Islanders to link up with climate change before 2005, even though regional and international forums for years had identified Pacific Islands as vulnerable "sinking islands", it appears as if the five cyclones infused a sense of excess, fitting well with both the scale of climate and the idea of major, global changes. Cyclones are weather events every islander knows of from first-hand experience, but five in a row within a few weeks was apprehended as a new state of affairs. Quickly, the questions were raised: Is this what we are going to expect in the future? Is this climate change—and have we, as such, already entered the future?

Since 2005 environmental consultants in cooperation with national, regional, and international institutions (e.g. SPREB, UNDP-GEF, Asian Development Bank, and FAO) have produced a long series of reports, and a new national disaster management plan for the Cook Islands has been worked out. As the quotes below demonstrate, the reports are all making links between the five cyclones and anticipated changes in the intensity and/or frequency of cyclones and climate change as a generic phenomenon. It is, however, in slightly different ways, so I will quote a few variations (my emphases):

> The world is warming. The effect of a warmer world on low lying island countries, such as the Cook Islands, *can be significant*. Climate change conditions *are envisaged already* as those impacts from extreme events such as tropical cyclones . . .
>
> *Climate scenarios* based upon Global Circulation Models *outline* that in this region of the Pacific *extreme events will increase* along with significant impacts upon the people and their environment . . . Cyclones *may become more intense* in the future, with wind speeds increasing . . . A climate *change scenario points* toward . . . at least a

> 20% increase in cyclonic activity both within and outside of current cyclone seasons . . .
>
> With regard to awareness, the issue of the vulnerability to cyclones and other disasters *is widely apparent*, both from recent memory and from the existence of some buildings that remain unrepaired after the 2005 cyclones. (Asian Development Bank 2006)

> The Cook Islands *has already experienced* first-hand the adverse impacts of climate change and extreme events. In 2005, the islands were hit by five tropical cyclones within the space of one month, an event that has never been experienced in the history of the Cook Islands. In 2005, the island of Pukapuka was completely inundated by wave surges associated with cyclone and strong winds causing the loss of agriculture land which took three long years to recover . . .
>
> People and infrastructure located close to the *coast are already threatened* by rising sea levels and storm surges. *Coastal erosion is evident almost everywhere* on the island of Rarotonga thus threatening the country's tourism industry, which is heavily based on the beaches and sea. (FAO 2008)

In this set of policy reports the 2005 cyclones are linked to climate change by varying ways of coordination, establishing links not only between cyclones and climate change, but also between the intensity and frequency of cyclones, the past and the future, between acute experiences of really bad weather and scientific reports—and between a nation and overseas agencies. The cyclones are prominent actors in a subtle weaving between what are actually known relations between causes and effects and between what has potentially happened and is going to happen. The links between cyclones and climate change are gaining some strength in the policy papers, but as I shall show, they are not entirely stable.

Although sea level rise, coastal erosion, and cyclones in official reports and in public discussions are frequently linked to climate change, it is important to note that the perceived coastal erosion is also tightly linked with existing malfunctioning coastal protection practices. This aspect is explicitly highlighted by the FAO report stating that "the ongoing conversion of coastal lands for settlement and hotel development . . . is therefore contributing to their own unfortunate demise as climate change related events will threaten the coastal areas and lands on which they are located" (FAO 2008). Along the same line, residents and local experts often mention a series of local drivers of beach erosion such as the construction of houses close to the beach, the removal of scrubs and sand at the upper beach level (to make a nicer and whiter beach), failed protection devices, and the high level of nutrients in the lagoon (stemming from insufficient sewers reducing the sand production of the coral reef). Cyclones and related weather patterns are not easily isolated phenomena when they transverse the horizon. This is what local residents sometimes ponder over when they lament the erosion: is this climate change—or is it our own work—or both, or is it primarily caused by natural variations in currents, temperatures, and wind directions? What will happen in the next few years, and in the lifetime of our children, and their children? This is also what residents disagree about. On some occasions the different repertoires rotate around their own circles and enter a state of mutual exclusion, on other occasions different drivers readily blend in e.g. negative feedback mechanisms.

Just as this multiplicity of relations is recurrent in everyday discussions as outbursts of disagreement or as more discrete instances of conjecture, it also inhabits strict geoscientific discourse, just as the news media quickly spot emerging controversies. Although many published studies on coastal erosion as such seek to demonstrate feedback mechanisms between human-induced global warming and local changes in coastal areas (cf. Nunn 2009, 2010), others set out on a more pronounced polemical track. Controversies on the question whether Pacific Islands are sinking or not (Barnett 2011; Webb & Kench 2010) seem to add to the elusiveness of the islands as places (cf. Hastrup, Chapter 1, this volume). Still, living with a rising sea level, anthropogenic influences on shorelines, and acidification of the sea—and more persistent cyclones—*may* add up to a pernicious maelstrom. As I will argue below, cyclones are traversing and changing other topologies as well, adding to their unstable character.

TOURIST TRACKS, TRADITIONAL KNOWLEDGE AND CHRISTIAN CONCERNS

Whirling / Raunuka / The waters were heaving, rushing!
Long dark clouds / Billowing storm clouds / Enveloped the world
The North wind / The East wind / The West wind / The South wind
Blow mighty winds, Blow! Bending over! / Bending over!
Rumbling winds, trembling winds / To shake the new world
All were shaken because of Ruanuku's anger.
(Turua 2003: 75)

There are other ways of enacting the cyclones and the debris they leave on the shores. The whirling, rumbling, and trembling winds have left a trail in the historical files in which observers again and again recount the damages of the winds and water and the feeling of experiencing something extraordinate. Reaching category 3 at the Saffir-Simpson scale, cyclones with maximum wind speeds around 200 kilometres per hour are per definition expected to blow large trees down and to cause extensive damage to small buildings and serious coastal flooding. Escalating to category 4 and 5, the expected effects are characterized by extreme structural damage, major damage to lower floors of structures, major erosion of beaches, devastating damage to roofs of buildings, and the overturning of small buildings (PDC n.d.). On the ground residents report about winds coming with a “crashing, throbbing rush . . . rattling the trees violently”, “striking the house with a sickening violence”, making sheets of iron “hazardous missiles”, and villages strewn with coral rocks of all sizes (Tom Davis; quoted in de Scally et al. 2006: 255).

No wonder that Raunuka, the god of the winds and the storm, is the most furious child of Papa and Tumu according to *Kia Pu’era*, an oral tradition of Rarotongan origins (Turua 2003). Obviously, the storms are shaking experiences, not only in terms of economic loss, and not only because precious crops are rotting, and roofs are curling away. It is also the experience of nature’s excess. Large, firmly grounded trees are falling over, waves are building up in giant sizes, an excess after which people not necessarily only lament the damages, secure news about family and friends, and organize the clean-up, but also start laughing and giggling when inspecting the “unbelievable” mess (de Scally et al. 2006: 255). Truly unbelievable, but also a fact: the storm, the noise, and the mess reach into the intangible, beyond the horizon of the everyday, where roofs are roofs, and the sea stops at the shore, thus opening a path into the potential and releasing both a sense of absurdity, joy, and, as I will show, divinity. The horizon is refigured and confirmed in a number of ways, and surely, the list could be long and never reach a conclusion. Past cyclones and the future ones are continuously in the making of concerned residents and scholars and others trying to build up a new sense of security about what to expect. Here, I concentrate on three different domains, each having distinct repertoires of anticipation.

Tourism. For ages, the South Pacific has been represented as a sunny, tranquil paradise with sparkling blue lagoons. Currently, around 100,000 tourists visit the Cooks every year, a result of a determined national strategy, generating an average of 80% of the gross domestic product in recent years, and sparked by the building of a new runway in 1974. Resorts have been constructed along the coasts, offering excellent swimming and diving facilities, whale watch, “Island Nights” with local dishes, drums and dance, and safaris into the cloud forest.

The tourist websites’ and travel books’ treatment of cyclones is well aligned to the enclosed and protected tropical domain of the resorts. Again, two quotes may illustrate the variation (my emphases):

> The warmer season is from November to March, when temperatures rise to 22-28°C. Although this is the rainy hurricane season, mornings are usually sunny, *with storm clouds building up in the afternoon and the rain falling in a late afternoon storm that leaves the air refreshed*. The Islands are out of the usual cyclone path, and major cyclones usually only happen *once every twenty years*. (South Travels Holidays & Tourism Co., online)

> The wet season is from December through to March, when around 25cm of rain can fall each month; this period is also the most likely time for *cyclones, which are becoming more frequent* due to warming seas across the South Pacific. On average, a *mild cyclone* will pass by two or three times a decade, while *severe cyclones* generally only hit the islands once every 20 years or so—but in 2005, an unprecedented five cyclones tore through the islands in five weeks, suggesting *that cyclones may be much more difficult to predict* in the future as climate change hits home. (Lonely Planet, online)

Here, in the first version, tourists seem to be on safe ground, when "storms" connote the recurring refreshment of the air in the late afternoon, and in the second version, relatively safe, when cyclones are rare exceptions hovering in the outskirts of what one appears likely to witness on a holiday.

A few days after Cyclone Pat hit Aitutaki on 10 February 2010 damaging more than 300 houses and making more than 80 islanders homeless, the tourism council quickly issued a "keep on visiting plea" stating: "The people of Aitutaki are resilient and already have made good progress to returning their lives to normal but it will take much work and importantly, it will need the support of our visitors from around the world" (Aitutaki Tourism Council, 14 February 2010). The local news reported about exceptionally helpful travellers assisting islanders in cleaning up, and about fine beaches and surprisingly neat and well-functioning resorts. This enactment of cyclones is part of the elusiveness of the islands and the unstable character of the cyclones. On average in a season, less than two cyclones visit the waters of Cook Islands, in total covering a vast area comparable to India (de Scally 2008: 447) and the home of less than 20,000 islanders. The winds are periodically shaking the ground, but presently tourism is leaving an even more visible trail of change in the coastal zones. Pushing the cyclonic threat towards the horizon, the reasonable assessment for the average tourist offered is to board the flight and enjoy a tropical holiday.

Traditional knowledge. The geoscientific and meteorological knowledge has managed to condense a myriad of facts into the concept of climate change, but it will not necessarily outdo other knowledge practices. Quite the opposite, it seems to attract almost everything into its vortex. In the Cook Islands and in the neighbouring islands states, traditional ways of predicting and coping with cyclones and other weather events are quite often popping up in public (Finucane 2009) and scientific publications (e.g. Mercer et al. 2007; Campbell 2009; Lefale 2010) and in conversations about cyclones and/or climate change. Employing the concepts of resilience and vulnerability, Campbell (2009) finds that many traditional "disaster reduction measurement practices" have been lost throughout the Pacific Islands, but also that these adaptive practices (food security, cooperation, and settlement patterns) indicate that the Pacific Islands should not be considered as "essentially or inherently vulnerable" (ibid.: 85). Taking another track, Lafale (2010) compares Samoan weather prediction measures with Western measurements—cockroaches are comparable to barometers—and he therefore argues that "traditional ecological knowledge in weather and climate" can play a major role in the advancement of "our western understanding of weather and climate" (ibid: 331). So, whereas scientific disagreements over "sinking islands" may rise to controversy, other scientific practices may clear rapid pathways into a grand union of Western and traditional ecologies.

In the Cook Islands, the old ways of predicting a cyclone are often and explicitly valued as grandparents' reliable wisdom, however, on other occasions it is stressed that climate change makes any effort of predictions more difficult. It is adding to the complexity that for some people traditional knowledge is simply legends, nice stories. Nevertheless, "traditional ecological knowledge" does turn up and does gain importance in certain contexts, not least because the traditional leaders are often holding many valued (and contested) positions in politics, in the churches, and in the families. Certainly, past horizons turn up, when new are appearing.

In the Cook Islands, repertoires of *traditional knowledge* are also entering the local environmental circles. At the Environment Forum, traditional leaders took part in the discussions sometimes as grey eminences, sometimes finding a platform from which the virtues of the old ways were translatable into the environmental concerns:

> *Climate prediction . . . Abundant bearing of mango fruit early in the season is a sign of a stormy summer. When the new leaf of the banana tree bends at right angles, that is a prediction of cyclones. If the new shoots of the banana tree curl around themselves, it is a sure sign of cyclones. When frigate birds circle the island, a cyclone is on its way . . . Traditional leaders will use both traditional ecological knowledge as well as modern technology to counter the effects of climate change.* (Imogen P. Ingram, traditional leader, Cook Islands n.d.)

Inhabiting the islands with histories of Maori tradition, colonial powers and Christian conversion ties most islanders up with a plural past. Even if old ways of prediction in many instances have withered away, some are exactly reawakened, when they seem to be surpassed with physical explanations of the cyclogenesis. Repertories of traditional knowledge may in turn whirl certain moral, spiritual, and political aspects into the cyclones. Arguments about the comparable sustainability of the traditional society may clear the path for getting the practicalities of the old days within actual reach again. Before the missionaries arrived and changed the patterns of habitation, the islanders lived in the higher interiors of the island and the suggestion is sometimes made that a retreat into the valleys could be a sustainable adaptive strategy.

Christian concerns. In the opening prayer at the Environmental Forum, a pastor addressed the seriousness of the wide range of environmental problems facing the Cook Islanders. He continued by stating that the answer is in the Bible, highlighting three guiding aspects in man's relation to nature: ownership, stewardship, and accountability: "The world is God's, and we are the stewards with an obligation to act with accountability towards men and nature". The pastor continued by lamenting that "we have moved away from Christian values . . . People say: I do not need God". Thus, he added, our "hearts are evil", and concluded: "I believe management is a question of life or death. Jesus, the spirit of wisdom is going to manage our hearts; he is the very owner of the sense of realization of how we manage the environment".

When explicitly asked about any possible connections between Christianity and climate change, ministers in the Cook Islands Christian Church, the mainstream church numbering about half of the population, are usually more reluctant. Yet, climate change is seeping into their work and communities in many ways and the pastors are apt at developing their theologies drawing on every domain of knowledge within their horizon. The repertoire may consist of the Bible, the latest IPCC report, the last cyclone, conversations with flowers, and a grandmother's early warning that the seasons seem to be changing. The search for what is actual and what is potential is not limited to strict genre, and the pastors, just as the traditional leaders and the tourist agencies, are expedient providers of pathways between many repertories. Ministers are always asked to say an opening and closing prayer at official meetings and workshops, and because there are many meetings, many ministers are taking part, also from the many, very active smaller Christian congregations. Questions of guilt and responsibility are quickly entering the cyclone discussion, ranging from conservative repertories stressing the hazards as divine intervention and the opportunity to repent one's sins to less redemptive oriented theologies with a focus on the moral, God-given obligation to care for the environment (Taylor 1999; Rubow 2009). In the wider region of the Pacific Islands climate change and the threat of cyclones are also incorporated in new eco-theologies, building on a longer tradition of environmental theology exploring traditionally founded and new theologies of the land and the sea (Tofaeono 2000; Halapua 2008). Taking climate change as a physical and metaphysical concern, new eco-theological versions also take the opportunity to link the change of climate with a possible reform of the churches themselves, turning away from an "interiorized" and "redemptive-oriented" Church "cut off from the life of nature or from ecology", to a church in which "the ecological crisis" is pertinent and inspiring (Bird 2009).

Although scientists, politicians, and NGOs for various reasons may express reluctance with regard to cooperation and coordination with both traditional leaders and church leaders and theologians, it is openly admitted as a *sine qua non*, because the churches and the traditional leaders are important institutions locally and nationally. When P. Nunn (2009) summarizes that international and national policy-making concerning climatic changes has not had any effects the last 20 years in the Pacific region, "and that the churches typically tend to emphasize the power of piety and prayer rather than other solutions" (ibid: 220), he still advocates that community leaders should be empowered (chiefs, church leaders, elected leaders) in the process of adaptation to climate change.

MORE WHIRLING, MORE TURBULENCE, ABOUT PRESENT AND FUTURE CYCLONES

In 2006 the World Meteorological Organization released a summary statement numbering a series of background circumstances, among them that with regard to cyclones there is no clear evidence of long-term trends and that the intensity of cyclones "will remain the same or undergo a modest increase of up to 10-20%. These predicted changes are small compared with the observed natural variations and fall within the uncertainty range in current studies" (2006:1). Furthermore, it points out that "the rapid increase of economic damage and disruption by tropical cyclones has been caused, to a large extent, by increasing coastal populations, by increasing insured values in coastal areas and, perhaps, a rising sensitivity of modern societies to disruptions of infrastructure" (ibid). More precisely, getting closer to the 2005 cyclones in the Cook Islands, the statement notices that a combination of a) high-impact tropical cyclone events occurred throughout the globe in 2004 and 2005, b) the IPCC TAR's linkage between global warming with increases in greenhouse gas, and c) two scientific papers appearing during 2005 in *Nature* and *Science* providing "evidence for an increase in the number of intense cyclones . . . has led to statements in the world press that the recent hurricane disasters can be directly attributed to the impact of global warming" (ibid: 2). On this background the panel of experts, including the two authors of the scientific papers mentioned, make a number of statements, among these:

a) "Current knowledge and available techniques are not able to provide robust quantitative indications of potential changes in tropical cyclone frequency."
b) "No single high impact tropical cyclone event of 2004 and 2005 can be directly attributed to global warming, though there may be an impact on the group as a whole."
c) "There is no consensus among current climate models regarding how ENSO variability may change in the future,[2] although any such changes in ENSO would be expected to alter Tropical Cyclones [sic] regionally." (WMO 2006: 2)

The obvious intention behind the statement of the World Meteorological Organization is to weaken or even deny strong links between the series of cyclones in 2005 and climate change in terms of clear scientific evidence, yet at the same time the call is also reflecting the potentially fruitful advancement of establishing links between global warming and cyclonic events. Although meteorologically robust knowledge about the links between global warming and the intensity and frequency of cyclones is not within the horizon, the potentiality is still in place. The reasoning by WMO is as such opposed to the reasoning in the vulnerability assessments and policy papers and among many NGOs in the Cook Islands. Concerning the possible link between changes in ENSO, global warming, and more frequent and intense cyclones, the estimation of Gerald McCormack, a local expert in biodiversity working for the government for decades, is that this is a reasonable hypothesis. Importantly though, he is also calling for the possible action it entails: "Every government and every individual should take immediate action to reduce their contribution to Global Warming" (McCormack 2005).

As different repertoires, the WMO statement and repertoires establishing much stronger links are clearly at odds, but presently a process of coordination is also developing and in yet another repertoire they may find a mutual ground through enactments of coordination. The link between climate change and cyclones is certainly still a theory, and although an increase in intensity up to 10–20% may be termed "modest", it is still an increase, which could be fatal for small islands, just as any cyclone. The uncertainty is still prone to shape both scholars', consultants', and local residents' awareness of past and present environmental change as intertwined with far-reaching global changes.

A recent historical study of the cyclone activity and the cyclones' individual impact in the Cook Islands, published in 2008 by Fez de Scally, displays the frictions of uncertainty surrounding the unstable character of the cyclones in yet a different way. On the one hand the study finds that the observed cyclone activity has more than doubled (from an average of 0.8 cyclones per season between 1820 and 2006 to 1.8 cyclones per season from 1970). On the other hand de Scally suggests that this increase is "probably attributable to satellite monitoring" which began in 1970 (2008: 433). A table shows how the number of cyclones clearly has increased, in fact more than redoubled; yet the sound conclusion is that this is probably not the case. The counting of cyclones is of course not a straightforward mission. Working meticulously with the set of data consisting of many scales of measurement, this was obviously what it said: more cyclones could be counted after 1970; however, it had most likely more to do with the counting method than with the cyclones.

Characteristically, the most cautious projections fusing a wide spectrum of scientific data bear the same quality of uncertainty. Commenting on the WMO statement, James P. Terry concludes that for the South Pacific region "it seems likely that there will be some evolution in tropical cyclone activity", where "some or all of the following effects may be experienced": changes to the pattern of origin, little change in numbers and frequency, increased intensities, enhanced precipitation, longer cyclone lifespans, track directions more southerly, and extended track length (2007: 84–85). Still, there is room for change, with cyclones hovering between the intangible and the concrete.

Getting closer to some concluding remarks, a short episode may illustrate the line of argument I am putting forward. Trying to keep track of the different enactments of the cyclones one day during fieldwork I had a brief chat with a meteorologist in Avarua, Rarotonga. He kindly lent me the above-mentioned study of de Scally, and he expressed that he was always careful not to make any conclusive statements about a changing climate and possible linkages to cyclones. As far as he could say, it was much too early to make such claims, and therefore, he noted, he always met requests from the government advisors, the media, and NGOs (and anthropologists) with the newest available, concise, meteorological data on sea level rise, wind patterns, and so forth. A few days earlier, I had noticed a quote by the meteorologist in a booklet from Cook Islands Red Cross establishing, I thought, close ties between climate change, cyclones, and the erosion of the coasts in the Cook Islands. In the first superficial reading, I had not remarked any frictions between the meteorologist's statements and the conclusions drawn, and in the next, more careful reading it was actually still hard (but actually possible, knowing what to look after) to isolate the meteorological data and the authors' links going in another direction than the meteorologist seemingly approved of. Such frictions, deeply enmeshed in commas, conjunctions, and the use of pictures, graphs, and tables, reflect some of the complexity in everyday ways of anticipation. Scientific facts, personal observations, the NGO's general guidelines for raising climate change awareness, and many more tangible and intangible entities are fused, blended, and gently coordinated in suggestive, whirling links.

SHIFTING HORIZONS

Anticipation takes time. As an expectant waiting for new, emerging horizons and for the remembering and widening of past traditions, the shifting horizons reveal, I have argued, a both concrete and metaphysical engagement with the world. Some repertories are aligned by strict methodology and some are shaped as long lists of miscellaneous hopes, fears, and plans: "Make a disaster management plan, go to church". In the Cook Islands, the past 10 years have been a period of time in which the group of people working with environmental issues—and many in the wider community—have come to take climate change as an upcoming point of reference. Taking the cyclones in 2005 as a point of reference, I have aimed at an account of how different enactments of past, present, and future cyclones are whirling into each other in a highly turbulent and changing environment. Rather than ignoring the turbulence or relegating it to the periphery of an account of how Cook Islanders and outside observers respond to climate change as a scientific and political discourse, the aspiration has been to further a portrayal in which the unruly nature of shifting horizons is played out in the midst of a changing social-natural environment.

Anticipation takes place, too. Digging into a few situations, episodes, and texts, this chapter does not, however, escape a social scientific aim at seeking tangible patterns and more prevalent connections between certain repertories rather than others. The linkage between the phenomenology of Schutz and the praxiography by Mol and Law has provided an analytical strategy in which both the very local practices, each in their here and now, and the simultaneous awareness of potentialities are incorporated into the anthropological account. As such it may become comprehensible how scientific scenarios may feed into aspirations about a Pacific Christian reformation, and how a retreating shore and a fallen coconut tree may give rise to deep anxieties about global warming. The potentialities of links are naturally endless, but the actual repertories and the connections between them may end up in more stable and competing formative patterns. In the Cook Islands—and in the wider region—in 2010 it was still a very turbulent field with the role of the cyclones unsettled. Already, in 2011, the future of the cyclone activity took yet another turn. A new scenario issued by the Australian Bureau of Meteorology for the Cook Islands concludes with “moderate confidence” that “tropical cyclone numbers are projected to decline . . . over the course of the 21th Century” (2011: 37). Whereas this caused some relief among governmental officers and NGOs, remembering the 2005 cyclones, another projection reinstalled the uncertainty: “Despite this projected reduction in total tropical cyclone numbers, five of six . . . simulations show an increase in the proportion of the most severe cyclones” (ibid).

In the field, the inhabitants, and the observing guests, are constantly side-tracked into new enactments and links between repertoires. Although the observers’ work at the desk encourages a more systematic perspective, the suggestion in this chapter is that it is fruitful to remember and render the plethora of side-tracks and the shifting horizons, which seem to characterize the way climate change is taking place in many so-called vulnerable and seemingly not-so-vulnerable places throughout the world. The instability and fragility inherent in the enactments of the cyclones amidst the many efforts towards establishing secure knowledge are, I would suggest, also a sign of creativity in this small island nation, and beyond. The unstable character of both the cyclones and the wider field of climate change is as such challenging the creativity of both the social and natural sciences.

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NOTES

1. According to the IPCC, small islands have characteristics that make them "especially vulnerable to the effects of climate change, sea-level rise and extreme events" (Minura et al. 2007: 689). In the Pacific area the risks enumerated are: 1) sea level rise resulting in inundation, storm surge, and costal erosion, 2) reduction of fresh water resources, 3) impact on coral reefs and fisheries, agriculture and biodiversity, and 4) negative influence on tourism (ibid.: 689). Besides, expected hazards throughout the Pacific are an increasing occurrence of heavy rainfalls and instances of hot days, drought, and cyclones (ibid.: 691–92).
2. ENSO (El Niño and Southern Oscillation) is a term covering a periodical weather pattern in the South Pacific. Basically it consists of a "current of hot water", and when it "flows eastward along the Equator it moves the likelihood of the winds developing into cyclones . . . The extreme is during a severe El Niño when a belt of 29-30°C water reaches across the Pacific. During these times we usually see the number of cyclones in the Cook doubling and tripling" (McCormack 2005).

REFERENCES

Aitutaki Tourism Council 2010. "Cyclone Pat: What It Means for Visitors." Issued 14 February. www.cookislands.org.uk/cyclonepatadvice.html, accessed May 2011.

Asian Development Bank 2006. Strengthening Disaster Management and Mitigation (Component 2: Preventive Infrastructure Master Plan), vol. 1, Master Plan. Final report, TA 4605-COO.

Barnett, J. 2011. "Forget That Sinking Feeling—There's Nothing Inevitable about Climate Change Refugees." The Conversation. 18 April.

Australian Bureau of Meteorology and Commonwealth Scientific and Industrial Research Organisation (CSIRO) 2011. Climate Change in the Pacific: Scientific Assessment and New Research. Country Report. Cook Islands. www.cawcr.gov.au/projects/PCCSP/publications.html, accessed January 2012.

Bird, C. 2009. Re-conceptualising Salvation: Some Insights from a Solomon Islands Culture towards a Relevant Theology of Salvation for Today." Pacific Journal of Theology II 42, 23–39.

Campbell, J. 2009. "Islandness: Vulnerability and Resilience in Oceania." Shima: The International Journal of Research into Island Cultures 3 (1), 85–97.

Cook Islands Government. 2011. The Cook Islands Second National Communication under the United Nations Framework Convention on Climate Change. Completed by the National Environment Service of the Government of Cook Islands. 110 pages.

FAO 2008. An Assessment of the Impact and Climate Change on Agriculture and Food Security. A Case Study in the Cook Islands. Prepared for FAO by Muliagatele Joe Reti, Apia: Samoa.

Finucane, M.L. 2009. "Why Science Alone Won't Solve the Climate Crisis: Managing Climate Risks in the Pacific." The Asia Pacific Issues 89.

Halapua, W. 2008. Waves of God's Embrace: Sacred Perspectives from the Ocean. Norwich: Canterbury Press.
Helmreich, S. 2009. Alien Ocean: Anthropological Voyages in Microbial Seas. Berkeley: University of California Press.
Ingram, I.P. n.d. Presentation by Imogen P. Ingram Te Pa Mataiapo of the Koutu Nui of the Cook Islands on TEK and Links to Climate Change in Rarotonga, Cook Islands, Southwest Pacific. www.ser.org/files, accessed May 2011.
Lafale, P.F. 2010. "Ua'afa le Aso Stormy Weather Today: Traditional Ecological Knowledge of Weather and Climate. The Samoa Experience." Climatic Change 100, 317–35.
Lonely Planet: Cook Islands. When to Go and Weather. www.lonelyplanet.com/rarotonga-and-the-cook-islands/weather, accessed May 2011.
McCormack, G. 2005. El Nino: Droughts, Cyclones and Coral Bleaching. http://cookislands.bishopmuseum.org, accessed May 2011.
Mercer, J., D. Dominey-Howes, I. Kelman & K. Lloyd 2007. "The Potential for Combining Indigenous and Western Knowledge in Reducing Vulnerability to Environmental Hazards in Small Island Developing States." Environmental Hazards 7, 245–56.
Minura, N. et al. 2007. "Small Islands", in M.L. Parry et al., eds. Climate Change 2007: Impacts, Adaptation and Vulnerability. Contribution to Working Group II of the Fourth Assessment Report of the Intergovernmental Panel on Climate Change. Cambridge: University of Cambridge.
Mol, A. 2002. The Body Multiple: Ontology in Medical Practice. Durham: Duke University Press.
Mol, A. & J. Law. 2002. "Complexities: An Introduction", in J. Law & A. Mol. Complexities, Social Studies of Knowledge Practices. Durham: Duke University Press.
Nunn, P. 2009. "Responding to the Challenges of Climate Change in the Pacific Islands: Management and Technological Imperatives." Climate Research 40, 211–31.
Nunn, P. 2010. "Bridging the Gulf between Science and Society: Imperatives for Minimizing Societal Disruption from Climate Change in the Pacific", in A. Sumi et al., eds. Adaptation and Mitigation Strategies for Climate Change. Tokyo: Springer.
PDC (Pacific Disaster Center) n.d. Hurricane. www.pdc.org/iweb/hurricane, accessed May 2011.
PICCAP 2000. Cook Islands Initial National Communication under the United Nations Framework Convention on Climate Change. Report by Pacific Islands Climate Change Assistance Programme (PICCAP), 41 pages. www.sprep.org/attachments/27.pdf, accessed May 2011.
Rubow, C. 2009. "Metaphysical Aspects of Resilience: South Pacific Responses to Climate Change", in K. Hastrup, ed. The Question of Resilience: Social Responses to Climate Change. Copenhagen: The Royal Danish Academy of Sciences and Letters.
Rudiak-Gould, P. 2011. "Facing Climate Change in the Marshall Islands: A Study in the Cultural Cognition of Risk." PhD thesis, Trinity, Oxford.
Scally, A. Fes de et al. 2006. A History of Tropical Cyclones and Their Impacts in the Cook Islands. Cook Islands Meteorological Service.
Scally, A. Fes de 2008. "Historical Tropical Cyclone Activity and Impacts in the Cook Islands." Pacific Science 62 (4), 443–59.
Schutz, A. 1970. On Phenomenology and Social Relations: Selected Writings. Chicago: Phoenix Books.
South Travels Holidays & Tourism Co. Weather and Climate in Cook Islands. www.southtravels.com/pacific/cookislands/weather.html, accessed May 2011.

SPREB 2009. Pacific Adaptation to Climate Change. Report of in-country consultations. www.sprep.org/att/IRC/eCOPIES/Countries/Cook_Islands/67.pdf, accessed May 2011.
Taylor, A.J.W. 1999. "Value Conflict Arising from a Disaster." The Australian Journal of Disaster and Trauma Studies 2.
Terry, J.P. 2007. Tropical Cyclones, Climatology and Impacts in the South Pacific. New York: Springer.
Tofaeono, A. 2000. Eco-theology: AIGA. The Household of Life: A Perspective from Living Myths and Traditions of Samoa. Erlangen: Erlangen Verlag.
Turua, B. 2003. "Kia Pu'era: An Oral Tradition of Rarotonga Origins", in R. Crocombe & M. Crocombe, eds. Cook Islands Culture: Akono'anga. Institute of Pacific Studies, Suva, Fiji, in Association with the Cook Islands Extension Centre, Rarotonga, University of the South Pacific, the Cook Islands Cultural and Historic Places Trust, and the Ministry of Cultural Development.
Webb, A.P. & P.S. Kench 2010. "The Dynamic Response of Reef Islands to Sea Level Rise: Evidence from Multi-decadal Analysis of Island Change in the Central Pacific." Global and Planetary Change 72, 234–46.
WMO 2006. Statement on Tropical Cyclones and Climate Change. Submitted to CAS-XIV under Agenda Item 7.3 by Dr G. B. Love, Permanent Representative for Australia. Prepared by the WMO/CAS Tropical Meteorology Research Program, Steering Committee for Project TC-2: Scientific Assessment of Climate Change Effects on Tropical Cyclones. 4 pages.

5 Anticipation on Thin Ice

Diagrammatic Reasoning in the High Arctic

Kirsten Hastrup

This chapter analyzes the experience and understanding of the changing environment by High Arctic hunters for whom the sea-ice is crucial to the mode of living. The idea is to show how they deal with the rapidly changing ice conditions and seek to anticipate both the near and the more distant future by way of diagrammatic reasoning. By this I refer to a kind of modelling that operates by means of networks and images, rather than concepts and numbers. It is at base a mode of reasoning that allows for both experimentation and anticipation, which is vital in the changing North. Until recently, the hunters of Northwest Greenland, from where my case derives, could rely on a stable sea-ice for nine to ten months a year (Gilberg 1986); now it is down to four or five months, and even within this brief period, the ice is increasingly unstable. To safely navigate the reformatted ice, the hunters must be extremely attentive and stretch their skills at reading the ice to the limit.

It is all the more pressing, because it has been suggested that the rapid and extensive variations in sea-ice may result in abrupt climate change, defined in relation to thresholds and to non-linear behaviour of the climate system per se, which evidently have major implications for social life in the North (Gildor & Tziperman 2003; cf. Hulme 2003). This will require new skills of navigation, and new kinds of social performance. The anthropological backbone of the argument is that social worlds are *enacted* rather than simply given. Communities have no existence outside of practice and action—however much they seem to be systematic in some sense or other. With no hunt, there can be no hunting community, to put it simply. By their unique and unrepeatable acts, people contribute to a perceived pattern; social life is routinely choreographed by the ceremonial animal (James 2003). Stressing the agentive rather than the institutional, the semantic, or the cognitive aspects of social life is to acknowledge both its spatiality and its temporality. In fact these two dimensions are so deeply ingrained within the social that they can hardly be singled out in practice; this composite world obviously incorporates the physical world.

People act upon climate change within particular knowledge spaces, by which I refer to David Turnbull's suggestion of a knowledge space being an "interactive, contingent assemblage of space and knowledge, sustained and created by social labour" (Turnbull 2003: 4). This goes to say that the physical space is always naturalized in particular ways within different forms of knowledge; thus, in the process of knowing the world, we also shape it in a particular way—which is again confirmed by knowledge. In both of these processes, social labour enters into the equation by the effort it takes to organize and argue for a particular kind of spatial knowledge. Once established, this effort recedes from view, and the knowledge stands out as self-evident.

When enacting a particular knowledge space, people act as much upon anticipation as upon antecedent (Hastrup 2007). In the process, they realize a world as they imagine it; the Arctic hunters do not go out in search of game simply as matter of habit, but also because they imagine an outcome in the shape of a possible catch. This implies that expectation and imagination play important parts in their actions. When the environment changes rapidly, the imagination is strained, and we have to revisit the ways in which people seek to anticipate their world in view of the comprehensive uncertainties. Anticipation is a precondition for responsible action, as opposed to random behaviour, and one of the responsibilities that the hunters take upon themselves is to enact the constituent of their society—the hunt. Increasingly, they also pose the question of how long this will remain possible, but so far they continue, if under ever more precarious conditions.

In the High Arctic the most pressing social concern relates to the changing nature of the ice, which was always part of their life (Krupnik et al. 2010). With raising temperatures, the glaciers are melting rapidly and the sea-ice has become unpredictable all over the Arctic (see e.g. Laidler et al. 2010). This greatly affects both the immediate living conditions and the future scenarios for life in the far North, not only because of the limitation imposed upon the ice hunt as such, but also because of the manifestly changing animal behaviour, in terms of patterns of reproduction, migration, and otherwise. My consecutive fieldworks in the northernmost settlements in Greenland over the past five years have illuminated some of the ways in which the new uncertainties are dealt with, and how the local "knowledge space" is reshaped when the physical space erodes.

In this chapter I shall seek to develop a general understanding of the mode of anticipation in a melting world, which is increasingly perceived as an *interim* between stable conditions, even if environmental stability has never really been the case (Hastrup 2009a; cf. Hulme 2010). The unnerving question for the hunters is for how long they can continue to take responsibility for enacting and even imagining the world they know best: the world of hunting.

ANTICIPATION AND DIAGRAMMATIC REASONING

Before we move north, so to speak, we have to establish some conceptual ground. I would like to start by expanding on the notion of the *interim*, used above. It has a particular meaning in relation to social action, and by implication in relation to anticipation in a world where responsible action is increasingly put to test by the mounting difficulties in forecasting—the weather as well as the world. Facing environmental changes of some magnitude, people seem to be caught in an interim like the one proposed by Shakespeare whose protagonists, e.g. Macbeth (in *Macbeth*) and Brutus (in *Julius Caesar*), are often caught up in histories that they themselves may have put in motion, but cannot control. The interim is a moment of fundamental uncertainty of outcome. It is a temporal crisis or "a moment that seems exempt from the usual movement of time, when the future is crammed into the present" (Kermode 2000: 205). The interim is saturated with ontological uncertainty. *Acting* temporarily may relieve this uncertainty, but only to land the agent in new uncertainties; the world as we know it is partly the result of acts, whose consequences we cannot foresee. This goes for social acting in general, because *the present* is always the momentous unknown, rather than the future per se; it is now that we do not know what to do and what our actions will entail (Strathern 1992: 178). Living within an ever more circumscribed environment presses this point home for the North Greenlandic hunters, whose future is intensely bound up with the present. Age-old knowledge of the actual entanglement of nature and society is moving out of bounds, as so vividly testified to by the sea-ice, where they cannot be disentangled at all. Ice, hunters, and game are mutually defining.

Action is never simply a *re*action to what has already happened; it is also a mode of acting upon anticipation. Agency in this sense is closely tied to a vision of plot, to the anticipation of a story, or a line of future development. It is a profound matter of *responding*, response being made within a moral horizon and within a social context that is interpreted and projected forward as people go along. "Anticipation is also potentiation" (Strathern 1992: 178). Without a sense of plot, meaningful action would be precluded—also outside of Shakespeare's dramatic world (Hastrup 2004). The sense of plot is what integrates individual actions into a larger vision of the world, filled out imaginatively and acted upon.

In a situation of profound environmental change, the sense of plot is blurred in the High Arctic. It is next to impossible to forecast even the near future of the ice, and concomitantly of the hunt. The moment seems outside the usual movement of time. Even the seasonal rhythm is thwarted: temperature, sea-ice, glacier velocity, ocean life—all of it is unpredictable. Too much future is crammed into the present, and social life cannot be unfolded along well-known story lines. Importantly, in the High Arctic, the temporal crisis is first experienced through spatial (physical) disruptions to which one must respond—once again testifying to the interpenetration of society, time, and space. Among the disturbing factors, the most prominent is the fact of the thinning, degrading, or melting sea-ice, which is so much more than a physical matter. The sea-ice "has a profound *social ontology*, an existence as a social object by virtue of the deep-seated meanings and relations that connect to Inuit life" (Bravo 2010: 446). This implies that the sea-ice is understood both in terms of its composition, texture, age, structure, and carrying capacity, and in terms of the social relations between people and their prey. The complex sociality of the sea-ice points to a knowledge space that is in many ways an alternative to geophysics, but neither inferior nor simply traditional—as opposed to scientific.

This is where *diagrammatic reasoning* may serve as a means to open up the mode of anticipation entertained by the hunters, operating within a particular knowledge space. It is a particular kind of reasoning, which is different from a historical projection as embedded in the linear narrative conventionally associated with ideas of history and plot. At present, in the High Arctic (as in many other regions equally affected by climate change) the future cannot be anticipated through simple historical projection, but calls for another kind of imagination based on a mental play with diagrams. Evidently, in the High Arctic people have lived with great weather variability and momentous uncertainties about the availability of game since times immemorial; but now, the sense of living in an interim is so pervasive that the thread of history seems about to break asunder along with the ice, which one still has to navigate in spite of everything. This makes it opportune to revisit the idea of diagrammatic reasoning as suggested by Peirce.

The diagram, according to Peirce, is an icon of a set of related objects, which is not subject to the trivialized concept of similarity, but connects it to processes of deduction: "All deductive reasoning . . . involves an element of observation; namely, deduction consists in constructing an icon or diagram the relations of whose parts shall present a complete analogy with those of the parts of the object of reasoning, of experimenting upon this image in the imagination, and of observing the result so as to discover unnoticed and hidden relations among the parts" (Peirce; quoted in Stjernfelt 2011: 91). By adding this operational aspect to the icon, Peirce's diagram parts company from simple iconic similarity and opens up for imaginative experiments; at the same time, due to the fact of its iconicity, the diagram corresponds to a real possibility (Stjernfelt 2011: 91, 98). In other words, being an icon, the diagram cannot be inconsistent. "It may display non-existent entities, but not logically inconsistent entities. Its object is necessarily *possible*" (ibid.: 99).

Therefore, there is more to diagrammatic reasoning than a simple imagery, if we follow Peirce, for whom logic itself could be seen as a manipulation of diagrams (Putnam 1992: 68ff). The diagram constitutes a basic laboratory where one may experiment with relations and make sound deductions; it provides a possibility of focusing on experimentation by way of geometric intuition (Peirce 1992: 262). Deduction in this sense is not a linear operation from the abstract to the concrete or from theory to actual instance. Actually, although a diagram is probably most often understood in more or less geometrical terms, Peirce understood the notion of diagram much more broadly as encompassing visual, tactile, and other entities that could be used to model a set of relations (Ketner & Putnam 1992: 2). For Peirce, the diagram began in mathematics and geometry, but it led onwards to a more general notion of diagrammatic reasoning that involved observation, imagination, and experimentation as elements in a larger process of deduction. We can see how this instantiates some of the qualities of modelling, as described in the introduction (Chapter 1, this volume).

The general point to remember in the present context is that a diagram is a synthesizing model of qualitative relationships, from which one may deduce a kind of knowledge that is consistent with the actual possibilities. It is this relational feature that makes the diagram open to experimentation. If one element changes, it may affect all the others, depending on their sensitivity. Thus, diagrammatic reasoning draws upon the ability to imagine logical (that is, truly possible) relations and manipulate them mentally in a time-ordered sequence, and thus to be able to anticipate outcomes of particular moves made somewhere on the "map". At this stage, we may think of a chessboard with its pawns, bishops, towers, etc. as a simplified map, and we already get an idea of the complexity of the diagram, being so much more than a two-dimensional representation, because of the rules that connect and empower the pieces and their positions on the board. The experimentation with this particular (chess) diagram is akin to the imagined, possible moves made by the various chess-pieces on the board, given their relations and, of course, their values. As noted by Saussure in 1916, when he used the chess analogy to enhance the understanding of the workings of language, every individual move may have repercussions in the entire system (Saussure 1974: 88–89). So also for the "moves" of nature, when it comes to understand the shifting possibilities afforded by observed instances of critical change. This is a first hint of the power of diagrammatic reasoning, which is part and parcel also of climate models of other kinds. With the chessboard in mind we get a glimpse of the multi-dimensionality of the diagram, being so much more than a simple spatial representation of a plane.

In the High Arctic, where people are now facing dramatic changes in the sea-ice and glaciers due to unprecedented warming also of the sea, and a concomitant uncertainty in navigating the environment, diagrammatic reasoning has become an all-important resource of anticipation in the practical negotiation of the new environmental challenges. Like other modellers, the hunters of the far North seek to anticipate what cannot be known, and they do it by way of a careful reading of the shifting qualities of the ice, which are then fed into a template for understanding the local topography and its affordances in relation to temperatures, winds, sea-currents, and much more (cf. Fortescue 1988). The inherent mobility and flexibility of social life was always a precondition for survival in the region, as noted by many early ethnographers, including the fine-tuned perception of wind and weather, and the movements of animals that installed a sense of an extensive network of affordances (e.g. Boas 1964). I suggest that we see this as an expression of an ongoing experimentation with relations and moves within a diagram, serving as an iconic model of the environment. Before we can properly assess the potential for anticipation, we shall briefly consider the cartographic aptitude of the Northerners, and discuss how cartography itself is also no simple representation of the world, but opens up for mental experiments like other diagrams.

MAPPING A MOVING WORLD

The environmental shiftiness and the dependence upon nomadic animal resources always induced the Arctic peoples to move around in order to survive (Boas 1964; Mauss 1979; Hastrup 2009a). At a symbolic level, there was a close connection between a child's first journey and being introduced to the world, as Knud Rasmussen (1929: 47) noted for the Igloolik in Canada (now Nunavut). People never moved at random, of course, but according to careful considerations of the potential affordances of particular sites. The mobile, even nomadic, life-style implied a deep familiarity with the region, which was appropriated from within, so to speak (Hastrup 2009b; Nuttall 2009). Tim Ingold has suggested that "a region" is constituted by people situating themselves within a matrix of movement (2000: 235); it consists in relations between places, which "exist not in space but as nodes in a matrix of movement" (ibid.: 219). This notion of a region makes sense in the Arctic, so thinly populated and so dependent upon mobility.

At present, this matrix is shifting and hence the region, which is predominantly constituted with the sea-ice. As noted by Aporta, the "fact that the sea temporarily transforms into a land fast ice platform, supporting movement and life, makes Ingold's concept of region even more adequate, as the Inuit's well established networks of trails are in constant transition between land and ice" (Aporta 2010: 165). The knowledge space of the Inuit thus incorporates the ice, which is implicitly ascribed with a social ontology, as suggested above.

The capacity for orientation within a vast region that appeared so ill defined to European explorers from the 19th century onwards made a strong impression upon the newcomers. In a number of early works from the first encounters it was highlighted as a particular geographical skill. In 1841, a biography of a Canadian Inuit, Eenoolooapik (or Eenoo), was published, subsequent to Eenoo's voyage to Scotland and back (M'Donald 1841; Jones 2004). When still at home, Eenoo had drawn a map for the Scotsmen, in which he had placed a deep fjord, then unknown (or forgotten) by the Europeans. From experience, Eenoo knew the fjord to be full of whales, and the sailors were keen to get there. Impressed by his skills at drawing, the Scotsmen invited him to return with them to Scotland, to help them seek funding for an exploration of the region he had depicted. M'Donald was surprised by Eenoo's intimate knowledge of places, even places where he had never been, but of which he had heard, and he made the following note on Eenoo's geographical mind:

> Indeed, he seemed to possess in a high degree those faculties of mind which phrenologists have adduced as finding their legitimate exercise in the observation of the relative situation, extent, and peculiar appearances of places. He also took delight in copying maps and charts, and in pointing out upon them such places as were familiar to him; and although he was ignorant of mathematical principles of geography, he could delineate with remarkable precision the actual direction of any coast, and the true position of its different parts. He could trace the course, which we had taken across the Atlantic and would, at any time when asked, point out the proper bearing of any place, which we had visited. (M'Donald 1841: 72; quoted from Jones 2004: 64)

When it came to the uncharted sound, its extension and shape were fully confirmed upon their return. It was later to provide a rich resource for whales, as Eenoo had already told them. The point of citing the tale in the present connection is the fact that it is probably the first discussion of the allegedly generic Inuit facility to prepare maps (Jones 2004: 70). As Eenoo's biographer says:

> The best marked feature of his [Eenoolooapik's] mental constitution was the ample development of those faculties on which the attainment of geographical knowledge depends; and it will be recollected that the first circumstance which attracted attention to him at all, was the extent of his acquirements in that department. The facility with which he had acquired this knowledge is apparent from his having only *once* sailed between Keimooksook and Durban along the coast, the features of which, after a long interval of time, he described with such remarkable accuracy, I am inclined to believe, not only from my own observation, but also from the accounts given by Parry and others, that the Esquimaux generally possess the mental faculties necessary for this attainment in a pretty high state of perfection; and when we consider that they are forced from their situation to derive almost their whole subsistence from the sea, and often obliged for this purpose to undertake long journeys, and necessarily migratory in their habits—the necessity for such observational capacities appears abundantly obvious. The readiness, too, with which Eenoolooapik acquired the power of communicating this knowledge—his using rude sketches for the purpose of making himself understood when language altogether failed him, and the fondness which he shewed for drawing, all afford additional evidence of the activity of the same elementary faculties of mind acting in a different manner in consequence of the difference in his situation. (M'Donald 1841: 107–08; quoted from Jones 2004: 71)

What is highlighted here is the practical engagement with the environment as the source of the remarkable cartographic capabilities. These were put to use also by the prominent British explorers, John and James Ross who had actively sought out Eskimo knowledge of the region into which they moved some years earlier. James Ross himself made an excellent drawing of two named Eskimos helping the explorers mapping and finding their way in the icy waters of the Arctic.

As noted by Turnbull (2003: 94ff), territorial (and scientific) discovery—which was the driver of the expeditions made by John and James Ross—has often been conflated with and mediated by mapping. The experience with maps increasingly made the Western scientific mind see the "world" as something laid out before their eyes, a vast assemblage to be further explored: "The ancient oral world knew few "explorers", though it did know many itinerants, travellers, voyagers, adventurers and pilgrims" (Ong 1982: 73). The alleged "great divide" between literate and oral traditions was bridged, it would seem, by the Eskimos who took hold of the pencil and contributed to the map-making, much to the delight of the European explorers whose expectations to the illiterates were different. In itself, this bridging points to the significant fact that all knowledge is *located*, and assembled from the motley of practices, instrumentation, theories, and people (Turnbull 2003: 38). Eenoo's practical knowledge was put to use within a particular social context, where different practices and theories were assembled in a shared, located knowledge—transcending the strictly local on both sides of the equation. With paper, pen, and pencil new avenues of two-dimensional mapping emerged, and the Eskimos were quick to take this kind of instrumentation into their own hands.

The remarkable dexterity by which pencils were used immediately after introduction is testified to also from an encounter in North Greenland. The people up there (thenceforth to be known as Polar Eskimos) were visited in 1902–04 by a Danish Literary Expedition to Greenland (aiming at collecting oral traditions). With the expedition was a painter, Harald Moltke, who was astounded by the Eskimos' urge to participate in precisely that kind of representation that he himself was engaged in. Moltke deliberately refused to teach them how to draw, not to direct their skills in any particular way, but he did lend them paper and pencils, and he relates the outcome in the following way:

> A true passion for drawing has seized all the young Eskimos in the settlement. They all beg us for pencils and paper and draw many interesting figures and tools. Their perception of the living life, and of its movement is always very characteristic. Especially Tâterark, who has never before held a pencil in his hand, draws pictures full of life, for instance of some men dragging a walrus ashore. There is certainly swiftness and movement in these figures; they run, they work eagerly, pull and drag. It is interesting and peculiar that these children of nature when they lay hand on a pencil for the first time, spontaneously reproduce the lived life with much more certainty that the majority of civilized people could muster. (Moltke & Mylius-Erichsen 1906: 420–21 [my translation, KH])

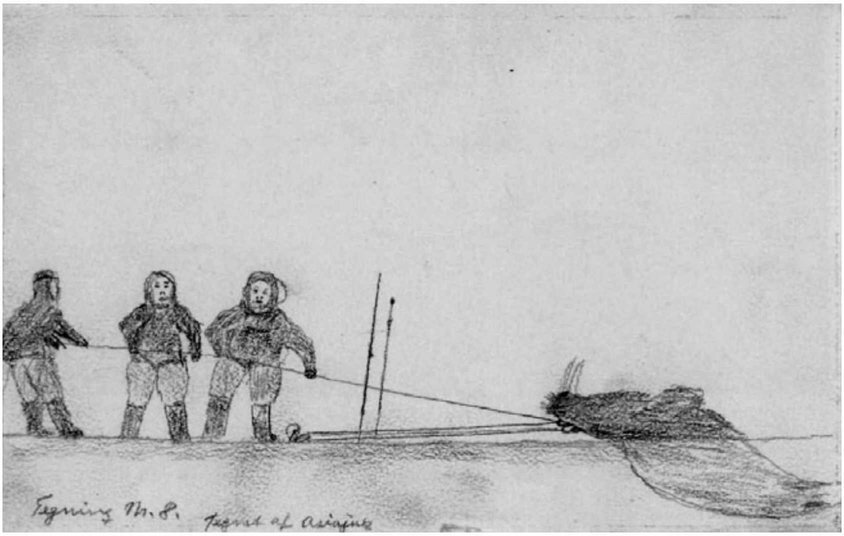

Figure 5.1 Three hunters dragging a walrus up onto the ice. Drawing by Asiajuk, 1903. (Original in Ilulissat Museum, Greenland. Reproduced with permission.)

Here we get a glimpse not only of the capacity for drawing, but also of the drawers' skills of transforming a life in flux into icons. The hunting scenes depicted show the movement related to hunting and travelling, and the landscape is drawn with a view to its potentiality for game. Thus, the drawings are indicative of the mode of orientation within the matrix of possible hunting grounds, which is not mediated by simple maps, but by a specific kind of diagrammatic reasoning that allows for anticipating the possible gains by moving in a particular direction, based in the relational actualities of ice, weather, and wind and the possibilities for game. In other words, anticipation takes place in a moving knowledge space that calls for a kind of "mapping" that is dynamic and based in a located diagrammatic reasoning that is consistent with the world and its (potential) affordances.

CARTOGRAPHIES AND POINTS OF SIGNIFICANCE

Before we move on to the present challenges in the High Arctic, where the movement of the world is intensifying and navigation calls for almost super-human alertness, we shall dwell a little longer with cartographic practices and their relation to diagrams. Before the pencil allowed for the drawing of "sci-entific" maps, another kind of maps were known in Greenland in the shape of three-dimensional carvings of driftwood, featuring the coastlines and carried within the mittens (Gulløv 2000). They date back at least 300 years and show

how the topographical features of the coastline were memorized through touching and feeling the wooden shape. Evidently, the mobile hunting populations charted their environment through all their senses and knew where they were going. If, in science, cartography is still very much a story of technological achievement and increasing precision, it is still just one particular version of the human mode of relating to the world (Pickles 2004).

In the Northwest Greenlandic region where I now have the privilege of working, we may see such versions side by side in the shape of two historical maps featuring the same coastline. In 1903, when the above-mentioned literary expedition "discovered" the Polar Eskimos in Northwest Greenland, Moltke asked one of them to draw a map of the coastline further north between the settlement of Etah and the Humboldt Glacier. It shows a detailed topographical knowledge and features a list of place names that are descriptive in various senses. Most of them refer to topographical peculiarities, such as flat-island, steep cliffs, or big ice, but they also comprise references to hunting feats or camps, such as Island of Geese, the Good Headland, and Housing Place; a couple of places are even named after incidents of manslaughter. Whether referring to features of topography or memories of particular events, the place names are practical landmarks rather than abstract positions.

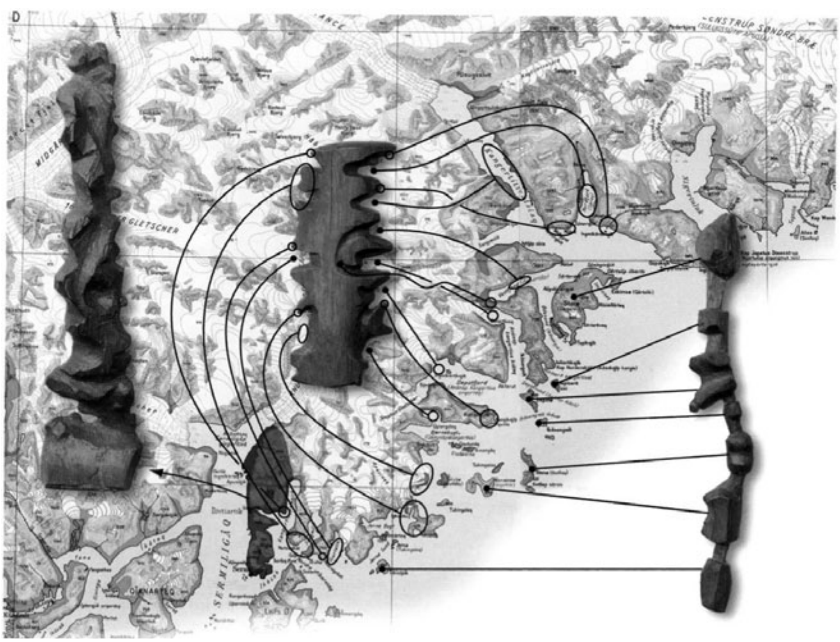

Figure 5.2 Three-dimensional wooden maps from the east coast of Greenland. (Reprinted with permission from H.C. Gulløv, the National Museum of Denmark, who made the connections between the xylographic and the cartographic representations. First published in his article "Østgrønlandsk kartografi og xylografi". *Topografisk Atlas Grønland*. Copenhagen 2000: Kort og Matrikelstyrelsen).

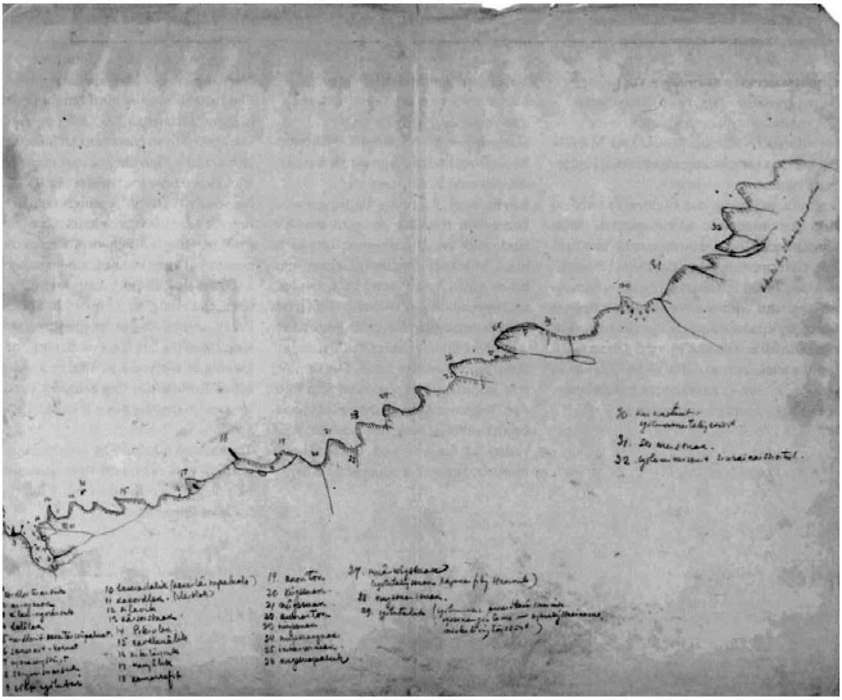

Figure 5.3 Map of coastline between Etah and the Humboldt Glacier. Drawn by unnamed hunter of the Thule District, 1903. It was presented as a map over the "Bear-hunting route" or the "Reindeer route", and covering the coastline up to "The Great land without many mountain hares". (Original in Ilulissat Museum, Greenland. Reproduced with permission.)

Toponyms are well-known points of orientation and of memory in the Arctic world and testify to a world of moving about within a region of particular contours and significant encounters with animals and people, on land and on ice (Collignon 2006; Krupnik et al. 2010). The names on a map (drawn or remembered) that point to visible ice structures or known hunting grounds no longer seem to fit, however. This is vividly suggested by Uusaqqak Qujaukitsoq, a hunter from Qaanaaq in Northwest Greenland:

> Sea-ice conditions have changed over the last five to six years. The ice is generally thinner and slower to form off the smaller forelands. The appearance of *aakkarneq* (ice thinned by sea currents) happens earlier in the year than normal. Also, sea ice, which previously broke up gradually from the floe-edge towards land, now breaks off all at once. Glaciers are very notably receding and the place names are no longer consistent with the appearance of the land. For example, Sermiarsusuaq ("the smaller large glacier"), which previously stretched out to the sea, no longer exists. (In Huntington & Fox 2005: 84)

This is a very important observation pointing to a deep-seated feeling of change in the local sense of emplacement. Place names have for a long time served as a means to crystallize memory and society in the Arctic environment of infinite extension. As Kleivan has it: "Past Greenlanders whose culture was based on oral and not written sources, were not in possession of maps, but the place names functioned as a kind of map which constituted a description of the land. Using place names enabled them to plan hunting trips as well as social visits: knowledge of place names was thus an important aspect of their hunting culture" (Kleivan 1986; quoted in Sejersen 2004: 72–73). The fact that in Greenlandic, most place names refer to physical features of the landscape, to particular hunting grounds, or to activities of some kind, testifies to what Basso has called the people's participation in the landscape (Basso 1996: 44ff). For Inuit outside of Greenland it has also been noted how place names reflect a particular environmental knowledge; increasingly, they serve as historical markers of past possibilities and activities, rather than actualities (Henshaw 2009: 161). The Earth has now become so "fast" that it has outrun the old terms (cf. Krupnik & Jolly 2002).

When place names are no longer consistent with the appearance of the land, a sense of homelessness enters perception. Memories have become invalid, and this affects the sense of self; possibly even more important, the people are also being deprived of their visions for the future. Thus, the changing environment not only affects the hunt and communication, it unsettles people profoundly. Over the past 10 years, in the wake of accelerating climate change, there is a widening gap between the signifier and the signified of the nodal points of the cartography. What were once located as points of significance, conflating natural and social movements, have now become empty landmarks.

A second, and apparently more solid cartographic form is found in another map of exactly the same coastline, made in 1916 when Knud Rasmussen, who had participated in the Literary Expedition, undertook another expedition in the region. The expedition (known as the Second Thule Expedition) was explicitly designed as a mapping expedition adding more knowledge about the topography and geomorphology of the northern coastline of Greenland. This time the cartographic exercise was in the hands of science, in the geologist Lauge Koch's person. Knud Rasmussen himself was to identify and map the traces of earlier Eskimo settlements. Before they set out, Rasmussen had someone drawing ink-maps of various portions of the coast, onto which he could then map the evidence of past settlements. What is striking is that the purportedly unknown coast is heavily loaded with place names already, pointing to a different kind of located knowledge. This time the place names are not descriptive of topography or hunting events; rather they reflect the passage of numerous ships, commemorating their sponsors, captains, kings, or relatives. Whereas the first map signified movement, the second signifies achievement.

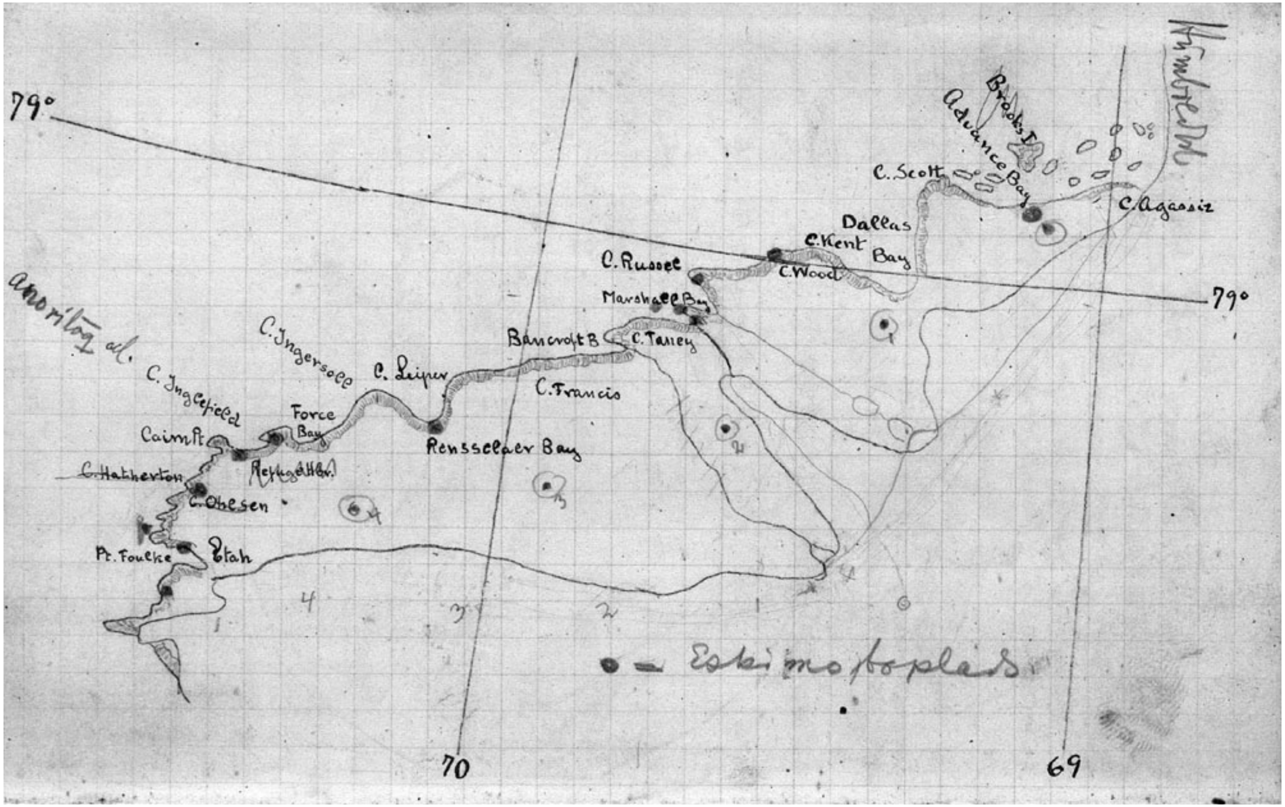

Figure 5.4 Map of coastline from Etah to the Humboldt Glacier. From the Diary of Knud Rasmussen, Second Thule Expedition, 1917. The map was pre-drawn in ink, while Knud Rasmussen added his own observations of pre-historical Eskimo settlements in pencil, marked by circles. (Original in the Royal Library, Denmark. Reprinted with permission.)

The different modalities of the two maps are not innocent; as spatial diagrams they produce social ontology in each their way, and create their own, located, knowledge spaces in the process. What they share is the feature of assemblage and sociality, and this is what makes them comparable. Even more pertinent to the present argument is that both may be seen as diagrams in Peirce's sense, because they are iconic and open to experimentation, if in different ways. In the first map the point of departure is an integrative social relationship between people and nature, where relations are more pertinent than boundaries, and place names serve as diagrammatic strongholds. In the second case, the feats of discovery and sea faring in a forbidding environment are celebrated. The coastline may look approximately the same, but it opens up for different kinds of experimentation, on the basis of their highlighting different points of signification.

Experimenting with a map may seem paradoxical if not outright impossible, but only if we subscribe to the implicit understanding of modern maps as objective renderings of geographical space. Yet, even the scientific maps are riddled with indeterminacies and replete with the social work that enters into any assemblage; creating the connections between the heterogeneous elements featured in the map cannot be achieved by abstract geometrization, because they have no natural relationship beyond that which is established as (socially) salient (Turnbull 2003: 100). This implies that all maps are also diagrams; they are both consistent with the worlds they depict and open to experiment. In the process of mapping, diagrams and worlds co-produce each other and fuse into particular knowledge spaces. Experiments with maps may be relatively simple and consist simply in finding an appropriate route between two localities, given the time of the year and the topography. Looking back at the coastline, as presented in the two maps referred to above, the experimentation might make us imagine the coast beyond the boundary of the map, knowing that "geographical ontology does not permit any coast suddenly to stop" (Stjernfelt 2011: 190). If we add the notion of social ontology to this, the imagination may take us even further. The boundaries of any map are always subject to selective interests, including political ones. While remaining consistent with it, experimentation may take us beyond direct experience and previous knowledge.

With computerized mappings we may find ourselves in a brave new world by comparison to the longhand of earlier generations, entirely relying upon the line of drawing (Pickles 2004). Yet we are still within a mode of spatial and diagrammatic reasoning, leading to some kind of truth about the world. With Latour (1999) we may argue that scientific truths about the world are established through circulating reference and a consolidation of facts that transform "raw data" to scientific categories without ever transcending them. In the process, the so-far-uncharted field takes shape as a map of discrete entities and relational objects. This is where it becomes pertinent to redeem cartographic reasoning as a means to multiplying the perspectives upon the world, not restricting them. The map is in itself an experiment:

> What distinguished the map from the tracing is that it is entirely oriented toward an experimentation in contact with the real. The map does not reproduce an unconscious closed in on itself; it constructs the unconscious. It fosters connections between the fields, the removal of blockages on bodies without organs . . . The map is open and connectable in all of its dimensions; it is detachable, susceptible to constant modification. It can be torn, reversed, adapted to any kind of mounting—reworked by an individual, group, or social formation. (Deleuze & Guattari 2004: 13–14)

This way of understanding a map relieves us of the antagonistic stance towards cartography in general, and takes us towards the diagrammatic logic. What is now possibly more significant than denunciating the enlightenment maps is the shift from a tactile cartography to a digital one (Pickles 2004: 174). The latter may enable us to map multi-dimensional spaces, but only in a language of abstract architectonic spaces and relations that have no direct bearing on experience and which therefore make endless experimentation possible. Although the technology may map new complexities, it does not necessarily follow that it produces a more solid ground for anticipating, and hence for enacting a viable world. This is evident in the Arctic where the introduction of satellite navigation has not reduced the hazards of moving about, but rather made them more serious (Aporta & Higgs 2005). Only by knowing the topography and by being able to read the ice in the first place may the abstract coordinates of the GPS be of any help.

NAVIGATION AND THE SKILL OF WAYFARING

We shall now discuss the diagrammatic logic by which the hunters navigate the rapidly changing environment, challenging the fixity of points while highlighting enduring relations between humans and the environment. The coastline may remain in place, but the territory for a large part consists of ice that not only shifts seasonally, but increasingly also shifts the seasons around. As Aporta has it for the Igloolik area:

> Ice is part of the territory where Inuit live for most of the year and traveling on the ice may take up to 8 months every year in the Igloolik area. Inuit understand the codes of such a changing place and have discovered its predictability, to the extent that they can exploit the moving ice on a regular basis. In the past, they used to make the landfast ice their home for part of the spring (Aporta 2002). Places like Agiuppiniq (an Ice ridge), Naggutilauk (an ice lead), Ivuniraarjuruluk (an ice build-up),

> and Aukkarnaarjuk (a polynia) recur every year at the same locations and are identified with names in a similar way as places on the land. The sea ice topography and processes are identified with complex terminologies. (Aporta 2010: 169–70)

The ice as depicted here, and as I know it from North Greenland, is now increasingly destabilized, and the diagrammatic reasoning must evolve accordingly for it to still be consistent with the world it depicts, and the possibilities it may afford. The challenge became evident to me, when joining a walrus hunting party and setting out on dog sledge on a day in early May 2010. I was (again) immersed in the emotional topography of the North (Hastrup 2010), where the expansiveness of the world within which the sledge moved conveyed a sense of unlimited potential. Wayfaring in snow-clad regions is a matter of weaving a strand of movement into the apparent stillness of the world (see Ingold 2010: 128). The dogs and the sledges leave their unmistakable impressions on the ground—at least when the snow has newly fallen or the route is just opening up for passage due to changed ice conditions or newly frozen ice.

In the case to be related here, the party set off at a time when the sea-ice still allowed for passage on the one hand, while the water had opened up within a day's reach on the other. This allowed potential access to walrus at the ice edge, which was now only about eight hours away. This was the actual time it took us to get there, but not one that could be pre-established, except in very general terms—between six and ten hours was the estimation when I asked beforehand. It was not so much a matter of uncertainty about the geographical distance; the hunters had been out there before and knew where they were going, but they could never know how the ice, snow, and wind would affect the actual (passable) route and the pace of the dogs. As inhabitants, the hunters move through the world and the weather *conditions* this move, to paraphrase Ingold (2010: 133, 134).

The hunters know that the sea-ice is susceptible to the forces of temperature and wind, but also to more hidden oceanic conditions, among which unpredictable currents were often mentioned. The hunters also knew that the closer we got to the ice edge, the more fragile the sea-ice was, and the more attentiveness driving demanded; cracks were not visible from the sledge until they were close, but cues were taken from the colour of the ice, the slush on the surface, and the position of small icebergs that might have turned upside down because they had become top-heavy, thus indicating a melt-off from below on the one hand, and a sea-ice so thinned out that it lost its grip. More long-distance cues were taken from distant flocks of seabirds known to congregate over open water.

While the ice was in some way a known territory, it was also a shifty partner in the progress of the party as it generally is in the Arctic (Henshaw 2009). *Knowing* the way certainly implied an astute "sensitivity to cues in the environment and a greater capacity to respond to these cues with judgement and precision" (Ingold 2010: 134). Usually, I was told, the edge would have been much further away at this time of year, but over the past 10 years the sea had opened up earlier and at a greater pace, and the hunting grounds for walrus had come within easier reach even from our village. The drawback was that access to the edge had become more risky, and that the islands further way, where multitudes of birds were just arriving from the south to nest, had been cut off.

Going to the hunting ground at the ice edge meant moving along a path that was already impressed into the surface; for the past two weeks, hunters had gone out there regularly for three- to five-day camps and the path was clearly marked, if not as a straight line then certainly as a track with its own curves and bends, responding to the ground. The sinuous course became even more conspicuous during the last (long) stretch to be covered, where the driver had to pay close attention even to the beaten track. New cracks might have opened and new patches of thin ice emerged. Some cracks could easily be driven over by the rather big sledge, whereas others had to be sidestepped. In that sense, the driver had to act out his skills at "wayfaring". "The wayfarer is a being who, in following a path of life, negotiates or improvises a passage as he goes *along*" (Ingold 2010: 126). In contrast to wayfaring, Ingold suggests that *transport* simply carries the passenger *across* a surface, connecting the point of embarkation with a point of destination (ibid.). I would argue that the hunters' mode of travelling combined the two. While certainly negotiating and improvising their passage as they went along, they also had a specific destination in mind. The ice edge was their destination, because that was where the emerging hunting ground was found. It was not an absolute or fixed terminus, because the edge might shift, and there would be other hunts, other paths, and other ice conditions to engage with. The terminus cannot be translated into a fixed point, defined by abstract coordinates; the hunters' destination was not so much the physical end point of the journey, as it was the opportunity to hunt, and thus to secure meat for people and dogs. The hunters would not set out without such destination; this is where moving *across* infiltrates the moving *along*, or where transport and wayfaring merge.

This is also where diagrammatic imagination again becomes all-important, because we move beyond calculation to orientation by other means of navigation. I have mentioned the reading of the sea-ice as an instance of this, but there is more to it. The behaviour of the animals is likewise closely monitored; one example is the increase in polar bear hunt in the region, which is read as a consequence of less sea-ice also further north—inducing the bears to go inland and approach the settlements (Born et al. 2008). In this particular case, the frequency of bears "here" is a lateral testimony to their shrinking habitat "there", and thus ultimately to the probable decrease in the polar bear population.

There are lots of such lateral clues to the state of the sea-ice, and the hunters have to move by an implicit diagram of their interrelations; in that they are like climate modellers who have mapped the relations between the Arctic Ocean ice and climate for 100 years. Cracks here means something different there; clouds forming in this way reflects a hostile wind to come;

the increase in fogs and hence the disappearance of landmarks tells about open waters nearby. While the hunters now have access to satellite images that may give them a general map of the lay of the land and the present edge of the ice, they still have to rely on their own skills of diagrammatic reasoning for moving on the ice in between. Like Pacific navigation, analyzed by Turnbull, we may conclude that the diagram, by which the Arctic hunters navigate, forms a logical construct or cognitive map:

> The sophistication and complexity of this cognitive map is fully realised when the canoe is tacking against the wind requiring constant course changes as well as estimations of the effects of drift and current. Thus the system is more than a map in the Western sense; it is a dynamic spatial organisation of knowledge, but it is also a technical device, albeit a mental one, for assembling and moving local knowledge. (Turnbull 2003: 140)

And like the Pacific navigators, the Arctic hunters have to consider both the immediacy of weather and wind, sea-ice and currents, and the distant and invisible destination.

PROJECTIONS IN TURBULENT TIMES

The hunters know where they want to go; on the undrawn map, the hunting ground is identified on the basis of past experience and anticipated game. The ice edge is moving, opening the sea ever more, but closing down traditional hunting grounds; in the process, new knowledge is assembled. Even when it is there, the sea-ice cannot be trusted, as I was told by a hunter when we sat on a sledge, conversing:

> The ice has changed. Right here, it is less than half a meter [he shows about 30 centimetres by his hands]. There are so many currents now around Herbert Island, which makes the ice thin close to land. Between Herbert Island and Qaanaaq, I think the ice is between one metre and one and a half metre. There are no problems there, but here it is bad. It is difficult, but we know how to move [clearly wanting to reassure me, the guest in their party]. The water has opened up, and has therefore become warmer, and this means that ice melts from below. This is also due to new and warmer currents. So the ice is very thin and unsafe in many places. (Conversation with hunter, May 2010)

Once again it is *akkarneq*, the thinned ice, that troubles the hunt, and the explanation is given by reference to new unpredictable sea-currents. Others echoed this in so many ways. An interesting case related was that of a recent *sassat* (the capture of a pack of whales in a hole in the ice, from which they cannot escape and are doomed to drown as the gap closes). The place was pointed out to me en route, and although I was impressed by the certainty by which the hunter knew the exact place on the ice by way of combining several landmarks, I was also hit by the fact that even the whales had lost their orientation. It had happened once before this past winter, but never within living memory before that. The open water and the packed ice used to be much more clearly demarcated, also in time, but now openings and closures of the ice had out-manoeuvred everybody's instincts. In the Arctic in general, the amount of accidents on the ice has increased, and in North Greenland now every year the ice edge breaks off unexpectedly, making dogs and hunters drift into the open sea on the floes—later to be picked up by helicopters from the Thule Airbase, so far.

This is the point: every diagrammatic logic must allow for deduction, and for the most part it does, also in the far North, as far as immediate navigation is concerned. Yet, the increasing amount of turbulence from below now seems to intensify the uncertainty about the future scenarios. Predicting the next *best* move becomes more difficult. This holds both in terms of whether to go hunting and in terms of the bigger question of when to pack up and move south. Experimentation in the world of hunters is akin to envisaging possible future scenarios for life in the far North—or elsewhere.

Returning to the chess analogy as a particular case of diagrammatic reasoning, I would like to suggest that the *valeur* (value) of the pieces on the ice board has become less readable within the logic of the environmental game. Let me explain this by referring to a board where two pawns of the same colour stand in the same column; we immediately know that one of them must have taken an opposing piece in a previous movement (Vendler 1967: 17). This must be so, given the rules of the game (entering into the diagrammatic logic), and it holds a priori, because the possibilities for movement enter into the very definition of the pawn. In principle one may detect the rules for movement by carefully watching a game, and once we have seen or experienced a sufficient number of moves or games, we will be able to identify the next "best" move, based on our empirical observations. It is quite a different task to investigate the a priori relations that obtain within the framework (ibid.: 21). This is what makes diagrammatic reasoning in general an instrument for deduction and mental experimentation beyond mere observation and spatial imageries. People must still find ways of moving forward, by establishing relations between observations that are consistent with real possibilities and hence iconic, but which are also theoretical in that they are configured from without.

With increasingly turbulent climate conditions, the rules become less rigorous and the spatial clues to the future less readable; when the anticipation of nature becomes more circumscribed, so does the future of hunting life in the far North. People will not starve in Greenland, and new possibilities may open up, literally, transforming the hunting community into something else. So far, however, the Northern hunters would rather continue being just that. The *valeur* of the sea-ice may change and a new beginning become possible.

In the eyes of earlier Western explorers and modern large-scale fishermen, the value of the sea-ice was always negative and prohibitive of progress (cf. Bravo 2010: 448). By contrast the ice was certainly not only positive, but constitutive of the Arctic world as seen from within. In 2007, several people told me that "in ten years, we are not here anymore", voicing their worst fear. Echoes of this are still heard sometimes, yet not consistently, because new avenues of living may (literally) open up. There is one rule in chess that cannot be deduced before the game is over, and that is the rule of checkmate, which may only be intimated or dreaded.

Insofar as the hunters continue to respond to an imagined future for the hunting community, we have to remind ourselves that checkmate is always a matter of definition in the human domain. "Abrupt climate change" may induce non-linear changes also in social life, yet such changes may actually also hold new promises. The most valuable general insight produced by anthropologists studying the impacts of climate change on particular societies is that people rarely define themselves as without some kind of future. Already now, some people in Northwest Greenland are beginning to mentally play with new possibilities: "If the sea-ice melts away completely, then maybe we can have a small harbour and real fishing vessels. We may even become the last port of call before the bigger ships go into the Northwest Passage or towards the North Pole", I was told in 2010. Although this still seems somewhat dreamy, it certainly testifies to the powers of assemblage with which the dreamers are gifted, incorporating the worst-case IPCC scenario, the emerging geopolitical reality of cartography in the polar region (Strandsbjerg 2010), as well as an extensive knowledge of other regions into a new diagrammatic logic of anticipation, still located, yet creating an entirely different, emergent knowledge space.

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REFERENCES

Aporta, C. 2002. "Life on the Ice: Understanding the Codes of a Changing Environment." Polar Record 38 (207), 341–54.

Aporta, C. 2010. "The Sea, the Land, the Coast, and the Winds: Understanding Inuit Sea Ice Use in Context", in Krupnik, Igor & Claudio Aporta, Shari Gearhead, Gita J. Laidler, & Lene K. Holm, eds. SIKU: Knowing Our Ice. New York: Springer Press (p.163–80).

Aporta, C. & E. Higgs 2005. "Satellite Culture: Global Positioning Systems, Inuit Wayfinding, and the Need for a New Account of Technology." Current Anthropology 46 (5): 729–53.

Basso, K. 1996. "Wisdom Sits in Places: Notes on a Western Apache Landscape", in S. Feld & K. Basso, eds. Senses of Place. Santa Fe: School of American Research.

Boas, F. 1964 [1888]. The Central Eskimo. Lincoln: The University of Nebraska Press.

Born, E.W., A. Heilmann, L. Kielsen Holm & K. Laidre 2008. Isbjørne i Nordvestgrønland: En interviewundersøgelse om fangst og klima. Nuuk: Grønlands Naturinstitut.

Bravo, M. 2010. "Epilogue: The Humanism of Sea-Ice", in Krupnik et al., eds. SIKU: Knowing Our Ice. New York: Springer Press (p.445–53).

Collignon, B. 2006. Knowing Places: The Inuinnait, Landscapes, and the Environment. Translated by L. Weber Müller-Wille. Canadian Circumpolar Institute: Circumpolar Research Series 10.

Deleuze, G. & F. Guattari 2004 [1987]. A Thousand Plateaus: Capitalism and Schizophrenia. Translated by Brian Massumi. London: Continuum.

Fortescue, M. 1988. "Eskimo Orientation Systems, Meddelelser om Grønland." Man and Society 11, 3–30.

Gilberg, R. 1986. "Polar Eskimo", in W. Sturtevant, ed. Handbook of North American Indians, vol. 5 (p.577–94).

Gildor, H. & E. Tziperman 2003. "Sea-Ice Switches and Abrupt Climate Change." Philosophical Transactions: Mathematical, Physical and Engineering Sciences 361 (1810), 1935–44.

Gulløv, H.C. 2000. "Østgrønlandsk kartografi og xylografi", in B.H. Jakoksen et al., eds. Topografisk Atlas Grønland. Copenhagen: Kort & Matrikelstyrelsen (p.170–71).

Hastrup, K. 2004. Action: Anthropology in the Company of Shakespeare. Copenhagen: Museum Tusculanum Press.

Hastrup, K. 2007. "Agency, Anticipation and Creativity", in E. Hallam & T. Ingold, eds. Creativity and Cultural Improvisation. Oxford: Berg (p.193–206).

Hastrup, K. 2009a. "Arctic Hunters: Climate Variability and Social Flexibility", in K. Hastrup, ed. The Question of Resilience: Social Responses to Climate Change. Copenhagen: The Royal Danish Academy of Sciences and Letters.

Hastrup, K. 2009b. "The Nomadic Landscape." Danish Journal of Geography 109 (2), 181–89.

Hastrup, K. 2010. "Emotional Topographies: The Sense of Place in the Far North", in J. Davies & D. Spencer, eds. Emotions in the Field: The Psychology and Anthropology of Field Experience. Stanford: Stanford University Press (p. 191–211).

Henshaw, A. 2009. "Sea Ice: The Sociocultural Dimensions of a Melting Environment in the Arctic", in S.A. Crate & M. Nuttall, eds. Anthropology and Climate Change: From Encounters to Actions. Walnut Creek, CA: Left Coast Press.

Hulme, M. 2003. "Abrupt Climate Change: Can Society Cope?" Philosophical Transactions: Mathematical, Physical and Engineering Sciences 361 (1810), 2001–21).

Hulme, M. 2010. "Learning to Live with Recreated Climates." Nature and Culture 5 (2), 117–22.

Huntington, H. & S. Fox 2005. "The Changing Arctic: Indigenous Perspectives", in Arctic Climate Impact Assessment. Cambridge: Cambridge University Press (p.51–98).

Ingold, T. 2000. The Perception of the Environment. London: Routledge.

Ingold, T. 2010. "Footprints through the Weatherworld: Walking, Breathing, Knowing", in The Objects of Evidence: Anthropological Approaches to the

Production of Knowledge, special issue, Journal of the Royal Anthropological Institute, 121–139.

James, W. 2003. The Ceremonial Animal: A New Portrait of Anthropology. Oxford: Oxford University Press.

Jones, H.G. 2004. "The Inuit as Geographers: The Case of Eenoolooapik." Études/Inuit/Studies 28 (2), 57–72.

Kermode, F. 2000. Shakespeare's Language. London: Allen Lane, the Penguin Press.

Ketner, K.L. & H. Putnam 1992. "Introduction: The Consequences of Mathematics", in C.S. Peirce, Reasoning and the Logic of Things, K.L. Ketner, ed. Cambridge, MA: Harvard University Press (p.1–54).

Kleivan, I. 1986. "De Grønlandske Stednavnes Vidnesbyrd om Vandringer og Forskeillige Aktiviteter", in H. Kleivan & K. Nørregaard, eds. Vort Sprog—Vor Kultur: Symposium I Nuuk 1981. Nuuk: Pilersuiffik (p.77–90).

Krupnik, I., C. Aporta, S. Gearhead, G.J. Laidler & L.K. Holm, eds. 2010. SIKU: Knowing Our Ice. Documenting Inuit Sea-Ice Knowledge and Use. New York: Springer Press.

Krupnik, I. & D. Jolly 2002. "The Earth Is Faster Now": Indigenous Observations of Arctic Environmental Change. Fairbanks: Arcus.

Laidler, G.J., P. Elee, T. Ikummaq, E. Joamie & C. Aporta 2010. "Mapping Inuit Sea Ice Knowledge, Use, and Change in Nunavut, Canada", in I. Krupnik, C. Aporta, S. Gearhead, G.J. Laidler, & L.K. Holm, eds. SIKU: Knowing Our Ice. New York: Springer Press.

Latour, B. 1999. "Circulating Reference: Sampling the Soil in the Amazon Forest", in Pandora's Hope: Essays on the Reality of Science Studies. Cambridge, MA: Harvard University Press (p.24–79).

Mauss, M. 1979 [1906]. Seasonal Variations of the Eskimo: A Study in Social Morphology. In collaboration with H. Beuchat. Translated by J.J. Fox. London: Routledge & Kegan Paul.

M'Donald, A. 1841. A Narrative of Some Passages in the History of Eenoolooapik, a Young Esquimaux, Who Was Brought to Britain in 1839, in the Ship "Neptune" of Aberdeen; An Account of the Discovery of Hogarth's Sound: Remarks on the Northern Whale Fishery, and Suggestions for Its Improvement, &c, &c. Edinburgh: Fraser & Co. and J. Hogg.

Moltke, H. & L. Mylius-Erichsen 1906. Grønland. Illustreret skildring af Den Danske Literære Grønlandsekspeditions Rejser i Melvillebugten og Ophold blandt Jordens Nordligst Boende Mennesker—Polareskimoerne, 1903–1904. København og Kristiania: Gyldendalske Boghandel/Nordisk Forlag.

Nuttall, M. 2009. "Living in a World of Movement: Human Resilience to Environmental Instability in Greenland", in S.A. Crate & M. Nuttall, eds. Anthropology and Climate Change: From Encounters to Actions. Walnut Creek, CA: Left Coast Press.

Ong, W. 1982. Orality and Literacy: The Technologizing of the World. London: Methuen.

Peirce, C.S. 1992. Reasoning and the Logic of Things. Edited by K.L. Ketner. Cambridge, MA: Harvard University Press.

Pickles, J. 2004. A History of Spaces: Cartographic Reason, Mapping and the Geo-Coded World. London: Routledge.

Putnam, H. 1992. "Comments on the Lectures", in C.S. Peirce, Reasoning and the Logic of Things, K.L. Ketner, ed. Cambridge, MA: Harvard University Press (p.55–102).

Rasmussen, K. 1929. "Intellectual Culture of the Iglulik Eskimos." Report of the Fifth Thule Expedition 7 (1). Copenhagen: Gyldendal.

Saussure, F. de 1974 [1916]. Course in General Linguistics. Translated by Wade Baskin. Introduction by Jonathan Culler. London: Fontana/Collins.

Sejersen, F. 2004. "Horizons of Sustainability in Greenland: Inuit Landscapes of Memory and Vision." Arctic Anthropology 41 (1), 71–89.
Stjernfelt, F. 2011. Diagrammatology: An Investigation on the Borderlines of Phenomenology, Ontology, and Semiotics. New York: Springer.
Strandsbjerg, J. 2010. Cartography and Geopolitics in the Arctic Region. Copenhagen: Danish Institute of International Studies, Working Paper 20.
Strathern, M. 1992. "Reproducing Anthropology", in S. Wallman, ed. Contemporary Futures. London: Routledge (p.172–89).
Turnbull, D. 2003. Masons, Tricksters and Cartographers. London: Routledge.
Vendler, Z. 1967. Linguistics in Philosophy. Ithaca, NY: Cornell University Press.

6 Deciding the Future in the Land of Snow

Tibet as an Arena for Conflicting Forms of Knowledge and Policy

Hildegard Diemberger

The ice and snow of mountain glaciers have been increasingly used to make projections about the future of the environment, with the idea of "darkening peaks" sounding almost as a sinister omen (see e.g. IPCC reports; Qiu 2008: 293–396; Orlove et al. 2008). As sources of "proxy data" (e.g. Mölg et al. 2008: 168) they have often taken centre stage in climate change debates, which raises a number of interesting questions that range beyond the domain of glaciologists and climatologists: what do mountains with their ice and snow cover represent in the collective imaginary, both at the local and the global level? Can they be seen as a "proxy" (Schaffer 2011) for the understanding of the natural environment within different knowledge systems and modes of communication? Can different interpretations of mountains, plants, and animals as indicators of wider natural processes relate to each other at the same time as reflecting different social and cultural frameworks? Which forms of knowledge matter when decisions concerning a changing environment are taken?

Having worked in the Himalayas, especially in the Tibet Autonomous Region, over two decades in various capacities, I came across many instances in which a highly symbolical place—usually a snow-mountain or a lake—was used to make statements about the well-being and the future of human communities and their environment at various scales: local, regional, or even global. It was remarkable how the sheer reference to highly significant places was able to mobilize collective feelings and narratives. I also remember how in the early 1990s rural people were flagging radical changes in their environment and in their weather pattern long before "climate change" (Tibetan *namshi gyurba*)[1] had become a popular topic there. In nomadic areas such as Porong I was told of better grass conditions in earlier times and of more timely rainfall as well as of specific events like a prolonged drought followed by an exceptional windstorm that covered the pastures in sand, with the grassland partially recovering only some years later thanks to a snow-rich winter.[2] Stories of exceptional droughts and snow disasters, greater thermal excursion between day and night, and unprecedented heat waves became later increasingly inscribed within narratives of "climate change". Lower snow-mountains that have recently lost their permanent snowfields such as Gyerpo Gang are often cited in Porong as indicators of the wider climatic trend. In the case of the "soul-mountain" (*lari*) of the neighbouring Gungthang, the observation of the recent loss of its permanent snowfields is sometimes linked to messianic narratives of the Age of Degeneration (Sanskrit: Kaliyuga), with evil seen as embodied by pollution, ruthless exploitation of natural resources, moral decline in human interaction, conflict, and so on. The exceptional drought of 2009, which forced the nomads to kill many of their animals, was spoken of among locals as a manifestation of this wider climatic process, as was the fact that the snow-mountains were unusually black. But are these phenomena actually the expression of a rapid change in the general weather pattern? If so, is this due to local or global factors? And how does it affect rangeland degradation? These remain highly debated issues demanding further research (see Harris 2010: 1–12 for a review of the relevant arguments). Local perceptions, whether consistent with the available weather and rangeland data or not,[3] do in any case matter and are immediately related to how livelihood is managed. In this chapter, looking at Tibetan pastoralists, I wish to explore the interface between local ways of anticipating the environment's behaviour and scientific models used to make projections about weather and climate. Do these forms of environmental knowledge belong to incommensurable scales or can they relate to each other? Is there a way out of the opposition between essentialized notions of traditional local knowledge (either doomed as backward or romanticized as indigenous wisdom) and modern universal scientific knowledge? Deteriorating environmental features, seen as an indicator of a potential unrecoverable future natural disaster, have often been at the centre of a blame game between local rural communities and state administrations on whether local herding and agricultural practices or industrial development are responsible for what people are experiencing. Beyond academic and global policy debates on what we know about climate, many decisions are currently taken on a day-to-day basis at the grass-root level to adapt to changing environmental conditions. The decision-making process itself is thus as important as the knowledge production that informs these decisions. In this chapter I am therefore also exploring the questions of what happens when specific decisions concerning the environment are taken by people who straddle a fine line across different forms of knowledge and of whether these can be related to historical precedents. More generally, I will look at how decision-making processes concerning specific environmental strategies, in which different forms of knowledge can be integrated or clash, can result in different social, political, and environmental outcomes that ultimately determine how specific communities are affected by and react to "climate change".

The geographer Andrei Florin Marin (2010: 162–76) has shown that in the case of Mongolian pastoralists, local observation of resource-dependent people can integrate climate modelling and weather records, which remain highly uncertain at the regional and local level.[4] Looking at the Tibetan case, I suggest that sacred mountains that have traditionally been used as indicators of the well-being of a certain landscape including its inhabitants are not only elements of a specific “moral climate” (Huber & Pedersen 1997) but can also reflect the state of the relevant micro-climate and its interface with the monsoon systems. One does not exclude the other, even though these dimensions cannot necessarily be articulated in terms of simple correspondence. Beyond the moral, religious, and even political connotations of the narratives in which they are embedded, these observations of snow-mountains can also make climatological sense and be relevant across knowledge regimes. On the basis of his fieldwork in Central Tibet, the meteorologist Hans-F Graf has explored the mechanism according to which rain and snow tend to concentrate on mountains with the relevant cloud formations potentially interacting with the moisture coming with the monsoon and inducing precipitation.[5] He also suggested that cloud formation and rain patterns in the Tibetan environment largely escape climate models and are often significantly affected by dew, which produces small but significant clouds, and is linked to vegetation cover and vulnerable to land use changes—especially overgrazing. Mobility in pastoral life—which reduces the impact on the pastureland—and the detrimental consequences of its loss (shown by David Sneath 2000, in the case of Mongolian herders) therefore are not only likely to affect the rangeland but also the local and the regional climate. In a system of beliefs that link landscape, weather, and livelihood, rules concerning the movements of herds, reflected in ancient documents or in current practices, can have both an ecological and a moral dimension linked to what is considered to be the best possible human interaction with the environment and what are the long-term goals of a community.

Recently one can see that this morality has been combined or contrasted with a new one coming from or associated with the Chinese state. This increasingly propagates “modern science” as the rationale underpinning policies that are implemented in rural areas to address the nation’s development and “ecological security” (Kang et al. 2005). The protection of the environment has thus acquired a new political and moral dimension through a range of campaigns. These are reflected, for example, in large posters at the road margins and near new settlements and express what Yeh has called a “green governmentality” (Yeh 2005: 9ff). Although the ways in which different forms of environmental knowledge can or cannot relate to each other are widely debated at an academic level, in practice these discourses are brought together in the lives of rural communities on a daily basis. This chapter suggests that different ways of understanding the environment can co-exist, both historically and now. It looks at the current opposition between so-called traditional knowledge and modern scientific knowledge as, in the Tibetan case, an element of a delicate ethno-political context within which competing claims to stewardship of the environment are often raised.

THE LAND OF SNOW AND TIBET’S “MORAL CLIMATE”

Since the beginning of Tibetan recorded history, features of the environment have been mentioned as indicators of human well-being and as distinctive features defining the lives of people living on the plateau. In early chronicles, dating from at least the ninth/tenth century, the Tibetan homeland is famously described as "fenced round by snow, the headland of all rivers, where the mountains are high and the land is pure".[6] Currently Tibet is widely referred to both in Tibetan and English as the "Land of Snow" (Gangjong). Tibetans are also known in their own language as the *gangchenpa*, literally, "people of the snow land", and the Buddhist rulers of imperial Tibet have also been known as the Dharmaraja of the snow land (*gangchen chogyal*). Mountains and adjacent lakes are often described in Tibetan texts and oral narratives as married couples, and the first mythological king of the Tibetans is said to have descended from heaven onto a sacred mountain like rain fertilising the soil.[7] These are just a few examples that show how ideas of nationality, culture, royalty, etc. often have been associated with specific geographic and climatic features.

Over centuries environmental features have been endowed with a whole range of such qualities that create what Huber and Pedersen define as Tibet's "moral climate", in which "dynamics of the social world were viewed as having a considerable effect upon the weather" (Huber & Pedersen 1997: 587). The local perspective, articulated within a more universal Buddhist cosmology, is, say the authors, "primarily concerned with respect for the gods of the world. It is conceived of morally in terms of binding relationships between localized human communities and non-human occupants of the same localities and entails mutual obligations" (Huber & Pedersen 1997: 585). They argue that weather conditions were "systematically linked to social life and correlated with a code for proper conduct. Nature and society were conceived to interact, thereby creating a 'moral climate' or, as we might say, a moral space" (Huber & Pedersen 1997: 588). Both past and future were, and to some extent still are, conceived within this moral space, where mountain gods, as lords of the land, control living conditions, including the weather. According to Huber and Pedersen, these underlying notions were central to narratives and ritual practices that anticipated the behaviour of the natural environment.

In my own experience in various Tibetan areas, I came across many manifestations of this way of relating to the environment, with observations of the ice- and snow-cover of mountains featuring very prominently. For example, in Thrika in Eastern Tibet the snow-line on the mountains is referred to as *gangdab*,[8] i.e. the hemline (*dabkha*) of the snow, like that of a traditional dress which has to be of a certain length, a notion associated with ideas of prestige and shame. In the Chomolangma/Everest region I heard songs that celebrate the environment as part of a universal order of things in which natural and social worlds merge:

> The honour of the snow mountains is their glaciers
> The honour of the lama is his books
> The honour of the man is his children
> The honour of the bride is her jewellery

And another:

High above are the snow mountains,
Where the snow-lion dwells according to the law of *karma*.
Over the pass a revolving takes place:
The great sun rises
The sun and the five-colored planets rise
May there not be any change, may prosperity prevail
May there not be any change, may good fortune prevail

Snow on the mountains is here an indicator of a stable and prosperous situation encompassing the natural and the social world, a situation in which the future behaviour of the environment can be anticipated with reasonable confidence on the basis of experience, like the movement of the celestial bodies. This is of course an aspirational vision and reality can often diverge considerably, as it is witnessed by various forms of divination and weather-making rituals that supplement local empirical knowledge in anticipating the future conditions of the natural environment. The gap between the aspiration towards optimal human interrelation with the natural environment and practice is also reflected in the evidence of ancient deforestation and degraded rangeland that have certainly had a negative impact on climate and human livelihood over a long period of time.[9] This pre-dated the second half of the last century which saw an unprecedented scale of environmental, social, and cultural change in a very vulnerable environment. Narratives of disastrous weather events such as "major snow calamities" (*gangkyon*) and prolonged droughts in historical records tell a story of variability and uncertainty to which human communities responded with greater or lesser success throughout their histories. In areas where human beings have been living at the margins of inhabitable conditions, there is a painful awareness of this variability. It leads to the deployment of strategies such as the careful observation of natural phenomena with the identification of a whole host of indicators (not only the ice and snow of glaciers but also lakes, plants, birds, etc.),[10] forms of collaboration and reciprocity that make it possible to absorb local variations in natural conditions, water management in terms of the construction of channels and reservoirs, and the creation of reserves to be redistributed in a crisis. In some cases one finds possible evidence of transitions in the economy (such as shifts from agriculture to herding and vice versa, changes in crops and herds, or an increased reliance on trade) and, as a last resort, the abandonment of certain areas.

Mountains with their ice and snow cover lend themselves to be seen as indicators that can differentiate between regular variability and more important changes in weather patterns. Narratives concerning famous mountains are therefore central to collective mobilization in the case of an emergency. These mountains, however, have characteristics that are different from those apprehended by scientists, and recall the glaciers in Athapaskan and Tlingit oral traditions described by Julia Cruikshank, which:

> attribute to glaciers characteristics rather different from those discovered through science . . . Glaciers are described in many narratives as characterized by sentience. They listen, pay attention, and are quick to take offense when humans demonstrate hubris or behave indiscreetly. (Cruikshank 2007: 365)

According to her such visions originate in intense engagement with the environment, creating what anthropologist Tim Ingold calls a "dwelling perspective" so profoundly relational that everyone understands how humans and nature co-produce the world they share (Ingold 2000: esp. 153–56; see also Basso 1996). She concludes that:

> Glacial landscapes described in oral traditions, then, are intensely social spaces that include relationships with nonhuman beings (like glaciers and features of landscape) which share characteristics of personhood. (Cruikshank 2007: 366)

Similarly, Tibetan holy mountains are social spaces at the centre of narratives about the environment that reflect also social behaviour within the community. In some cases, they refer to encounters with friendly or dangerous others, and even to competing claims about the environment. In Porong, for example, a few years ago an anemometer set up by Chinese scientists was destroyed by local nomads because it was considered responsible for winds that had that year kept the long-expected rain away. In the summer of 2010, a scientific expedition was unable to operate in the Namtso area because it was considered responsible for the late arrival of the monsoon.[11] These incidents are typically dismissed as examples of superstition or as reflections of an anti-scientific attitude per se, but to an anthropologist they speak of competing claims over the environment, within an ethnicized context and within a broader dispute over modernization. To put it more simply, the Chinese state has become an important player in local Tibetan discussions and experience through its administrative reforms, subsidies, new land use policies, and, sometimes, through relocation, the most radical and controversial of the measures in terms of social and cultural consequences.[12]

All of these are argued for in the name of modern scientific knowledge, and all are presented in contrast to backwardness. Emily Yeh (2007: 73) follows Huber in observing that representations of Tibetans as naturally "eco-friendly" only began to be produced after 1985, mainly among exiles writing in English, and observes that:

> Within China, the emergence of the Green Tibetan as an indigenous formation has been even more recent still. The state's official position on the Tibetan environment attributes all positive environmental stewardship to Chinese science and modernization, not Tibetan tradition;

> for example, “it was after the peaceful liberation of Tibet that ecological improvement and environmental protection started there and began to progress along with the modernization of Tibet” [White Paper on ecological improvement and environmental protection in Tibet. State Council, March 2003]. Tibetan self-representations of environmental stewardship have become possible only in the space created by China’s small but growing environmental movement, and in particular by Chinese staff of transnational conservation NGOs, as well as Chinese social scientists who have become interested in indigenous environmental knowledge.

Beyond these more or less recent claims to Tibetan environmental stewardship, there is a wealth of specific local knowledge about the environment that is not expressed as environmentalism but nonetheless has been shaping the relationship between human communities and environment for generations. This may be included or excluded when decisions are taken by local representatives and government officials at different levels. As it was stated in the blurb of the “Anticipating Nature” conference (see introduction to this volume), “nature” is always anticipated and climate has always been changing. The question is how much, how rapidly, in which terms, by whom, and on the basis of which knowledge coping strategies are deployed. The way in which uncertainty is managed is therefore likely to reflect the different, and sometimes competing, moralities and aspirations that inform ideas about the environment.

Figure 6.1 Yak herd grazing in Porong (photo by author).

THE CASE OF THE PEOPLE OF PORONG: TIBETAN PASTORALISTS IN A CHANGING ENVIRONMENT

Porong is currently a municipality (Ch. *xiang*) in Nyalam County (Shigatse Prefecture, Tibet Autonomous Region) inhabited by some 2,000 people who are largely pastoralists except for one village on the shore of the Pekhu lake that combines farming with animal husbandry. They inhabit a high-altitude plain (see Figure 6.1) at some 4,500–800 metres stretching between the Himalayan range (and Mt Shishapangma in particular) in the south and the hills that act as watershed in relation to the valley of the Tsangpo River to the north, the shore of the Pekhu Lake in the west and the headwaters of the Phumchu/Arun River in the east. Historically this was the centre for a wider nomadic principality that included several pastoral areas to the west and to the east of today's Porong municipality (Ch. *xiang*). This was dismantled following the failed Tibetan uprising of 1959 and the subsequent "democratic reforms" with many members of the traditional leadership escaping into exile. Whoever remained experienced the turmoil of the Cultural Revolution and a wide range of administrative reforms (for an overview see Bauer 2006: 24-47f). Since the 1980s this area, like the rest of the Tibet Autonomous Region, experienced the radical political change that enabled the reconstruction of a Tibetan cultural and social life within the framework of the minority nationalities (Ch. *minzu*) policy. I have followed this community since the early 1990s carrying out a variety of anthropological projects there. When I first came to this area it was rather isolated—the scattered settlements made up of recently built winter housing were reachable through a barely recognizable dirt track and had no electricity. Things have changed rapidly and now these houses are attached to the grid, many houses have TVs and there is mobile phone coverage almost everywhere. Rapid changes have also affected health and education as well as consumption patterns.

People have inhabited this area combining local knowledge linked to the management of pastoral life with two kinds of ideas coming from outside: modernist innovation, chosen or imposed, and revival of what is perceived as a traditional heritage that could be restored through recording the memories of the older generation and new access to ancient textual sources regarding the area and Tibet in general. Accordingly, the environment is perceived through different lenses by different people in the community, who rely to a varying degree on two very different forms of spatial understanding—the Chinese maps and the traditional sense of mapping that had been closely observed and harnessed in his compilation of GIS maps by Ken Bauer: "The nomadic sense of land is exceedingly topography-wise. The location of mountains, passes, ravines, good stands of grass, caves, rivers, springs, swamps are all closely observed and recited. Through their criss-cross patterns of daily walking or riding after animals, the herdsmen intimately know their landscape" (Bauer 2006: 31). Because by and large life remains marked by the movement of the animals between winter and summer pastures, the narrative of deteriorating environmental conditions seems pervasive and linked to very specific observations and daily challenges. Yet, ideas about causes and strategies may differ significantly. Porong is one of the many examples of rural communities perceiving "climate change" from the margins, both in terms of their own experiences of environmental changes and as a globalized phenomenon reaching them through the newly acquired TV.

According to a local saying, in the centre of the nomad land of Porong there is Takyong,[13] a mountain with a little snow on its summit. This snowfield never melts away. The elders of Porong used to say that as long as this snow is there, the land, the people, and the rulers of Porong will prosper. If this disappears it will be the end. As well as being part of a local theory of political and social stability linked to the environment, mountains like this have been important indicators of weather trends affecting the region. It is even possible to suggest that they have been politically effective because their "readings" entail enough empirical knowledge and observation to ground them in people's experience. It is interesting that the most important holy mountains of Tibet are located in the Trans Himalaya rather than the Himalaya, and in a climatological perspective their snow coverage reflects an important interface between the local climate and the monsoon system. I started to consider these holy mountains, known to us from the most ancient Tibetan records,[14] in a new light after seeing the graphs of Hans-F. Graf, a meteorologist who worked together with an interdisciplinary research team in the Namtsho (Nam Co) area, at the foot of Nyanchen Thanglha, one of the holiest mountains of Tibet.

Figure 6.2a Photograph of overshooting cumulus-nimbus above Mt Nyanchen Thanglha. (Courtesy of Hans-F. Graf.)

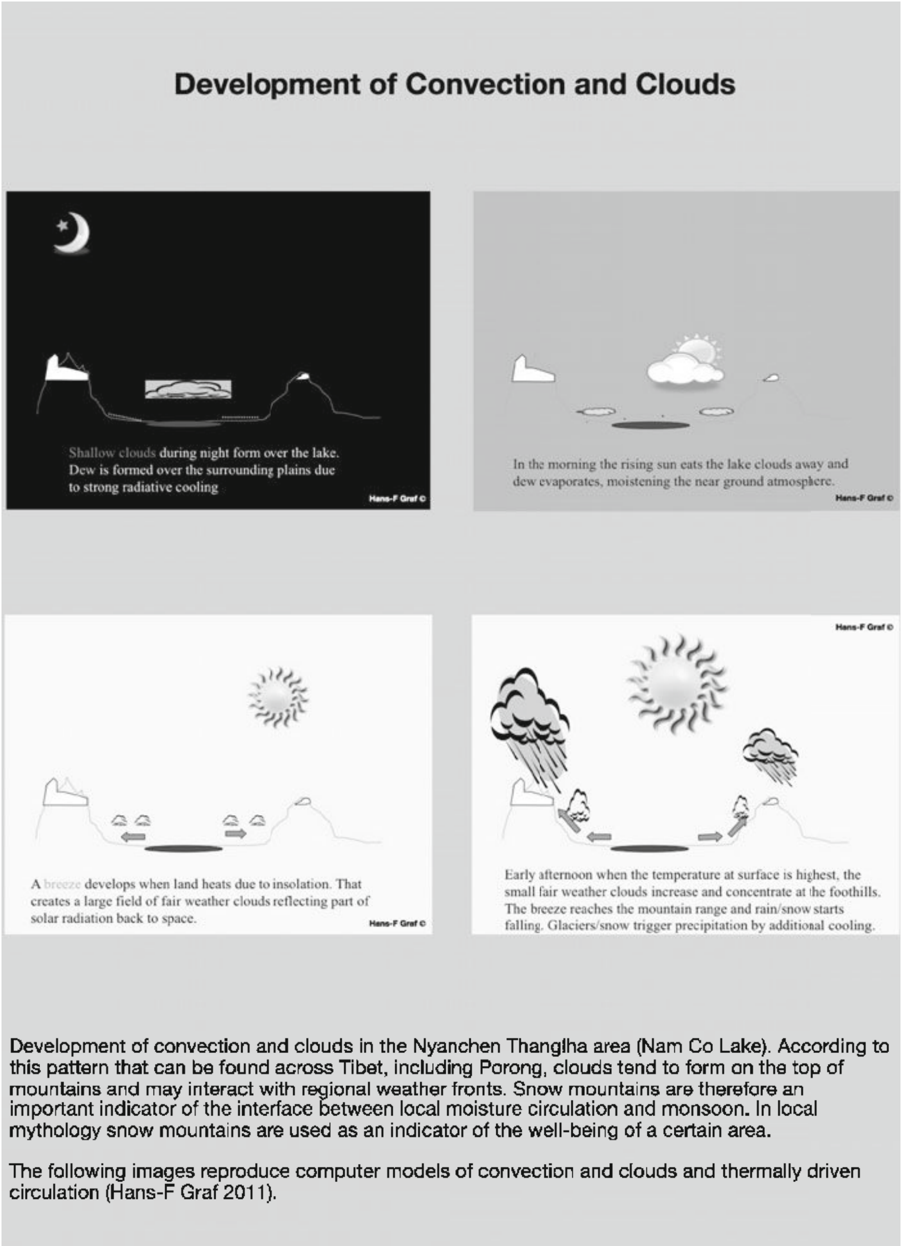

Figure 6.5b1 Development of convection and clouds, Tibet: image. The set of graphs included in the figure are based on an unpublished PhD thesis of Tobias Gerken, Centre for Atmospheric Studies, Cambridge (supervised by Hans-F. Graf). Reprinted with permission.

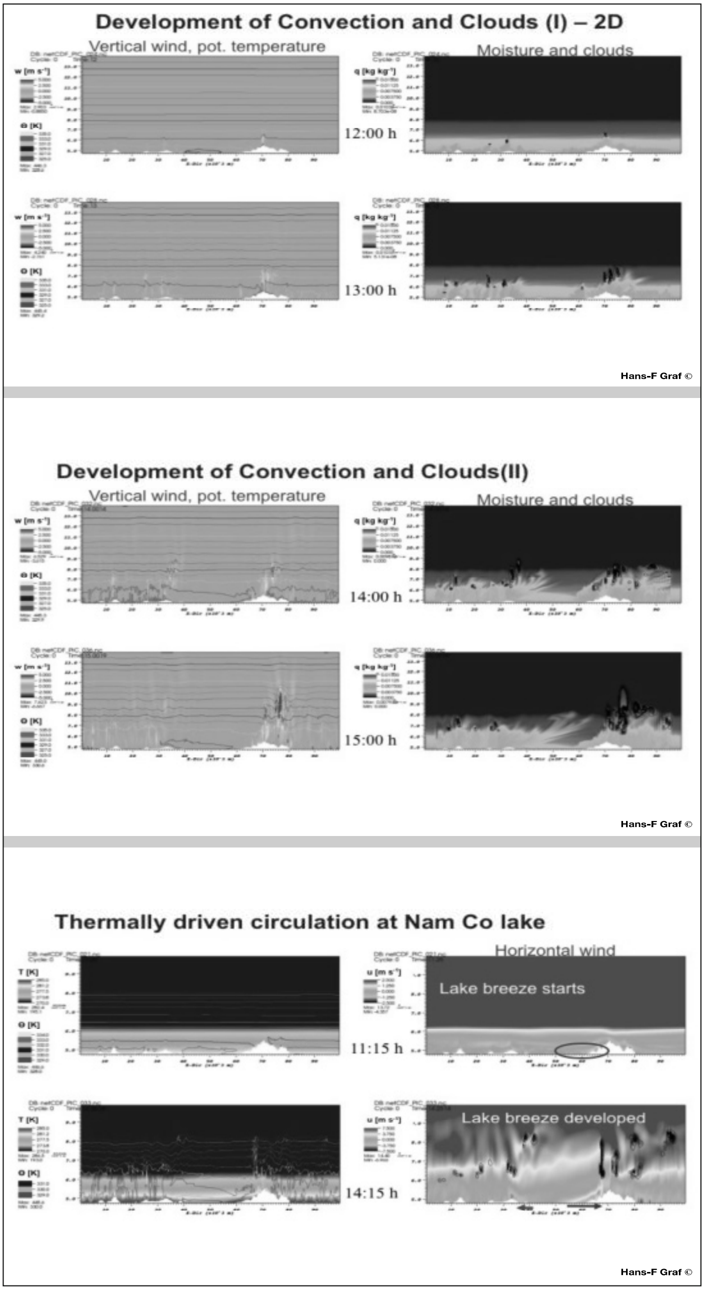

Figure 6.5b2 Development of convection and clouds, Tibet: graphs. The set of graphs included in the figure are based on an unpublished PhD thesis of Tobias Gerken, Centre for Atmospheric Studies, Cambridge (supervised by Hans-F. Graf). Reprinted with permission.

These figures show how the clouds gather in real time (Figure 6.2a), whereas the graphs detail how topographical elevation affects moisture circulation, cloud formation, and precipitation, with some of the clouds reaching up to the stratosphere (Figure 6.2b). The clouds interact with the moisture of the monsoon weather fronts that reach the area, leading to precipitation in the form of rain and snow. This creates the conditions for an interaction between the local recycling of moisture and the regional weather system. In order to capture the specificities of this topographical setting this team of scholars has tried different modelling techniques and concludes:

> It is demonstrated by this study that higher resolution than those used in GCM [Global Circulation Models] is important for a reasonable rainfall simulation on the TP [Tibetan Plateau], an extreme mountainous area. The double-nesting technique worked here and helped reduce the conflict of higher resolution and larger domain and decrease the influence of large biases on the target area of the driving dataset. (Cui et al. 2007: 52)

There are, however, still serious limitations regarding how far these techniques can be used in this environment:

> The quantitative evaluation of climate and land cover changes on the TP and their possible interactions is very difficult because reliable long term meteorological and land surface observational data are not available. A further complication is that the land–atmosphere interactions are highly nonlinear and the GCMs with current resolution only poorly perform on the TP. The few model studies published so far found also important hydrological implications of land cover changes on the TP on local climate and Asian area, where one third of world population lives. Further integrated investigations on the ecosystem on the TP are highly recommended to international scientific community. (Cui & Graf 2009: 58)

One of the factors that seems to play an important role in local moisture circulation (and weather systems) that so far has escaped climate modelling is dew, which is linked to local vegetation cover. This is currently being explored within an ongoing project (Atmosphere—Ecology—Glaciology Cluster within TiP—SPP 1372).

Cui, Graf, and others described how in Tibet changes in land use can influence local and remote climate. The phenomena narrated by local people may thus be linked to global climate changes as well as to local ones linked to the local anthropogenic impact on the environment. As the latter argument can be used to transform marginal communities of herders from victims into perpetrators of climate change (see also Harris 2010: 1ff), capturing how these dimensions articulate remains an open scientific and political challenge:

> Anthropogenic land use changes (LUC) on the Tibetan Plateau (TP) influence the local and remote climate. Specifically, the TP is warmer (0.17 °C) and drier (–9 mm/year) than it would be without anthropogenic LUC. The TP has been observed to warm over recent decades, corresponding with a concurrent human-induced LUC towards urban and desertified areas. This trend towards warming is shown in our model experiments . . . Cui et al. (2004) concluded that a regional model might improve the local climate simulation on the TP than GCMs. Such nested model system may help not only to specify the local response to LUC but also transfer or downscale the information from global models. Generally very high resolution or locally adjusted grids will be necessary to fully account for the effects of TP ecological modifications on large scale and global climate. (Cui et al. 2006: 53–54)

The anthropogenic impact on the Tibetan climate is undeniable but it is highly variable in relation to areas and pastoral (but also agricultural and industrial) practices and both hazards and environmental resilience prove difficult to assess. A possible productive integration of local knowledge and experiences in a comparable environment has been suggested by Andrei Florin Marin concerning Mongolia:

> I propose that the cumulative evidence from Mongolia's desert steppe indicates the existence of a possible positive feedback mechanism between precipitation and vegetation, mediated via local characteristics. Thus, if the larger scale moisture supplied by the East Asian Monsoon is indeed decreasing due to the southern shift of the rain belt (Yu et al., 2004), precipitations in southern Mongolia depend to a large extent on local recycled moisture. This recycling process is influenced by local surface heating and winds, in turn connected to vegetation cover. Because vegetation cover depends on the amounts and timing of precipitation, the feedback loop becomes evident. This positive feedback is not explicitly argued for by the herders but it is often suggested by the observations and reflections like the following: "If we have good grass, there's no sand, and no winds. And that keeps the clouds above this place and so it rains." (Munkhnasan, January 21st 2007). The mechanism has also been proposed by Xue (1996) based on simulations of the climate changes in the region. (Marin 2010: 173–74)

More generally he observes: "Sources of uncertainty in climate modelling have been related to unreliability and incompleteness . . . By the nature of their lifestyle pastoralists are in the position to gather environmental information (including climatic) over much larger areas and with much more detail than conventional meteorological stations" (Marin 2010: 174). According to his study the herder's evidence of change is partly supported by meteorological records and larger scale predictions and models. They provide also observations that refer to qualities and spatial scales not covered by instrumental measurements but that are essential for their livelihood.

Different pastoral practices may have a different impact on the pastures and therefore also on local weather patterns. David Sneath, looking at pastoral economies in three bordering Inner Asian countries, has shown how different policies in similar ecological settings can have a very different impact on the environment and, specifically, on pasture degradation (Sneath 1998: 1147–48). Building on rangeland non-equilibrial theory, he demonstrates that mobility can be crucial, and often more important than carrying capacity, for a sustainable form of pastoralism, and the loss of it detrimental.[15] In Inner Mongolia communal structures implying a highly mobile pastoral system survived, reframed within the Communist communes system, until the reforms of the 1980s. At that point, this was broken up with a significant impact on the conditions of the grassland (Sneath 2000).

Not only mobility, but also other features of pastoralism have changed, to a varying degree in different areas, over the last three decades, moving from what was largely a subsistence economy to a system more geared towards production for the market. This has been strongly promoted by the government and has had an impact on the numbers of the animals and the composition of the herds. Porong, which has been less affected than other areas by these radical changes, has remained largely a subsistence economy even though it became moderately involved in cashmere wool production (the quality from this area is not very good) and saw an increase in sheep and goats to be sold to Nepal for the Dasain festival. Yaks, which were very important in the traditional economy, are not only considered to be less profitable but also less resilient to degraded environmental conditions (which affect among other things female fertility)—but they are also less extreme "grazers". This changed the composition of the livestock: a rise in sheep and goats and a decline in yaks and horses with a consequent impact on the environment.

More importantly, pastoral strategies have tended to be oriented towards short-term benefits rather than long-term interests, affecting the overall management of the environmental resources significantly (see also Harris 2010: 8).

SPACES AND TIMES OF PASTORAL LIFE: NEGOTIATING STRATEGIES BETWEEN THE PREDICTABLE CYCLES OF THE HEAVENLY BODIES AND WEATHER UNCERTAINTY

In the 1980s I carried out fieldwork for my PhD among transhumant pastoralists who live to the east of Mt Everest (further downstream the Pumchu/Arun River, within Nepalese territory, at some 100 kilometres from Porong). I remember how the ritual and agricultural calendar marked the times of this community that inhabited a very steep landscape and whose movements ranged from the 2,000 metres of altitude of the winter pastures, to the 2,200/2,300 of the villages, to the 5,000 metres of the summer pastures. The summer pastures were located at the core of the so-called Hidden Valley (Beyul) of Khenbalung and there were specific rituals to open and close the gates of this sacred landscape. The times of departure and return from the summer pastures were decided through a careful interplay of star observation (in recent times supplemented with the Nepalese calendar) and a grass and bird calendar that helped determine the season (e.g. spring could come early or late, etc.), thus detecting the right moment for the move. In that particular setting getting the time right made a huge difference, for the herders had to cross a high pass before getting into or coming out of the Hidden Valley and sudden snowfall could lock people into life-threatening predicaments. Going into the Hidden Valley before the gates were opened or after the gates were closed was an offense that could enrage the lords of the land (*sadag*, *shibdag*). When I arrived there, this system had recently been abandoned in favour of the still-limited but gradually developing mountaineering and trekking trade, which implied obvious transgressions to this rule. This was something that was strongly criticized by some of the community elders who saw in the increasingly unpredictable weather patterns an outcome of the fact that people disregarded this rule (as well as many other traditional rules concerning woods and pastures), whereas the younger generations welcomed the new opportunities without worrying too much about the weather.

The people of Porong moved—and still move—with their herds between lower winter pastures and higher summer pastures in a similar way even though the difference in altitude is not so extreme. Deciding the times of movement is about finding the right balance between how long the winter pastures can endure the pressure of the herds and the movement to the more distant summer pastures that have better grass but are more exposed to extreme weather at the foot of mountain peaks (7,000 and 8,000 metres high) located to the south. The ancient system was dismantled when the "democratic reforms" of 1960 changed the entire administrative setting of Tibet, but some of its features were retained.

An 1884 document (see Figure 6.3) concerning pasture management in Porong outlines in remarkable detail the times and spaces of pastoral life. It has the title "The virtuous list of places of the [Porong] units"[16] and begins as follows: "In the year of the wood-monkey, when the planets and the stars auspiciously gathered, the list of the places of each Porong unit was recorded. . ." (Anonymous 1884, folio 1). It then gives a detailed description of the territory of the various units with their boundaries and an enumeration of the places and the timing of herd movements in relation to season. It sets out a pattern of extreme mobility prescribing the movement among specific pastures and also the movement between pastures and salt lakes where the animals would be able to get salt after having had a long period

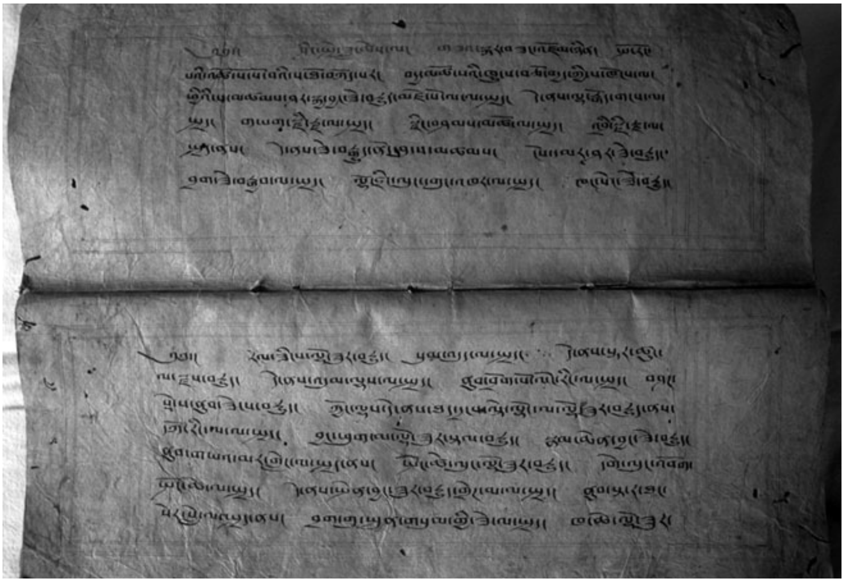

Figure 6.3 Tibetan document listing the Porong pastures (photo by the author).

of snow-melt water in the high pastures. Timing and routes were laid out to ensure that the animals would not damage the pastures that they were crossing by trampling on the grass. The borders between the different communal lands were porous, allowing for transit, and a system of reciprocal use agreements made it possible to respond to relative disadvantage linked to weather variability. A copy of this document preserved by the Porong exile community in Kathmandu became accessible to the Porong people in Tibet in the late 1990s. Comparing the mapping of the Porong territory reflected in the document to the current distribution of settlement and pastures, Porong people recognized many of the places and noticed that the old structure of numerous small encampments had been reduced to fewer settlements that were larger and more permanent (this is also reflected in Bauer 2006: 24–47). Gaining access to this historical document prompted a lot of reflection among senior members of the Porong community. It raised questions concerning the extent to which the worsening of the pastures they were experiencing was due to global climate change, increase in animals, a change in the composition of herds, or changes in pastoral practices that followed the break-down of the ancient system. Addressing each of these factors would have demanded different strategies. Thinking about the past had prompted a lot of thought about the future.

Meanwhile the government was promoting the construction of fences to protect pastures with mixed success (see Bauer 2005: 53–79) while local people were experimenting with the cultivation of barley as fodder, the sealing off of a series of very small plots of land to test grass behaviour under different conditions (non-grazing, irrigating, planting of grass) and even conceived ambitious ideas of pasture irrigation systems making use of the streams coming from the glaciers to the south of Porong (which were never realized due to the investment required).[17] Beyond the shared narrative that there was a problem with the pastures, I found a whole range of different ideas and combinations of ideas, with some people more proactive and others more resigned to a condition of increasing dependency on governmental subsidies. I thus realized that the question of the relationship between different forms of knowledge had to be seen in the specific context of decision-making: at times the same people who were enthusiast researchers of ancient practices were the most proactive in trying out new solutions calling upon scientific knowledge; at other times there was a clear-cut opposition between what was seen as traditional versus what was seen as modern, or specific to herding versus farming or rural versus urban or Tibetan versus Han Chinese. Statements about the environment and the relevant strategies could then be seen as highly political and treated as such.

DECISION-MAKING ON THE BASIS OF DIFFERENT FORMS OF KNOWLEDGE: HISTORICAL PRECEDENTS AND CURRENT DILEMMAS

The question of how different forms of knowledge about the environment can relate to each other has recently been at the centre of many academic and popular debates. "Climate change" seems to have brought a new sense of urgency to the issue and in fact terms like "interdisciplinary" and "cross-disciplinary" often appear along the relevant initiatives. One of the reasons for this is that decisions need to be taken and questions are being raised about the forms of knowledge informing these decisions and how this knowledge is used.

The urgency and the scale of the problem confronting Tibetan rural communities might be unprecedented, decision-making about environmental resources involving different groups of people and different forms of knowledge not necessarily. I wish here to describe in some detail the decision-making process concerning the construction of water channels in Porong, the nomadic area mentioned above, as it took place in the middle of the 15th century. The setting is of course very different from the one confronting today's Porong community but the cultural mechanism deployed on that occasion to decide on a strategy can be revealing and refer to a constant in Tibetan civilization: the management of water. In addition, according to current research in Tibet's climate history the events that I am going to describe have to be seen against the background of the mega-droughts that had affected the region for several decades in the 14th century and had forced people to abandon certain areas and relocate some of their settlements (Sinha et al. 2010: 1–16). The biggest challenge was then, as it is now, the building of a consensus around a strategy.

Figure 6.4 Chokyi Dronma as depicted in a mural painting in Nyemo, Central Tibet (photo by the author).

The story is told in the biography of Chokyi Dronma (1422–55), a Tibetan princess who gave up worldly life to become a nun and was eventually recognized as the incarnation of a tantric deity (see Figure 6.4) (for a study of her life see Diemberger 2007). She was a popular albeit controversial leader in her time, promoting cultural and social innovation that ranged from the building of iron-chain bridges to printing and the education of women. In her biography she is described as a heroine within Tibet's "moral climate", ending droughts and bringing rain to the areas she visited. Whereas some parts of the biography celebrate her as a heroine of wonderful deeds and recall the causal link commonly inferred, in hindsight, in popular narratives among all that is good (and all that is bad), other passages sound much less formulaic and give a different view of the social and cultural process she was involved in. One of the aspirations Chokyi Dronma is mentioned as having pursued was that of promoting better water management to expand the areas that could be cultivated, fostering thereby the spread of Buddhism so that peace among communities could be maintained. She came from an area with a mixed economy, largely living on farming supplemented by trade and some animal husbandry, in which sophisticated irrigation systems enabled high altitude agriculture. The area of Porong in which she had decided to act out her vision was largely inhabited by nomadic pastoralists but had traces of ancient agriculture, presumably linked to an earlier period of milder climate.[18] As the work started:

> She first went to Ganden where new channels were being made and gave instructions about the construction on the basis of those that had been given previously by the Omniscient Great Lord Chogle Namgyal. She considered that if the course of the channels went straight, next to the locality called Nyaphar, these were easy to build and would be reliable in the future. Then she discovered the remains of four or three fields, which had been cultivated by people in ancient times. (Biography of Chokyi Dronma: 108b–109a)

The channels that she was building would take water from a spring above the monastery of Pemo Choding and would take it beyond a saddle to a slope descending towards the shore of the Pekhu Lake. Fields would have therefore benefitted from the climate-mitigating influence of the lake. Having relied on the previous experience of those who had already tried to build a water channel in that locality under the guidance of her late master, the technical instructions she had received from him, and her own reasoning about construction, maintenance, and steadiness of the water source, she was pleased to discover that ancient people had already developed agriculture in that place. She thus thought that a historical legacy was confirming her plan. She was conscious that the construction was only one part of the plan and that there were more questions about sustainability and maintenance of the project. She needed to have a wider consensus within a community that respected her but, to some extent, saw her as an outsider coming from the agricultural areas; and being a woman did not make things easier. She thus decided to go for divination:

> Let's ask the dough balls [for divination] in front of the statue of the Great Lama [Chogle Namgyal]. As it is necessary that the entire monastic community, undivided, prays together, let's all go in front of his statue. (Biography of Chokyi Dronma: 109a–109b)

After drawing one of the dough balls, she announced the positive verdict. This process, which added to her more empirical reasoning, helped her rally the community behind her plan: the late Chogle Namgyal had expressed his support and conferred authority to her vision, dispelling general doubts. She could thus look after the material aspects of the project:

> She covered most of the expenses for the construction materials. The people belonging to Choding provided most of the labour . . . [She thought]: This will benefit greatly not only those who live on the Pelthang plain but will make this the best place of refuge for the people coming from India, Nepal and so on who travel in Tibet and face great hardship and unbearable anxiety. In particular a great seat for the monastic community could be established. In ancient times people used to say that . . . it was good to build on the ruins of former buildings . . . the existing surrounding fields would produce large amounts of crops and numerous senior monks from many places and their followers would gather there. In the same way scholars from Southern and Northern Lato and Ngari[19] should gather here to study and practice the precious teachings of Chogle Namgyal and spread Buddha's doctrine. Thanks to these good deeds this great land can remain peaceful. (Biography of Chokyi Dronma: 109b–110b)

Despite her efforts at rallying the community behind her, there were misgivings and these gradually emerged: "The people of Choding showed changeable views, speaking like the tongue of a snake . . . Thinking that she would not fulfil her aspirations and having lost all her hopes, she said: 'Let's leave for the east the day after tomorrow'" (Biography of Chokyi Dronma: 110b–111a). As an intellectual from an agricultural and trading area, she had tried to combine her own experience and aspirations with that of nomadic pastoralists, bringing together different forms of empirical knowledge about the environment, different technologies, and Buddhist thought and morality. At that time she failed. She left Porong for good, but her legacy remained and the channels were eventually built and the remains can still be seen today. The spring, however, has recently dried up.

Tibetan biographies have often a strong hagiographical character and have to be read with a critical eye. What is remarkable in this case—probably due to the closeness of the events to the writing process—is that this story of tension between Tibetan people of different cultural background (nomads and farmers, common people and elite) who were taking decisions concerning resources management seems rather plausible and that the story actually ends in a failure (which makes it historically more credible and less hagiographic than other narratives). More generally, this story can give some insight into negotiation processes that combine different forms of environmental knowledge and different strategies, and to some extent constitutes a precedent for the challenges faced today by Tibetan rural communities as they need to manage their natural resources. Most importantly this narrative highlights the question of authoritative knowledge (based on direct experience, local tradition, wider Buddhist and/or scientific worldviews) that informs the way in which people manage knowns and unknowns when making decisions and implementing actions. Then as now, the management of uncertainty is deeply intertwined with questions of authority and social acceptance, aspirations and reality assessment.

Byg and Salick (2009: 156–66) working among Tibetans in Yunnan noticed how different rural communities perceive and respond to climate change differently in relation to their specific environment (e.g. higher or lower altitude), economy (farmers/semi-nomads/nomads), and social and political setting. Higher altitude communities can harness some benefit from the warming such as pushing up the limit of agriculture while a wide range of specific problems can be tackled in different ways at different altitudes. In any case, a whole range of decisions needs to be taken at the grass-root level when responding to local challenges and opportunities and when implementing governmental policies. These processes involve in different ways the rural communities, their representatives, the different levels of the Chinese administration embodied by a range of Chinese and Tibetan cadres as well as scientists that may act as consultants or even NGOs. On the one hand, scientific knowledge has been referred to as the main rationale for both environmental protection and economic development against backward local practices often identified with subsistence-based livelihood strategies (Wang & Bai 1990).[20] On the other hand, local knowledge may inform to a larger or lesser degree the actual decision-making process through the background of the people involved. Ken Bauer observed:

> Conversations with residents of Porong revealed that certain individuals, particularly the literate and lineal members of Porong's various encampments, played a key role in bridging administrations and helped to maintain the status quo in terms of resources availability through their multiple roles as enforcers, advocates, and interpreters of community property boundaries. Even at the ideological height of state intervention in Tibet, township and county cadres relied on intermediates to help them govern, especially in relation to administering natural resources. (Bauer 2006: 40)

These same people were also important in the revival of local religious and artistic traditions and were often seen as the advocates of a Porong way of life. The intimate link between human communities and environment, reflected in a complex system of empirical knowledge and religious beliefs in the so-called “old society” (*chidzog nyingba*), proved to be resilient so that some of its elements have re-emerged in the “new society” (*chidzog sarpa*) after the 1978 policy shift.[21] Among farmers not far from Porong, in 1993, I even came across a party secretary who was leading a procession to bless the fields in order to protect them from hail. He was not an isolated case. Many local leaders felt that they had to fulfil the ritual obligations expected of traditional headmen. The relationship to the environment had to be managed, both empirically and ritually.

Cadres that belonged to the communities themselves, not extraneous to the local customs and able to move skilfully across the newly defined boundary-lines between “old” and “new” society, as well as officials sent from outside who were prepared to comply with the demands of local communities, made a whole range of local informal arrangements possible. The 1994 enforcement of tighter policies on religion, cadres’ appointment and behaviour, and the pace of modernization (see also Barnett 1996: 25; Diemberger 2010: 113ff) made many practical arrangements that spanned different forms of understanding the environment increasingly unviable. Although progress was made in terms of economic development, education, and health with more educated cadres taking responsibility over the management of rural areas, something seemed to become increasingly marginalized: local skill. By skill I refer here to the definition given by Tim Ingold as inherently linked to a “‘dwelling perspective’, i.e. [not] techniques of the body but the capabilities of action and perception of the whole organic body (indissolubly mind and body) situated in a richly structured environment” (Ingold 2000: 5). What is at risk of being lost when local people are not actively involved in the decision-making process about the environment and just act out what has been decided elsewhere is their experience and what they have learnt from people who preceded them, a “practical knowledge about survival and livelihood skills like herding, trade, and animal husbandry [which] are absorbed in doing, watching, and living a particular way of life” (Bauer 2006: 32). This includes the ability of reading mountains, lakes, clouds, grasses, etc. monitoring the conditions of the environment at the micro-level.

In 1997 in an area that used to belong to the Porong principality and is now to the west of the Porong municipality I happened to be witness to a lively meeting in which a commission was trying to carry out a new land survey which should have converted the traditional system of pasture measurement into *mu* (i.e. a Chinese unit of measure corresponding to 666.6 square metres) and reallocated pastures. Apparently it was very difficult to translate a system based on herds that could be sustained (*khyusa*)[22] into a universal system of measure and no agreement could be found on the reallocation of the pastures. I was then told that the commission gave up trying to find an agreement. They did fill in the papers in some way but it was said that people could carry on doing what they had always done. This was fine, for the moment—but only until new cadres came in and tried to enforce what was written on paper. This is an emblematic anecdote recalling the fact that one of the biggest challenges remains how to bridge the gap between very different systems of understanding the environment and that this is not necessarily just a question for academics and researchers.

CONCLUSION

Tibetans, like many "indigenous" peoples, have often been seen as holders of an ancient ecological knowledge. This perspective has proved to be problematic. Addressing, critically, recent claims by Tibetan exiles to traditional ecological wisdom, Huber and Pedersen suggest that these "are anachronistic in that they are projections of ideas of nature which belong to a modern knowledge tradition unknown to the ancient Tibetans" (1997: 577) and, because these involve radically different premises, conclude that "modern knowledge constitutes [the environment] as an ecological space, whereas in traditional Tibetan knowledge it is a moral space" (Huber & Pedersen 1997: 588–89). Although I agree that there has been a lot of reading of current environmental agendas into Tibetan understandings of the environment, I find the opposition problematic as it neglects the moral and political dimension of the first and the empirical knowledge of the second, both of which can, in some cases, be translated across different modes of knowing the environment.

In this chapter I have argued, both from ancient documents and modern parallels, that adapting to local environments often needs to be based on a synthesis of different forms of knowledge and ways of anticipating nature. The issue "climate change" is a new challenge for the rapidity and the scale of the transformations involved but it can be seen in a continuum with what Tibetan communities experienced before. The abandonment of certain areas and resettlement were not unheard of historically but they usually were a last resort. Now they have become part of a wider, radical strategy. Ecological relocations (ER) have become an increasingly widespread phenomenon affecting the Mongolian and Tibetan areas of China to a varying degree. Promoted in the name of environmental protection and sometimes as measures to tackle factors that exacerbate the effects of climate change they often come at a high social and cultural cost for the people involved and also for the government when they end up in protests (especially when they fuel ethnic tensions). Some critical reflection has also been voiced by scholars in China. Du Fachun of the Chinese Academy of Social Sciences states in the conclusion of his 18 December 2009 presentation:[23]

> Tibetan eco-migrants are marginalized . . . Clearly, the relocation of Tibetan herders from their traditional lands raises several key issues. In particular, the ecological rationale for this policy and the implications

> for those being resettled require careful examination. ER policy needs to be improved. Future Research Proposed: . . . not relocating herders but to achieve the balance between the grassland, animals and people? (Du 2009)

This kind of approach, reflexive and constructively critical of current policies, could open up new perspectives for an engagement with local knowledge understood not only as a potential source of long-term data but also as the potential basis for a combination of different approaches to environmental stewardship. One of the solutions could in fact lie in harnessing and building on the agency of local people who have, or are prepared to research, skills deployed by earlier generations in managing specific and vulnerable environments (with more or less success) and combine them with other forms of knowledge coming from a rapidly evolving climate science. Centrally formulated policies would in this case be implemented, taking local circumstances into serious consideration; the environment would be read with different lenses that could help validate each other; resettlement would possibly remain a last resort as it was in the past rather than a generalized strategy; measures of restraint in the exploitation of natural resources would be perceived less as an imposition and more as a jointly concerted strategy.

More generally, by looking at natural phenomena in their social and cultural context, scientific investigation could discover and harness a host of unusual "proxies" that reflect long-term human observation and engagement with specific environments. Mountains and lakes, ice and snow, clouds and dew, birds and grasses can tell stories that may relate to the findings of natural scientists, talking across knowledge regimes. Looking at the complexities of how "climate change" is handled at the local level may also help recognize the cultural and political nature of the process through which decisions about the environment have always been taken in human history, including those that are informed by scientific knowledge. In particular, looking at how peoples have been operating across and in relation to the gap between their visions and reality can be the key to looking at natural phenomena in their social and cultural contexts and make the knowledge and policy scales commensurable. The multiplicities and uncertainties of scientific research could then be better taken into account when science is expected to translate into policy in the Himalaya and elsewhere. At the same time the hazards entailed in letting numbers and mathematical models work within modern myths that provide all kinds of moral and political answers would be better recognized. As Mike Hulme (2009) suggested, "climate change" can offer a real opportunity to reflect about the values, sense of identity, and purpose that inform our approaches to "the problem" from different vantage points. The way in which ice and snow in the Himalaya, the Arctic, and other significant places have been used and abused in climate change discourse could thus be seen as an opportunity to reflect on the "entangled narratives" (in the sense given by Julie Cruikshank to this term) that inform the tortuous process through which climate science is called upon to inform policy at an unprecedented pace and scale, cutting across different scientific disciplines, cultures, and political agendas.

NOTES

1. Except where otherwise specified, vernacular terms mentioned in this paper are Tibetan.
2. Some 300 kilometres southwest of Tibet's second largest city, Shigatse.
3. Data remain scanty in an area characterized by extreme local variability. Also, average rainfall data do not actually tell how this is distributed locally in time and space, the level of intensity, the specific rain patterns that can have a very different impact on the conditions of the pastures. I was also told by a nomad from Porong that even if phenomena like the 'big snow calamities' have been recurrent throughout Tibetan history, what makes a real difference is whether this follows a period of drought, for the weakened animals are much less likely to survive and thus the impact is much greater.
4. Similarly Magistro and Roncoli (2001: 91–96) have suggested an integration of anthropological research with its localized scales of analysis with global modelling exercises. I believe, however, that what is outlined by Marin is a more articulated and detailed form of integration of different forms and scales of knowledge.
5. His view was presented in detail at a seminar at the Mongolia and Inner Asia Studies Unit in February 2010, and is also reflected in several articles that he co-authored, e.g. Cui et al. 2006: 33–56; see also Graf et al. 2011 for his most recent work on the Atmosphere Ecology Glaciology Cluster.
6. This is one of the most famous descriptions of Tibet found in the Dunhuang documents (ninth/tenth century), which is also evoked more concisely by popular epithet such as "high peaks, pure land" (See Bacot et al. 1940:86).
7. See for example Bacot et al. (1940: 86). Even though the details of this translation are debated the overall cosmological meaning is clear.
8. Tsering Thar, professor of Tibetology at the Minority Nationalities University native from Thrika (Qinghai), personal communication.
9. The destruction of forests peaked in the years 1959–76, whereas junipers used generally to be protected as holy before. However, it is clear that human activities have had a significant impact on the vegetation leading to the current degraded commons at least for the last 600 years (see for example Miehe et al. 2008: 171).
10. See e.g. Hobbs (2011) for a study of the "milkbird" among Amdo Tibetan nomads that acts as a proxy indicating stable/changing environmental conditions.
11. Hans-F. Graf, personal communication.
12. These policies have been considered critically for their social consequences also by some scholars and journalists in China (See for example Feng 2008; Du 2009).
13. This is one of the main protectors of the ancient nomad principality of Porong. The ruler of Porong together with a ritual specialist called Aya used to sacrifice a white sheep to this mountain once a year, just after the New Year celebration. The ritual specialist used them to make predictions about the weather and the health of the community by reading the entrails of the animal (see Diemberger & Hazod 1997).

14. These are listed in the Dunhuang documents, in early post-dynastic chronicles such as the *dBa' bzhed* and the *lDe'u chos 'byung*, in later ritual texts and oral traditions. Particularly interesting is the set of three brothers: Tise (Kailash), Chang Targo, and Nyanchen Thanglha.
15. Looking at bordering regions in China, Russia, and Mongolia, David Sneath shows that areas that have retained mobile pastoralism have much lower levels of reported pastoral degradation than those that introduced static, highly mechanized agro-industrial techniques.
16. This is glossed as *Porong Boundary Survey* by Ken Bauer (2006: 27) who discusses the mapping and the pastoral practices reflected in this document. They used to be composed of eight sub-units called *tsho* or, more formally, *gyatsho*.
17. There are actually a couple of channels to the Southwest of Porong that have been irrigating small plots of grazing land on what used to be an important route for trade and pastoral movements. These are ancient, and worked as a kind of 'service station' for passing traders and nomads. Extensive irrigation of pastures has so far proved to be uneconomical.
18. See also Sinha et al. (2010: 1–16) for a discussion of important climate variations in Asia in the 14th century.
19. This indicates the three brodering polities located to the west, east, and north of Porong.
20. This is something that has recently been extensively discussed and criticized (Fisher 2008).
21. In 1978 Deng Xiaoping inaugurated a new political course of action, which, among other things, enabled the rehabilitation of people who were condemned or demoted during the Cultural Revolution and the revival of Tibetan traditions. "Traditional" features of pastoral economy and social life have persisted or re-emerged in different forms in many areas; see also Golstein and Beall (1990).
22. This was flexible and comparable to the *marke* system described by Goldstein and Beall (1990: 69–71). The Tibetan pastoralists of Pala calculated the relative carrying capacity of their pastures in relation to how much land was necessary to produce one measure of butter and used this as the basis for a periodical reallocation of pastures.
23. See also Du (2006: 45–48).

REFERENCES

Anonymous 1884. rGya tsho ba'i sa tho dge'o. Tibetan manuscript kept by the Porong exile community in Kathmandu.

Bacot J., F.W. Thomas & C. Toussaints 1940. Documents de Touen-Houang relatifs a l'histoire du Tibet. Paris: Librairie Orientaliste Paul Geuthner.

Barnett, R. 1996. Cutting Off the Serpent's Head: Tightening Control in Tibet, 1994–1995. London: Tibet Information Network and Human Rights Watch / Asia.

Basso, K. 1996. Wisdom Sits in Places: Landscape and Language among the Western Apache. Albuquerque: University of New Mexico Press.

Bauer, K. 2005. "Development and the Enclosure Movement in Pastoral Tibet since the 1980s." Nomadic Peoples 9, 53–81.

Bauer, K. 2006. "Common Property and Power: Insights from a Spatial Analysis of Historical and Contemporary Pasture Boundaries among Pastoralists in Central Tibet." Journal of Political Ecology 13, 24–47.

Biography of Chokyi Dronma. Dpal 'Chi med grub pa (?)Ye shes mkha' 'gro bsod nams 'dren gyi sku skyes gsum pa rje btsun ma chos kyi sgron ma'i rnam thar. Incomplete manuscript of 144 folios.

Byg, A. & J. Salick 2009. "Local Perspectives on a Global Phenomenon—Climate Change in Eastern Tibetan Villages." Global Environmental Change 19 (2), May, 156–66.

Cruikshank, J. 2007. "Melting Glaciers and Emerging Histories in the Saint Elias Mountains", in M. de la Cadena & O. Starn, eds. Indigenous Experience Today. Oxford: Berg.

Cui, X. & H.F. Graf 2009. "Recent Land Cover Changes on the Tibetan Plateau: A Review." Climatic Change 94, 47–61.

Cui, X., H.F. Graf, B. Langmann, W. Chen & R. Huang 2006. "Climate Impacts of Anthropogenic Land Use Changes on the Tibetan Plateau." Global and Planetary Change 54, 33–56.

Cui, X., B. Langman & H.F. Graf 2007. "Summer Monsoonal Rainfall Simulation on the Tibetan Plateau with a Regional Climate Model Using a One-Way Double-Nesting System." SOLA 3, 49–52.

Diemberger, H. 2007. When a Woman Becomes a Dynasty: The Samding Dorje Phagmo of Tibet. New York: Columbia University Press.

Diemberger, H. 2010. "Life Histories of Forgotten Heroes? Transgression of Boundaries and the Reconstruction of Tibet in the Post-Mao Era", in Oral Histories of Socialist Modernities in Central and Inner Asia, special issue, Inner Asia 12 (1), 113–25.

Diemberger, H. & G. Hazod 1997. "Animal Sacrifices and Mountain Deities in Southern Tibet—Mythology, Rituals and Politics", in S. Karmay & P. Sagant, eds. Les habitants du toit du monde: Volume in Honour of Alexander Macdonald. Paris: Société d'Ethnologie, Nanterre.

Du, F. 2006. "Grain for Green and Poverty Alleviation: The Policy and Practice of Ecological Migration in China." Horizons 9 (2), 45–48.

Du, F. 2009. "Ecological Resettlement Policies Increase Herder Vulnerability and Poverty." Presentation given at the conference Sustaining Herder Community-Based Grassland Management through Improved Government Policies and Practices. Chinese Academy of Social Science, 18 December 2009.

Feng, Y. 2008. "The Tibetan Plateau: The Plight of Ecological Migrants." http://www.chinadialogue.net/article/show/single/en/2445.

Fisher, A. 2008. "Tibet in Transition: Challenges to Development and Environment in the Shadow of the Leaping Dragon." http://www.thlib.org/collections/texts/jiats/#jiats=/04/bauer/b3/#ixzz1AlQnyS3T.

Goldstein, M. & C. Beall 1990. Nomads of Western Tibet: The Survival of a Way of Life. University of California Press.

Graf, H.F. et al. 2011. "Boundary-Layer Structure and Turbulent Fluxes above the Tibetan Plateau." Presentation at the 7th Sino-German workshop on Tibetan Plateau research, Hamburg, 2–6 March 2011.

Harris, R.B. 2010. "Rangeland Degradation on the Qinghai-Tibetan Plateau: A Review of the Evidence of Its Magnitude and Causes." Journal of Arid Environments 74, 1–12.

Hobbs, J. 2011. "'When the Milkbird Comes': Amdo-Tibetan Constructions of Time in Gansu and Qinghai Province, PRC." PhD thesis, Department of Social Anthropology, University of Cambridge.

Huber, T. & P. Pedersen 1997. "Metereological Knowledge and Environmental Ideas in Traditional and Modern Societies: The Case of Tibet." JRAI 3, 577–98.

Hulme, M. 2009. Why We Disagree about Climate Change. Cambridge: Cambridge University Press.

Ingold, T. 2000. The Perception of the Environment. London: Routledge.

Kang,M. , S. Liu, X. Huang, S. Dong, and P. Shi 2005. "Evaluation of an Ecological Security Model in Zhalute Banner, Inner Mongolia" Mountain Research and Development Vol 25 No 1 Feb 2005: 60–67

Magistro, J. & C. Roncoli 2001. "Anthropological Perspectives and Policy Implications of Climate Change Research." Climate Research 19, 91–96.

Marin, A. 2010. "Riders under Storms: Contributions of Nomadic Herders' Observations to Analysing Climate Change in Mongolia." Global Environmental Change 20, 162–76.

Miehe, G. et al. 2008. "An Inventory of Forest Relics in the Pastures of Southern Tibet (Xizang A.R., China)." Plant Ecology 194 (2), 157–77.

Mölg, T, D.R. Hardy, N.J. Cullen, G. Kaser 2008. "Tropical Glaciers, Climate Change and Society—Focus on Kilimanjaro" in Orlove, B., E. Wiegandt & B. Luckman (eds) *Darkening Peaks: Glacier Retreat, Science and Society.* Berkeley: University of California Press.

Orlove, B., E. Wiegandt & B. Luckman 2008. Darkening Peaks: Glacier Retreat, Science and Society. Berkeley: University of California Press.

Qiu, J. 2008. "The Third Pole: Climate Change Is Coming Fast and Furious to the Tibetan Plateau." Nature 454, 24 July, 393–96.

Shaffer, S. 2011. "Public Trials and Climate Shows." Paper given at the conference Climate Histories: Communicating Cultural Knowledge of Environmental Change, Cambridge, CRASSH, 22 January 2011. http://climatehistories.innerasiaresearch.org/wp-content/uploads/2011/01/SimonSchaffer.pdf.

Sinha A., L. Stott, M. Berkelhammer, Hai Cheng, L.R. Edwards, B. Buckley, M. Aldenderfer & M. Mudelsee 2010. "A Global Context for Megadroughts in Monsoon Asia during the Past Millennium." Quaternary Science Reviews 30, 1–16.

Sneath, D. 1998. "State Policy and Pasture Degradation". Science 281 (5380), 21 August, 1147–48.

Sneath, D. 2000. Changing Inner Mongolia: Pastoral Mongolian Society and the Chinese State. Oxford: Oxford University Press.

Wang, X. & Bai, N. 1990 [1986]. The Poverty of Plenty. Basingstoke: Macmillan.

Xue Y.K. 1996. "The Impact of Desertification in the Mongolian and the Inner Mongolian Grassland on the Regional Climate." Journal of Climate 9 (9), 2173–89.

Yeh, E. 2005. "Green Governmentality and Pastoralism in Western China: 'Converting Pastures to Grasslands'." Nomadic Peoples 9 (1–2), 9–30.

Yeh, E. 2007. "Tibetan Indigeneity: Translations, Resemblances, and Uptake", in M. de la Cadena & O. Starn, eds. Indigenous Experience Today. Oxford: Berg.

7 Scaling Climate
The Politics of Anticipation

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In recent years, adaptation has emerged as an important concern of climate policy. A growing interest in adaptation reflects recognition of the fact that despite mitigation efforts people all over the world struggle to cope with a rapidly changing environment. In comparison to the well-defined causal factors addressed by mitigation policies (i.e. greenhouse gas emissions), adaptation strategies respond to a variety of challenges that may only to a limited degree be seen as relating to climate change. In many cases, environmental changes are embedded in a long history of social, technical, and political changes. Whereas multi-national actors such as the United Nations Framework Convention on Climate Change emphasize the importance of adapting to the local effects of anthropogenic warming, "climate impacts" may not appear as such a distinct category to local actors. Indeed, the most pressing local environmental hazards may be perceived as only marginally caused by climate change, if linked to them at all (Pielke et al. 2007). This evokes questions of how people come to understand and engage with their immediate environment in terms of climate change. How, for instance, does a devastating storm in the North Atlantic—a region notorious for its storminess—become understood as a consequence of climate change? Such a framing of local environments and events as climate change is not self-evident. It is a result of a social process that—like other forms of globalization—can be traced empirically.

In this chapter I will do just that. I follow a multi-national climate adaptation project in the making to examine how it acts as a setting for practices of assigning entities to spatial levels. My aim is to explore two sets of questions. The first relates to how the local and the global are constructed in programmes of climate adaptation. More specifically put, through what practices do entities become framed as instances of the global environment? And what constitutes "the local" in experiments of climate foresight? Secondly, what implications do scaling practices have for agency in adaptation programmes? My aim is to build on insights from science and technology studies (STS) to challenge the assumption, widely enacted in programmes of climate adaptation, that local knowledge and science stem from two fundamentally different ways of approaching a given nature. In brief, STS theorists such as Latour and Haraway have pointed at two main problems with this assumption. First, that when analyzed empirically, the divide between universal science and local knowledge does not hold. Both are situated, hybrid practices. Second, that "nature" does not pre-exist its construction. What counts as "nature" in any given circumstances is an outcome of multiple (and constructed) relations between humans and non-humans. Importantly, this is not the same as saying that nature is a "mere" social construct, thus reducing it to ideology. Rather, these scholars emphasize *the work* that goes into establishing what nature is, including "the work" of *both* non-humans and humans (see e.g. Latour 2005; Haraway 1992). Therefore, there are multiple "natures" and a central question is who is given the power to speak on behalf of any specific nature (Asdal 2003). In the chapter, I show that such assumptions within adaptation regimes have consequences for how the public is involved in climate foresight experiments.

My case is a three-year policy-oriented adaptation project called Climate Adaptation in Seaside Communities (CASE-C).[1] The main aim of CASE-C was to create tools and methods to enhance the adaptability of coastal communities faced with climate change. In the chapter, I follow the development of CASE-C from the time of the event that triggered the idea of the project in 2005 until one of its first experiments in public involvement four years later. My attention is on how the objective and the approaches of the project develop during this time, in particular on moments where scales are being negotiated and enacted.

Before coming to my empirical material, I first look at STS' criticism of conventional understandings of local knowledge and public involvement and how these relate to discourses on adaptation within climate change regimes. Then, I outline a practice-oriented approach to scaling, building on the work of Bruno Latour and Anna Tsing, where spatial levels are understood as both fluid and emerging.

LOCAL KNOWLEDGE IN CLIMATE FORESIGHT

Compared to mitigation, adaptation policies require more intricate methods of foreseeing changes. For the most part, the outputs of climate models are in the form of measurable climate variables, such as temperature, precipitation, and sea level rise, scaled to global or continental levels. However, as Oreskes et al. (2010) point out, people do not adapt to global temperature averages. Adaptation calls for information on a finer scale, along with the participation of a wider range of disciplines including biology and the social sciences. To account for the needs of adaptation, future projections have thus become more complex, incorporating diverse knowledge as well as efforts of "downscaling" outputs to a regional level (ibid.). Public involvement is also seen as increasingly important.

The story of CASE-C describes the collaborative efforts of experts and publics in imagining the future. I use the term "climate foresight" to describe such participatory approaches in contrast to the more technical "scenario building" of climate modelling.[2] The term "foresight" captures a more democratic sense of creating future visions, going beyond scenario building to include broader involvement and ideally, the construction of a shared sense of commitment to a strategic vision of the future (Jensen 2010; Miles et al. 2008).

Yet, the ways by which foresight experiments enact democracy is open to question. Recently there has been a turn to politics in STS, especially experiments of public engagement in scientific governance and scientific controversies (see e.g. Irwin 2006; Callon 1999; Leach et al. 2005). Research within this strand indicates that although there is an increased emphasis on public participation in political discourse on scientific governance, many of these experiments are embedded in the old model of science as the main and uncontested "informant" of politics. In these "new" ways of bringing in citizens, the public continues to be constructed as lacking in some sense, either lacking in scientific literacy or in trust in science (Irwin 2006). Increased participation does therefore not necessarily represent a transformation in the democratization of scientific governance, and may indeed "serve to promote and conceal socio-political agendas, while pre-empting debate on alternative futures" (Levidow & Marris 2001: 357). One problem with these experiments of public participation is that they also assume that different groups act within the same epistemological regimes (Leach et al. 2005; Verran 2002). Verran (2002) has portrayed how such assumptions contributed to extensive confusion and distrust when environmental scientists in Australia attempted to involve aboriginal landowners in their firing practices. The aboriginal landowners and the scientists had fundamentally different ways of generalizing about the habitats in which the firing practices took place. Verran describes the episode as highly bewildering for both groups. Aboriginal perspectives that the land had agency, for instance, made little sense to the scientists who saw themselves as separate from the land. Verran points out that the two groups were not simply placed in the same nature, with different cultural labels attached to it, but rather, nature itself was fundamentally different to the two groups and failing to recognize this lead to perplexity and frustration during their collaborative efforts.

In order to elucidate such encounters between different *ways of being*, STS scholars such as Marres (2009) and Hinchliffe (2001) argue that there is a need to turn away from normative models of public participation towards empirical studies of how citizens engage with techno-scientific objects. Marres emphasizes that mobilizing the local through programmes of public participation does not simply involve bringing local knowledge into established expert-defined frameworks. Rather, the involvement of citizens is inclusive in the very making of the scientific object in question. As local entities are mobilized, the object and its context are altered. The STS critique of programmes of public participation is based on this insight, namely, that such programmes tend to fail to take into consideration that a locality is not merely a part of a given global, but also a place of the production of alternative globals. Applying this to analysis of climate adaptation programmes implies exploring whether and how such projects act as a setting for a two-directional intervention between science and local knowledge.

Such a fluid understanding of spatial levels is in stark contrast to representations of the local in climate adaptation regimes. The following definition of the term "indigenous knowledge systems" in the IPCC 2007 report on "impacts, adaptation and vulnerability" (Parry et al. 2007: 865) provides an example: "The term 'indigenous knowledge' is used to describe the knowledge systems developed by a community as opposed to the scientific knowledge that is generally referred to as 'modern' knowledge". In this passage, local knowledge—here referred to as indigenous knowledge—is not only constructed as distinct and fundamentally different from science, but also as somewhat less "modern". Implied here is the old story STS has refuted in 30 years of research, of objective science as being opposed to the cultures and politics of the local.

Another manifestation of this narrative is found in discussions of the status of local knowledge in climate foresight. It is sometimes suggested that anthropology—as the discipline that studies local knowledge—can take on the role of bridging the local and the global, offering "finer levels of resolution" to climate models by bringing in "local perspectives" (see e.g. Magistro & Roncoli 2001). According to this viewpoint anthropology has the role of complementing global models in order to "zoom in" on local social and environmental specificities. The problem here is that scales are seen as something that orders rather than something that has to be explained. This perspective bypasses the question of *where* local knowledge is found and how some situated knowledge acquires the status of the global. The assumption is that the "human dimensions" of climate change are situated in specific types of locations such as with the small farmers in the Amazon coping with drought or among Inuits in Canada dealing with changes in sea-ice. Culture is seen as an empirical category *external* to knowledge and climate models, analogous to, say, economic institutions (Lahsen 2010). Exploring the categories of the global and the local as emergent in the practices of anticipation, rather than as given, opens up space for a more symmetrical approach, where globalities are seen as originating from a multiplicity of places, not uniquely, but including the offices and meeting-rooms of climate scientists.

A PRACTICE-ORIENTED APPROACH TO SCALING

In looking at the categories of the global and the local as fluid, I follow recent trends within STS of analyzing scales as emergent rather than fixed (see e.g. Blok 2010; Jensen 2007; Latour 2005). I propose a practice-oriented approach that draws on the work of Latour (2005) and Tsing (2010). In applying such an approach to my empirical case, I move away from assuming that the actors involved in the project "naturally" belong to a given level, to look at how scaling serves both to create and delimit spaces for social action. I look at the global and the local as enacted and as a relational effect, rather than as a given.

Actor-network theorists such as Bruno Latour (2005) reject the notion that scale pre-orders social spheres. Rather, Latour argues, the social world is "flat" in the sense that the macro is not a *larger* place found "above" the micro, but simply another "local", with a multiplicity of connections to other places. Scale emerges in the scaling practices of actors, as they contextualize each other. The local is a product of "making small" whereas the global involves "making total" (Blok 2010: 53). The value of this approach is that it steps away from the local and the global as rigid dual categories and towards seeing them as continually emergent and situated.

This implies that the global ceases to be the given context into which micro-level social scientists, such as anthropologists, "zoom" in order to sharpen the picture, as when a photograph is sharpened with increasingly finer pixels. Rather, the task becomes that of analyzing the very practices of contextualizing. This involves exploring the ways by which parts are enacted as belonging to wholes. If a part receives its meaning from the context, the context, in turn, does not exist outside these enactments. The part and the whole thus co-emerge and mutually constitute each other.

This is an important point, because just as the global is not self-evident, so is there no naturalized category of "local" knowledge to "complement" the global in order to allow for a "fuller picture". As in the example of the Australian aboriginal fire-practices, "local" ways of knowing may entail different ways of being, including different ways of generalizing and globalizing. Similarly, "local" ways of knowing may embrace the global environment, such as when local people "see" global climate change take place in their immediate environment. What constitutes the "local" and the "global" is an empirical question.

A practice-oriented approach to scaling, therefore, involves exploring how specific entities or events become spatially framed and what such contextualizing practices do to the entities and their context. In the following I attempt to show that projects of adaptation do not simply involve intervening in localities through "raising awareness" and public education. Rather—building on Marres (2009)—they are instances of the co-production of entities, publics, and the global environment. The global environment only exists in its specific and situated enactments.

Before moving to my case I would like to include one more analytical step. Latour's "flat" approach implies that context *only* exists as a relational effect that is traced empirically. Just as the global is a sort of a local that needs to be traced in the ethnographic encounter, so is context "flat". Actor-network theorists thus deny social scientists of a pre-existing social context (such as "the capitalist system") as a backdrop to analysis. This approach is both refreshing and challenging because it demands a thorough examination of what the whole is made of in every social instance. However, Tsing (2010) points out that such an abolition of analytical context is also problematic. She uses the concept *worlding*, defined as "the always experimental, partial, and often quite wrong, attribution of worldlike characteristics to scenes of social encounter" (ibid.: 48), to refer to the contextualizing practices of scientists, including actor-network theorists. Worlding is what scientists do when they make sense of their data or when they create a narrative out of raw data. This involves making judgements about which "worlds"—or wholes—are fitting. Constructing wholes not only involves deciding what is part of a given "world", but also what is not. Here, the problem with actor-network theory is its sharp focus on the movement involved in the construction of actor-networks such as contexts or globalities. Actor-network theorists tend to bypass that which is being ignored, silenced, and left out in practices of constructing contexts, including in their own efforts of whole-making.

Importantly, Tsing's criticism points at a lack of reflexivity in actor-network theory. However, what is of main interest here is her insight that wholes—or contexts—are powerful in the sense that they allow for the room where sense-making takes place. Like Latour, Tsing is critical toward the naturalization of contexts, but unlike Latour, she acknowledges the inevitability of context-making as well as its importance for carving out space for social action, all the while recognizing that worldings are always partial, fragile, unstable, and excluding. Looking at worlding practices in adaptation programmes compels us to ask not only what worlds come into being in the making of climate foresights, but also what worlds are silenced and excluded in the practices of anticipating the future.

INTERVENING IN THE GLOBAL

The project Climate Adaptation in Seaside Communities (CASE-C) was launched in the spring of 2009 with funding from the Northern Periphery Programme (NPP). The CASE-C partners were five municipalities and several research institutes in the North Atlantic region. The aim of the project was to develop and implement tools and strategies for adaptation in order to "enable people living in coastal communities to take action and adopt strategies that deal with sea-level rise" (the application) and other expected climate impacts, both negative and positive. I became a participant in the project at the time it was launched. My personal aim was to follow it as a part of my PhD research. I actively participated in meetings, social events, and workshops in addition to conducting interviews and focus groups as an expert within the project.

The history of CASE-C began in the evening of 11 January 2005 in South Uist of the Scottish Outer Hebrides. A violent cyclonic windstorm hit the island and caused severe damage to infrastructure, buildings, and roads. During the evening, a young couple escaped from their flooded house with two small children and the wife's father. As they drove away in two cars, a wave swept them into the ocean, taking their lives. The tragic incident and the extreme destruction caused by the storm left a huge impact on the community of South Uist.

Cyclonic windstorms are not uncommon in Northern Europe. However, when they are combined with a high tide in coastal regions, they can result in coastal floods that sometimes cause damage and occasionally take lives. On average, there are two coastal floods a year in Scotland, but prior to the incident, coastal flooding was perceived as a low risk, "highly localized and unlikely to generate significant economic losses" (Ball et al. 2008: i).

The community experienced the flood as a great menace to their feeling of safety and the local government was accused of having failed to protect the people from coastal hazards. In the months following the tragedy, a series of events initiated by the people of the community as well as the local government established coastal floods as an environmental threat that demanded both political action and expert intervention. In February, a month after the event, two public meetings were held in South Uist to organize further action. The Ichodar Flood Action Group was formed with the aim of promoting a "holistic approach to addressing coastal sustainability issues" (CoastHebrides n.d.).

During the following spring and summer, more open meetings were held and scientists were consulted. The immediate concerns of the action group were not framed in terms of climate change, but focused on measures to ensure the safety of the community, such as better monitoring of the sea, the creation of new escape routes, and the design of safer causeways between islands (see e.g. Bell 2008; Scott 2009). As the public debate broadened to include possible preventive actions and future developments of coastal erosion and storminess, the flooding event was increasingly put into the context of global climate change in public meetings as well as by the media and NGOs (see e.g. Oxfam 2009; BBC 2009). There was growing concern in the Outer Hebrides, especially in South Uist, that extreme weather events were becoming more common. Climate change moved to the fore as a political issue as one local pointed out in an interview: "The year 2005 changed the way people thought; the storm put climate change on the agenda".

Climate and earth scientists were more hesitant in making the link between the storm and climate change. Reports and scientific papers published as a reaction to the storm were generally in agreement that North Atlantic storms had not increased in the past years and were not likely to do so in the near future despite climate change (Ball et al. 2008: iv; Dawson et al. 2007; Corbel et al. 2007; Wolf & Woolf 2005). Neither could coastal erosion be linked in any decisive way to sea level change according to some research (Dawson et al. 2008).

The raised awareness of climate change after the event was thus not an instance of increased "public understanding" of science, but rather a result of local efforts of contextualizing. Nonetheless, increased concern about climate change should not be explained away as a "mere misunderstanding" of the facts. An expert I interviewed said that although the link between the storm and climate change was fragile if not non-existent from a scientific point of view, it did not matter so much because after all, climate change was a *fact*. A growing concern about climate change mobilized the community in ways that did not relate directly to storm, such as in the preservation of the machair, a rare habitat particular to the northwest coastlines of Scotland and Ireland.

Evoking climate change was not only a political act in the sense that new environmental concerns were brought to the fore. It also allowed the locals to take action to attempt to re-establish a feeling of safety in their relationship with the sea. Reframing the event as an instance of something much larger than its immediate temporal and spatial setting radically altered the room for action. New connections were made, such as those to the other communities of CASE-C. Yet, as I will show later, it also led to the exclusion of certain forms of agency, as framing the storm event as climate change involved defining *which* nature was enacted in anticipating the future.

Relating the storm to climate change radically altered the context that forms a part of the Outer Hebrides. Suddenly, the storm did not only tell a story about the Hebrides and its past, but also about the world and its future. In this story, it was not the experts that informed local people about the impact of climate change on their environment. Rather, the local people related to climate change as they attempted to make sense of the event and establish spaces for action. Furthermore, this framing had an impact on global environments as the story travelled outside the Outer Hebrides. In 2009 the BBC broadcast a special about climate change that included the story of the tragedy of storm in South Uist (BBC 2009). Here, local practices of scaling were reshaping the global.

INTERVENING IN FUTURES

A part of the local council's reaction to public demands of improved safety measures in the Outer Hebrides was to increase the emphasis on cross-regional collaboration. It was recognized that funding from the European Union—particularly through the Northern Periphery Program (NPP)—could provide the basis for such collaboration. It was in this context that CASE-C began to be realized. The project came to be framed around "climate adaptation", but such a framing was not given from the start, as the project leader explains:

> When I first thought of such a project, it had to do with the environmental impacts of the action of the sea and flooding and that type of

> thing. Climate change didn't really come into the project until later on, and I must admit that it was a bit more funding driven (laughs).

Yet, to draw the conclusion that framing the project as climate change was "only" about attracting funds would be to miss the point. Funding cannot be seen as an external actor in the production and mobilization of science. Rather, the framing of the NPP was a part of an ongoing process of contextualization—or worlding, involving actors both inside and outside the confines of science.

The funding agency had an impact in other ways as well. The initial ideas of the project group were to map and quantify the impacts of projected climate change in the participating communities and, further, to develop policies of mitigation and adaptation. However, as soon as the partners met with the representatives of the NPP it became clear that the aims would have to be altered. The NPP was not interested in funding projects that were primarily aimed at producing knowledge. Rather, their concern was with what they saw as more efficient ways of intervention. The NPP suggested the project group move away from the focus on *climate impacts* towards developing strategies and tools aimed at enhancing the *adaptive capacity* of communities faced with climate change. These tools were to be in the form of visualizations of sea level rise, vulnerability and adaptive capacity assessments, checklists, guidelines, factsheets, best practices, etc.

The goals of the NPP have to be understood in the context of the EU's political agenda. The NPP is a part of the EU's regional programmes that aim at strengthening and building up regional identities and cooperation in Europe. The NPP thus promoted a specific kind of intervention: one that served the political goal of the EU's region building. The NPP's own funding is limited in time, so the programme regularly has to provide evidence of its efficiency to the EU. As such, the mobility and the direct applicability of the project's products, providing relatively quick impact, were of primary importance.

Hence, the NPP took an active part in contextualizing and scaling the project. Whereas the project leader who initiated CASE-C was mainly concerned with bringing in EU resources to act on the immediate and newly politicized issues of coastal safety in his community, the NPP intervention expanded the context towards the global environment of climate change. This included financing tools such as checklists and vulnerability assessments that worked towards the homogenization of places. For the Outer Hebrides framing in terms of climate change resulted in the contradiction that as people attempted to act on the sea as a hazard, other local entities—those seen as impacted by climate change—were being identified as uncertain as well.

In the preparatory phases of the project, the partners were concerned about how to approach the local people in order to both gain access to local knowledge and influence strategies of adaptation. What were the appropriate ways of communicating climate change futures in such a way to avoid both raising false expectations and being overly negative? How to secure the interest of local stakeholders who perhaps did not see the immediate relevance of climate-related impacts for their sector? Much emphasis was put on including local knowledge and using "bottom-up" methods, but it was also clear to the project participants that the main aim of the project was to influence local strategies and visions. They were keen to advocate socio-economic approaches as well as—or sometimes rather than—technical solutions and this would imply going beyond simple "fixes". One CASE-C participant expressed this concern a preparatory meeting:

> We are trying to add information into [the local people's] future, into their long-term plans. We are not necessarily referring to protection, but rather to the question "can we adapt our way of life, our socio-economic systems?" A last resort would be to build a sea wall! We must give them other ways of thinking, e.g. soft solutions, new activities, etc.

One concern was that the project might not be effective for the simple reason that local materialities and politics would fail to be mobilized in the project. Influencing local policy-making and future anticipations would not be successful without citizen involvement. For this reason, "climate change impacts" had to have some relevance to established social and environmental issues, such as the increase of jellyfish that was causing problems to fisheries—to name one example used in the preparatory meeting. So although NPP felt it was important for the project to aim at local policy-makers, this would not work without also including the general public, as one CASE-C participant pointed out:

> Relatively deep knowledge [about climate change] to a few people [is needed], but also a little bit of knowledge to as many people as possible. You need to try to get engagement with as many people as possible. If councils start taking actions without community support, there will be reactions. You want the entire community pushing for action.

Hence, the initial meetings were concerned with the question of what methods of intervention were the most appropriate in the attempt to influence and alter local visions: to rescale the local environment both spatially and temporally. The project's experts were well aware that this could not be arrived at by only engaging certain elite groups within the communities, such as local policy-makers. In their attempt to intervene in the future, the experts relied on the engagement of citizens and the politics of local entities, such as jellyfish. Thus, science was not only brought to the different localities, but the localities also had to be brought to the experts in order for their efforts of setting the context as the global environment of climate change to be successful.

INTERVENING IN THE LOCAL

But how to successfully bring together local entities and expert knowledge to intervene in the five pilot sites? This did not prove to be a straightforward task for CASE-C. The project's primary tool for engaging citizens was a series of open workshops, held in the pilot sites. The workshops were seen both as sources of information to develop tools such as vulnerability assessments and adaptation strategies and as a forum for the active intervention into local futures. For that, "the expert knowledge must be interpreted by local stakeholders according to local context" (the application).

In the autumn of 2009, I participated in an open workshop in Kvalvik in Northern Norway.[3] It took place in a small auditorium in a museum lying centrally in the town. Despite workshop advertisments in local newspapers, no participants came "off the street". The 15–20 people who attended were there by invitation, mostly from the local administration and related institutions.

The workshop began in at two o'clock in the afternoon. For the first three hours several experts presented research on diverse socio-economic aspects of projected climate change impacts that related to Kvalvik. This part of the workshop was "aimed at providing the audience with the current state of scientific knowledge on the direct impacts of climate change and sea level rising on the [community] and introduce the topic of adaptation" (workshop report). From my field notes:

> After the break a professor from a regional college talks about the question: How will social and economic change influence climate adaption in the next decades? He says he has a "scenario sketch", built on a bottom-up-perspective, to portray Kvalvik in the year 2040. "Most scenario work is top-down, from macro perspectives, like in the case of emission scenarios," he says. "We need more bottom-up approaches, municipal perspectives. There is a need of foresight with the participation of local authorities," he continues. "Not scenarios, but rather foresight. The difference is that foresight includes participation." His slides portray a forecast of what Kvalvik will be like in year 2040 in terms of population, mentality, industry, settlement structures, infrastructure and mobility.
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> It's already past 4 o'clock. The meeting has gone on for more than two hours. The talk is interesting, but I feel tired from sitting and listening. I notice that the woman next to me is falling asleep. I also notice that some people from the audience have already left.
>
> The professor says pointing at the slide, "Kvalvik will be rich, a small Stavanger. There will be a growth pole in the oil and gas cluster, low consciousness about climate and environmental threats, high value creation, civil pride in the new oil capital, strong consumerism, a new

rich class—conspicuous consumption. Kvalvik will move from being a small municipality to being a regional centre."

After the expert talks, the group is divided into four discussion groups and each group is given a specific topic to discuss relating to the impact of climate change on livelihoods, infrastructure, and housing. Papers with discussion questions have been distributed. They include questions such as "what do you see as the most important challenges for the Kvalvik community with regards to a changing climate and more extreme weather conditions?" and "what do you think are the most important measures to implement in order to face the challenges?". The discussions last about an hour and then another 45 minutes are used to round up the results. It is well over seven in the evening when the workshop is over.

In the evaluation report from the workshop, the participants are generally content with the meeting, but complain that the presentations were too complicated and scientific and there was too little time allocated to the discussions. Similar criticisms were put forth in other CASE-C workshops.

The criticism and the limited participation are an indication that the workshop failed, at least partly, in intervening and bringing climate change to the local people. I argue that this was because of implicit assumptions about the role of local knowledge that were enacted in the design and the implementation of the workshop. First, specific futures were communicated during three hours of expert lectures, in the aim of informing the participants. This one-directional transfer of knowledge was seen as essential to "put the stage" for the public discussion. The discussion topics were also rigidly defined by the experts. The goal was therefore not to discuss *any* nature, but *one specific* nature of climate change. Secondly, the very arrangement of the workshop, emphasizing slideshows, expert language, passive listening, structured discussions, etc. also placed the participants within a specific setting prioritizing expert ways of being. The setting reflected the assumption that both lay and expert knowledge is easily de-contextualized from practices and that they are equally transformable into a text. It was assumed that local knowledge is epistemologically similar to expert knowledge. The design of the workshop thus excluded alternative ways of being.

Although CASE-C's emphasis on involvement and participation is a step in the direction of a more democratic approach to climate foresights, the project participants still took the authority of experts in defining nature for granted. The workshop was as much a moment of intervention into local knowledge as it was a source of knowledge about the local because it enacted certain assumptions about "worlds" or wholes. It served as a setting where local entities were rendered into the global, thus transforming both. The participants' role was to contribute to the global environment provided by the experts, thus depriving them of agency and authority on nature.

CONCLUSION: THE POLITICS OF ANTICIPATION

In a recent volume, Strauss and Orlove (2003) bring into attention the question of how social and cultural forms influence the way short-term weather becomes understood as climate. They point out that "people experience, discuss, and interpret meteorological phenomena in ways that are dependent not only on the physical characteristics of the events, but also on the cultural frameworks that divide time into current, recent and distant periods" (ibid.: 6). One of the questions addressed in this chapter is: where do such "cultural frameworks" come from? My example shows that climate change is not something that is received by local people via awareness-raising efforts, to be understood through the lens of the "local culture". Rather, what is taken as "local cultural frameworks" is better to be described as a set of constantly emergent relations. If climate change becomes a local source of causality it is because local cultural frameworks have been altered. And in the process the global environment of climate change is altered as well.

The story in this chapter is essentially about the politics of anticipating the future. First, community politics were altered in relating entities to climate change. In the example of the Outer Hebrides rescaling the storm provided new means to act on the uncertainties that it gave rise to, thus altering the room for political agency. At the same time, this led to a proliferation of uncertainties. In the workshops, for example, the participants were asked to envision the impacts of climate change on livelihood, businesses, infrastructures, and housing, identifying new sources of uncertainty.

Yet, the point here is not that climate change is "just" a social construct, a mere buzzword to attract attention and funds. After all, not everything lends itself to becoming climate change. Environments are able to resist scaling practices. The argument is that the boundaries of climate change are no longer only negotiated by scientists but also outside the confines of science.

In the story we travel with the project between its different geographical sites. The actors in each location have their specific concerns that they attempt to translate into the project. Here, climate change acts as a boundary object (Star & Griesemer 1989) fluid enough to be able to take different shapes in the various localities involved. It homogenizes in the sense that it brings different localities into a common understanding of the dimensionality and temporality of nature. This was done in many stages of the project: through institutionalizing with checklists, assessments, and visualizations; in the negotiations between the partners and the funding agency; in the definition of local environmental concerns; as well as in the workshops. Yet, although shared futures brought the different localities together, the ways of anticipating were never fixed. By examining how the global emerged as a specific enactment, it appeared as different in the various localities of CASE-C. In the Outer Hebrides the storm accentuated the uncertainties of climate futures, while in Kvalvik such a sense of urgency was lacking.

Hence, anticipations were never more than partially shared. In the story, the fluidity of climate change allowed it to act as a centre of gravity into which the different actors were able to render their diverse purposes and in doing so carve out space for social action.

Secondly, at the same time as climate change enabled politics, the design and execution of CASE-C, in particular the way "local knowledge" was constructed within it, marginalized or silenced certain forms of agency. This is most clearly seen in how the design of the workshop restricted certain forms of involvement. The workshop was an enactment of one specific nature—the global environment of climate change. The experts did not question their own ways of relating the local to a context or, put differently, to their own practices of worlding, to use Tsing's terminology. In the workshop, only experts acted as spokespeople for nature, "informing" the other participants. Thus, although the project stressed local engagement as well as "soft" and culturally sensitive methods of adapting to climate change—moving away from technical fixes—it also enacted old assumptions about linear knowledge transfers from science to policy. Local people were constructed as "cultural beings" simultaneously opposing and complementing the universal knowledge of science.

The experts of CASE-C recognized that the risk in their attempts to involve the public was that citizens would remain indifferent to climate change. Although they appreciated the importance of attending to ways of framing environmental changes, by relating climate foresight to local concerns (such as the increase of jellyfish) they took ways of being—understood as ways of practicing and relating—for granted. Intervening was thus seen as primarily a cognitive challenge—one that related to altering ways of thinking, rather than recognizing that people may live in different natures in a very real sense.

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NOTES

1. The name of the project has been changed.
2. In using the term "foresight", I follow one of the speakers in a CASE-C workshop held in Hammerfest. See later in the chapter.
3. "Kvalvik" is a pseudonym.

REFERENCES

Asdal, K. 2003. "The Problematic Nature of Nature: The Post-constructivist Challenge to Environmental History." History and Theory 42 (4), 60–74.

Ball, T., A. Werritty, R.W. Duck, A. Edwards, L. Booth & A.R. Black 2008. Coastal Flooding in Scotland: A Scoping Study. Edinburgh: SNIFER.

BBC 2009. "Island Braced for Winds of Change." 18 November.

Bell, E. 2008. "Iochdar Residents Set to Take High Road." The Stornoway Gazette. 6 March.

Blok, A. 2010. "Divided Socio-natures: Essays on the Co-construction of Science, Society, and the Global Environment." PhD thesis, University of Copenhagen.

Callon, M. 1999. "The Role of Lay People in the Production and Dissemination of Scientific Knowledge." Science Technology Society 4, 81–94.

CoastHebrides n.d. "What We Do."

Corbel, G.G., J.T. Allen, S.W. Gibb & D. Woolf 2007. "Wind Trend in the Highland and Islands of Scotland and Their Relation to the North Atlantic Oscillation." AMS 87th annual meeting, 19th Conference on Climate Variability and Change, San Antonio, Texas, January.

Dawson, A.G., S. Dawson & W. Ritchie 2007. "Historical Climatology and Coastal Change Associated with the 'Great Storm' of January 2005, South Uist and Benbecula, Scottish Outer Hebrides." Scottish Geographical Journal 123 (2), 135–49.

Dawson, A.G., W.A. Ritchie, D. Green, R. Wright, C. Gomez & A. Taylor 2008. "Assessment of the Rates and Causes of Change in Scotland's Beaches and Dunes—Phase 2." Scottish Natural Heritage.

Haraway, D. 1992. "The Promises of Monsters: A Regenerative Politics for Inappropriate/d Others", in L. Grossber, C. Nelson, & P.A. Treichler, eds. Cultural Studies. New York: Routledge (p.295–337).

Hinchliffe, S. 2001. "Indeterminacy In-decisions: Science, Policy and Politics in the BSE (Bovine Spongiform Encephalopathy) Crisis." Transactions of the Institute of British Geographers NS 26, 182–204.

Irwin, A. 2006. "The Politics of Talk: Coming to Terms with the 'New' Scientific Governance." Social Studies of Science 36 (2), 299–320.

Jensen, C.B. 2007. "Infrastructural Fractals: Revisiting the Micro-Macro Distinction in Social Theory." Environment and Planning D: Society and Space 25 (5), 832–50.

Jensen, S. 2010. "Foresight between Uncertainty and Convention: An Ethnographic Study of Research Policy Foresight at the Research Council of Norway." PhD thesis, University of Oslo.

Lahsen, M. 2010. "The Social Status of Climate Change Knowledge: An Editorial Essay." Climate Change 1, 162–71.

Latour, B. 2005. Reassembling the Social. Oxford: Oxford University Press.

Leach, M., I. Scoones & B. Wynne 2005. "Introduction: Science, Citizenship and Globalization", in M. Leach, I. Scoones, & B. Wynne, eds. Science and Citizens. London: Zed Books (p.3–38).

Levidow, L. and C. Marris 2001. "Science and Governance in Europe: Lessons from the Case of Agricultural Biotechnology." Science and Public Policy 28, 345–60.

Magistro, J. and C. Roncoli 2001. "Anthropological Perspectives and Policy Implications of Climate Change Research." Climate Research 19, 91–96.

Marres, N. 2009. "Testing Powers of Engagement: Green Living Experiments, the Ontological Turn and the Undoability of Involvement." European Journal of Social Theory 12 (1), 117–33.

Miles, I., J.C. Harper, L. Georghiou, M. Keenan & R. Popper 2008. "The Many Faces of Foresight", in L. Georghiou, J.C. Harper, M. Keenan, I. Miles, & R. Popper, eds. The Handbook of Technology Foresight: Concepts and Practice, Glos, UK: Edward Elgar Publishing Limited (p.3–23).

Oreskes, N., D.A. Stainforth & L.A. Smith 2010. "Adaptation to Global Warming: Do Climate Models Tell Us What We Need to Know?" Philosophy of Science 77 (5), 1012–28.

Oxfam 2009. "Scotland's First Climate Hearing Takes Place in Western Isles." 20 November.

Parry, M.L., O.F. Canziani, J.P. Palutikof, P.J. van der Linden & C.E. Hanson, eds. 2007. Climate Change 2007: Impacts, Adaptation and Vulnerability. Cambridge: Cambridge University Press.

Pielke, R. Jr., G. Prins, S. Rayner & D. Sarewitz 2007. "Lifting the Taboo on Adaptation." Nature 445, 597–98.

Scott, N. 2009. "WaveNet Buoy Launched Off Coast of South Uist. 2 March.

Star, S.L. & J.R. Griesemer 1989. "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907–39." Social Studies of Science 19 (3), 387–420.

Strauss, S. & B.S. Orlove 2003. "Up in the Air: The Anthropology of Weather and Climate", in S. Strauss & B.S. Orlove, eds. Weather, Climate, Culture. Oxford: Berg (p.3–14).

Tsing, A.L. 2010. "Worlding the Matsutake Diaspora: Or, Can Actor-Network Theory Experiment with Holism?", in T. Otto & N. Bubandt, eds. Experiments in Holism. Malden MA: Blackwell (p.47–66).

Verran, H. 2002. "A Postcolonial Moment in Science Studies: Alternative Firing Regimes of Environmental Scientists and Aboriginal Landowners." Social Studies of Science 32, 729–61.

Wolf, J. & D.K. Woolf 2005. "Waves and Climate Change in the North-East Atlantic." Geophysical Research Letters 33.

8 Emancipating Nature

What the Flood Apprentice Learned from a Modelling Tutorial

Anders Kristian Munk

Durham University, May 2007: Our instructor sings our praise as the water levels skyrocket and the ground fades away beneath a transparent layer of blue. "Congratulations, you've got flooding!" Quite, by a click of the mouse we have submitted the flood plain on the screens in front of us to a deluge of surreal proportions. The flow of water quickly reaches the edge of the animation window and queues up in a square column behind a bridge crossing the river. Despite the fact that we are all novices in the art of hydraulic computer modelling, it is painstakingly obvious to all of us that we may have managed to produce a flood, but not a particularly credible or realistic one of those. We are still a far cry from claiming that we are simulating something that could realistically take place. But that is not the point right now. What feels like a real achievement is that the model is actually running after an unending stream of bugs and error messages. Finally, we seem to be doing something right; we have learned how to feed it the correct stuff; it is complying. Or rather, we are complying: at this point in the training exercise the happy amateurs are tuning in to the demands of the software and for the first time we get a sense of what it means to become a modeller.

"Hope", writes the British geographer Ben Anderson, "is easily identified and its quantitative presence or absence highlighted, but the taking-place of hope, its mode of operation, remains an aporia" (2006: 733). As flood apprentices we are perhaps not quite becoming hopeful on behalf of our simulated flood events, but a somewhat related propensity is definitely detectable in the room, namely that of anticipation. What I want to explore here is the ways in which it takes place. No matter how you choose to construe the ambiguous notion of "anticipating nature", the virtual world of hydraulic modelling is arguably ripe with it. Firstly, and perhaps most straightforwardly, it constitutes the motivation for simulating floods in the first place: the ambition to pre-empt the riverine environment is the driving force of the enterprise. In this sense one could talk of anticipating nature as a kind of professional purpose. My fellow trainees and I constitute a newly formed trans-disciplinary research group with no prior experience in flood modelling and the reason we are following the training course is to get a preliminary sense of what it means to be a professional in the business of estimating and preventing floods.[1] We have taken on the role as novices aspiring to become skilled anticipators. Secondly, in a more philosophical vein, that kind of proficiency presupposes the fundamental anticipation that nature is indeed out there as a well-defined motif to be modelled and simulated. In this sense one could talk of anticipating nature as a kind of epistemological precept or felicity condition for the modelling enterprise with which we are about to engage. As we are gradually finding out, our apprenticeship entails quite a bit more than learning which buttons to click: in order to evaluate our results we are developing a "perceptual model" of the environment. Thirdly, the theoretical distillation of nature, which is built into the software in the form of its hydrodynamic equations, has its own anticipations; it exacts a certain demeanour on behalf of its modellers; it expects us to feed it with a world rendered in specific and digestible formats. In this sense one could talk of anticipating nature as a curiously demanding attitude on behalf of something ("nature"), which is otherwise thought of as indifferent, if not downright inert. These stipulations, and our hard-won ability to abide by them, seem to be at the root of our excitement this afternoon: even though we are not yet able to claim that we are simulating anything, we finally have the model running.

What strikes me as interesting about these varying adaptations of anticipating nature is the element of paradox, which haunts them all in different ways. Pre-emptive intentions or not, the risk of flooding in the UK is rarely attributed to the riverine environment per se. On the contrary, it is constantly caught up in issues concerning urban development, rural land management, insurance policy, or emergency response times (see for example ABI 2000, 2007; DEFRA 2004; Pitt Review 2008; EA 2009; Crichton 2005, 2008; Kellman 2001; Brown & Damery 2002). In turn, and partly as a consequence, the well-defined nature serving as motif for the modelling effort turns out to be a heterogeneous arrangement which requires work in order to sustain itself as what Marilyn Strathern calls the "grounding conceptualization of knowledge for understanding the intrinsic character ('nature') of anything [in this case flooding]" (1992: 124). That arrangement relies, for example, on the rich history of hydrology, its techniques and its devices and the prosthetic character of such a nature comes particular to the fore when we, the novice modellers, have to face up to the exacting anticipations of the model. If it is true to say that we would be useless as modellers without the aid of the computer programme, it is equally true to say that the programme would be useless without the aid of us, its modellers, for translating the world into formats which conform to its inbuilt hydrodynamic formalizations of nature. Computer simulation thus implies the becoming of a hybrid—the model-modeller—which has more to do with Vinciane Despret's anthropo-zoo-genetic horse-rider constellation (2004) or Sarah Whatmore's more-than-human hybrids (2002) than any distinct bounding of a nature which can either be anticipated or have anticipations of its own. Following Bruno Latour's call to do for "nature" what feminism did for "man", namely to "wipe out the ancient self-evidence with which it was taken a bit too hastily as if it were all there is" (2004: 49), I pursue the idea that the becoming of this model-modeller is more about emancipating nature from its exile as a detached domain of inert objects and indifferent forces, and less about anticipating nature thus bounded. In doing so, I will argue that in the case of my flood modelling tutorial anticipation took place through a series of contraventions which did indeed wipe out any self-evidence with which a thing like nature might otherwise have been taken.

ANTICIPATING NATURE #1: THE PARADOX OF PURPOSE

Although flooding formally counts as a natural hazard, and although insurers label that type of peril an "act of God" to denote an origin outside the sphere of human control and responsibility, most flood events do not align with such straightforward causation. Not only are they influenced by a broad range of factors which cannot be traced to a detached domain of nature, but they themselves represent a very diverse set of incidents. Floods come in a plethora of formats ranging from sewage backing up through toilets and drains, over rivers swollen by torrential downpours, to storm surges wreaking havoc across coastal regions. They can be attributed to anything from weather patterns to urban planning or rural land use, and they concern a range of academic fields, technological specialties, and areas of expertise. As the water started receding after the hitherto most severe flood disaster in the UK in July 2007, the Daily Mail ran the following editorial:

> Scientists say that Monday's monsoonlike downpours are the increasingly strange shape of things to come . . . But this is not simply an act of Nature. This flooding was also a result of systematic, shortsighted failings on part of successive governments. Now questions must be answered: Why has half the new housing built since the Second World War been built on flood-prone land? Why do we keep concreting over the countryside, destroying the natural drainage process? Why are less than 50 per cent of our major flood defence systems up to the job? Why have so many of our rivers been straightened in a disastrous attempt to control their flows? Why do local authorities and ministers continue to flout official planning guidelines? (*Daily Mail*, 27 July 2007)

Three days later a commentary in the Guardian reviewed the emergency as it had unfolded in the pages of the press:

> "What went wrong?" asked the Times and the Guardian last week. "It rained a lot" was not the answer they were looking for . . . Such events are traditionally described as acts of God but, despite the efforts

> of the Bishop of Carlisle, the press has written the deity out of the script. "Whose finger is on the nuclear button?" the papers used to ask. Now they want to know whose finger is in the dyke or, rather, who's taken their finger out. The policy issues rose in unison with the waters. "Gordon Brown was under pressure last night," declared the Sunday Telegraph, hopefully. Ministers had been warned in advance, yet flood barriers (which could have saved all of 30 houses) had been held up on the motorway. "Where were the emergency preparations to clear ditches and drains? Where were the sandbags and pumps?" demanded the Mail. "Why are electricity substations and waterworks not being protected?" the Mirror wished to know. "Why doesn't the Severn have diversion channels and water storage areas?" asked Ross Clark, more technically, in the London Evening Standard. All papers wondered why people were living on flood plains, a question which, unfortunately, the Romans weren't around to answer. (*The Guardian*, 30 July 2007)

Where to point the finger here? At the incoming waters? Or at the political neglect? In the words of Wiebe Bijker, "Politics is water, and water is politics" (2005: 512), and yet, as we were sitting through our modelling tutorial trying to get to grips with a key practice in the business of flood management, it was indeed the flow of water, not the course of politics, we were trying to anticipate.

Durham, the previous day: "Being able to relate your perceptual model of the environment to the computer models you will be using is important", stresses our instructor. There are no computers in the room at this point and the attention centres on a flipchart where one of my fellow trainees is drawing up the profile of a landscape. The challenge is to produce a "hydrological cycle" and we are brainstorming on things which could have a place in it. "Clouds", shouts one, "the sea", says another, "some kind of human settlement", "groundwater", "trees", "rain", and "a glacier" is suggested. We end up with a rough sketch of diverse ways in which we think water might be circulating the ecosystem. Effectively, what we have constructed is a world assembled around water. In its very basic form it represents a way of thinking about things without which the science of hydrology would be impossible. Why is the human settlement there? Because it consumes water, and subsequently disposes of it. Why is the glacier there? Because it retains water. Within the hydrosphere of the Earth, water can and must always be accounted for. It is an arrangement summed up in the mass balance equation:

$$P - Et = \pm \Delta S + Q$$

If evapotranspiration (Et) is subtracted from precipitation (P), what emerges is a change in storage (S) plus discharge (Q). In other words: if you want to know how much water is liable to flood you, you essentially want to know how much water comes down as rainfall, and how much of that is either evaporated or retained somewhere. The water unaccounted for is your discharge.

There is nothing trivial about this water world when it comes to anticipating nature. Our hydrological cycle does not agree with Bijker's contention that water is politics and vice versa, on the contrary it enables a world considerably less complicated than that. It is of course entirely possible to argue that if you really wanted to know your risk of flooding, surely you would also want to know if planning guidelines were being flouted, if emergency preparations were being taken, not to mention whose finger was (supposed to be) in the dyke. But our perceptual model grants us the possibility of momentarily leaving that aside. It provides a focus and a way of making sense. Departing from our notes on the flipchart we start exploring: What would generate precipitation? What would affect evapotranspiration? As I begin to sketch out in my field notes from that day (see Figure 8.1), a variety of soil-related properties such as "slope", "drainage", "saturation", "vegetation", and "conductivity" are spun into the assemblage as water makes its way over/through land from rainfall to river channel and we are prompted to ask questions: What is the infiltration capacity of the soil? To which degree will roots conduct water into the subsoil as opposed to leading it over land? How much precipitation will be stored as ground water? How much will be absorbed by vegetation and led back into the atmosphere as evapotranspiration? How long, if ever, before rainwater ends up in a river?

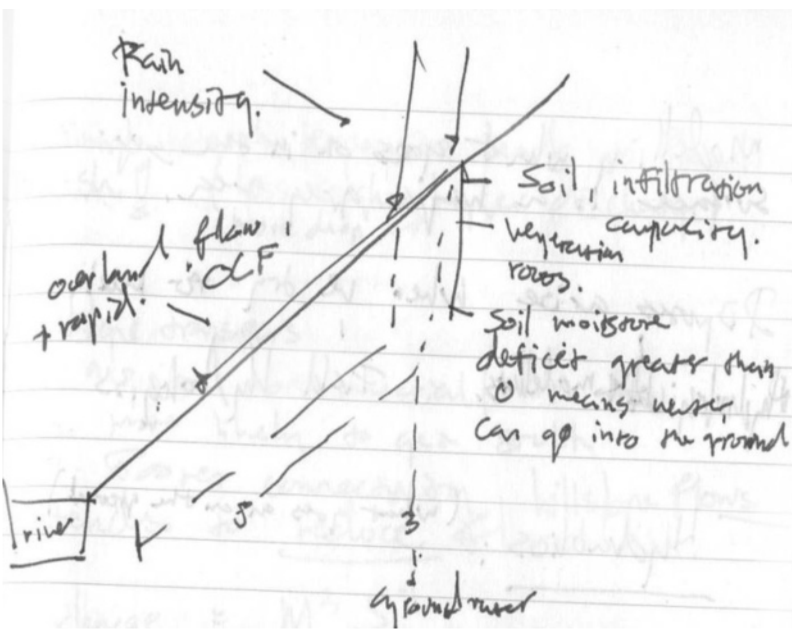

Figure 8.1 Field notes. Water on the ground—conductivity, saturation, drainage, and overland flow.

With the perceptual model in place we can home in on the processes we are interested in pre-empting: those related to the flow, attenuation, infiltration, and evaporation of water. But politics will not be ignored forever. The software we will be working on offers a quite playful way of reintroducing some of the socio-technical complexity that haunts the flood issue in different ways, albeit in a much more hygienic and orderly manner than the way in which it unfolds outside the confines of the modelling office—the crucial difference being that with the birth of our perceptual water world all things political can be safely separated out into their own ontological domain: water will be water, and politics will be politics.

Back in front of the screens: The piece of software we are trying to familiarise ourselves with is called HEC-RAS (the Hydraulic Engineering Center's River Analysis System, or simply HEC in modeller banter). For the past decade it has been one of the most widely used software packages for determining water levels and designing engineered structures in rivers and on flood plains (for a good overview of flood modelling practices see for example Frost & Knight 2002). Operating it is fairly straightforward: it runs in Windows, offers a graphic display of the river system, and features like culverts, bridges, and reservoirs can be easily edited and their effects monitored.

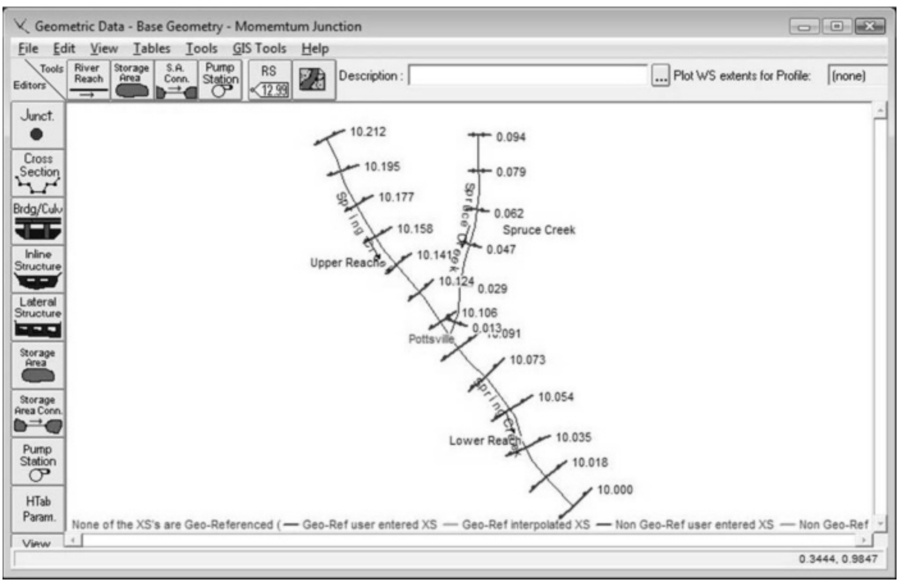

Figure 8.2 Screenshot from HEC-RAS (the Hydraulic Engineering Center's River Analysis System). Overview of a river system; notice the graphic interface, which makes it possible to experiment with different types of engineered solutions along its course.

Programmes like HEC-RAS are built around hydrodynamics but they are also little exploratoriums in which some of the issues concerning drainage, protection measures, and the built environment can be partly taken into account and tinkered with. The way in which this happens is thoroughly modern and "bicameral" (Latour 2004: 49): the model, which claims to speak on behalf of nature, can be asked its opinion on different possible outcomes of a political process, which claims to speak on behalf of society. The answers can of course be fed back across the divide, but it seems important to the professional ethos of the modeller that a divide is maintained in spite of the tendency of successive flood events to transgress any such partition. If our role vis-à-vis nature will be to anticipate, then our role vis-à-vis society will be to notify. Anticipating nature; notifying society.

But it is not simply a matter of us, the apprentices, owning up to the conventions of a trade. Keeping the division intact is important not only to the proficient guise of the modeller, but also to the proficiency itself. The real significance of the perceptual model, and the main reason why we are being introduced to it on the first day of our training course, lies in relating the virtual world of the computer simulation to the world outside it. It serves as a key arbiter when it comes to evaluating and eventually justifying the model outputs and claiming their veracity. As much as the perceptual model ferments a way of thinking about and around water, which will eventually enable us to model, it also provides a sense of what we cannot do with our computer model. The directions in which water moves across the pages of my field diary (e.g. Figure 8.1), the expanse of the geographies sketched there, the cast of actors comprising anything from Atlantic depressions to moisturised subsoil, are all at pains with finding room within a piece of software like the one we will be using. In essence, what I will be modelling during the following days of my training will take place in the bottom left-hand corner of Figure 8.1, namely in the river channel itself and on its immediate flood plain. Contrary to my perceptual model, water will neither be moving vertically nor laterally across the direction of the stream. This is called one-dimensionality and further simplification is necessary in order to achieve it.

The perceptual model is thus not only there in order to give us a sense of what it is we are going to be modelling, but also what it is we will not be able to model. And it does so not only explicitly by demonstrating to us which parts of our newly assembled water world we can expect to be playing around with (what John Law calls the making of manifest absence: that which is "recognized as relevant to, or represented in, presence" [2004: 157]), but also implicitly in the very assemblage of that water world which has made absent all things non-water (Law calls this the making of absent otherness: "that which is absent because it is enacted by presence as irrelevant, impossible, or repressed" [ibid.]). If the explicit process of doing away with complexity in the movement from perceptual to computerized model provides the main epistemological problem to which we must relate our choices and assumptions in order to justify our results, then it is only because the implicit process of assembling an anticipatable water world stripped of its socio-technical entanglements (a bounded domain of nature) has already delivered the necessary framing of that problem. Any anticipation of nature (anticipation understood as professional purpose) must assume that we can already count on nature to be out there as the object and motif of our efforts (anticipation understood as epistemological precept). The first paradox of anticipating nature, then, has to do with the modeller's need to make nature responsible for flooding while at the same time recognizing that it is not. Following from this, the second paradox has to do with the derived need to presume a bounded domain of nature in which claims can be grounded and to which such responsibilities can be attached.

ANTICIPATING NATURE #2: THE PARADOX OF PRECEPT

Durham, second day of the tutorial: One of the first things that strikes me as we are told to start HEC-RAS is the scant simplicity of the interface. Besides the evocative pushbuttons with their flood-related iconography, I am met with a series of blank slates. There is neither river nor water to be seen anywhere. An empty field says "Geometry", another says "Unsteady Flow". I push the buttons only to see more blank slates pop up. In turn, the device will produce a host of numbers, water levels, which can even be animated as virtual flood events as experienced in the introduction to this chapter. Before we can get to that, however, we will need to provide the software with something to flood and something to flood it with: it needs a floodplain, a channel, and some water, or rather: it needs "geometry" and "flow".

Step one in "running the model", or what is sometimes just called "modelling" (not to be confused with the "modelling" referring to the process of building a new model),[2] is the construction of a geometry. Built up as a series of cross-sections lined out along a stretch of river, successive stages through which the flow will eventually pass, it offers a first impression of what one-dimensionality entails (Figure 8.2 provides an example of this way of conceptualizing the river system in one dimension). As opposed to an actual river, this virtual version will only allow water to move in one direction, namely downstream from cross-section to cross-section, vertical and lateral motions being absent. A cross-section is constructed as a series of data points, each with a "station" and an "elevation" coordinate specifying the lateral and the vertical location of the point respectively. Here is a first, but fundamental, transformation of the actual river and its flood plain: in order to turn it into geometry it must be reduced to points. Had this not been a training exercise this task would have involved the mobilization of survey instruments and charts. We, however, are supplied with readymade data by our instructor and can simply key them in. Figure 8.3 shows a window open with a cross-section consisting of 12 such data points which have been keyed in to the columns on the left. The elevation and the station coordinates are specified for each of them and point 5 and point 8, at station coordinates 54 ft and 96 ft, have been assigned as the main channel banks. This is very important: water levels above this point (21.15 ft elevation) are how the model will now construe "flooding".

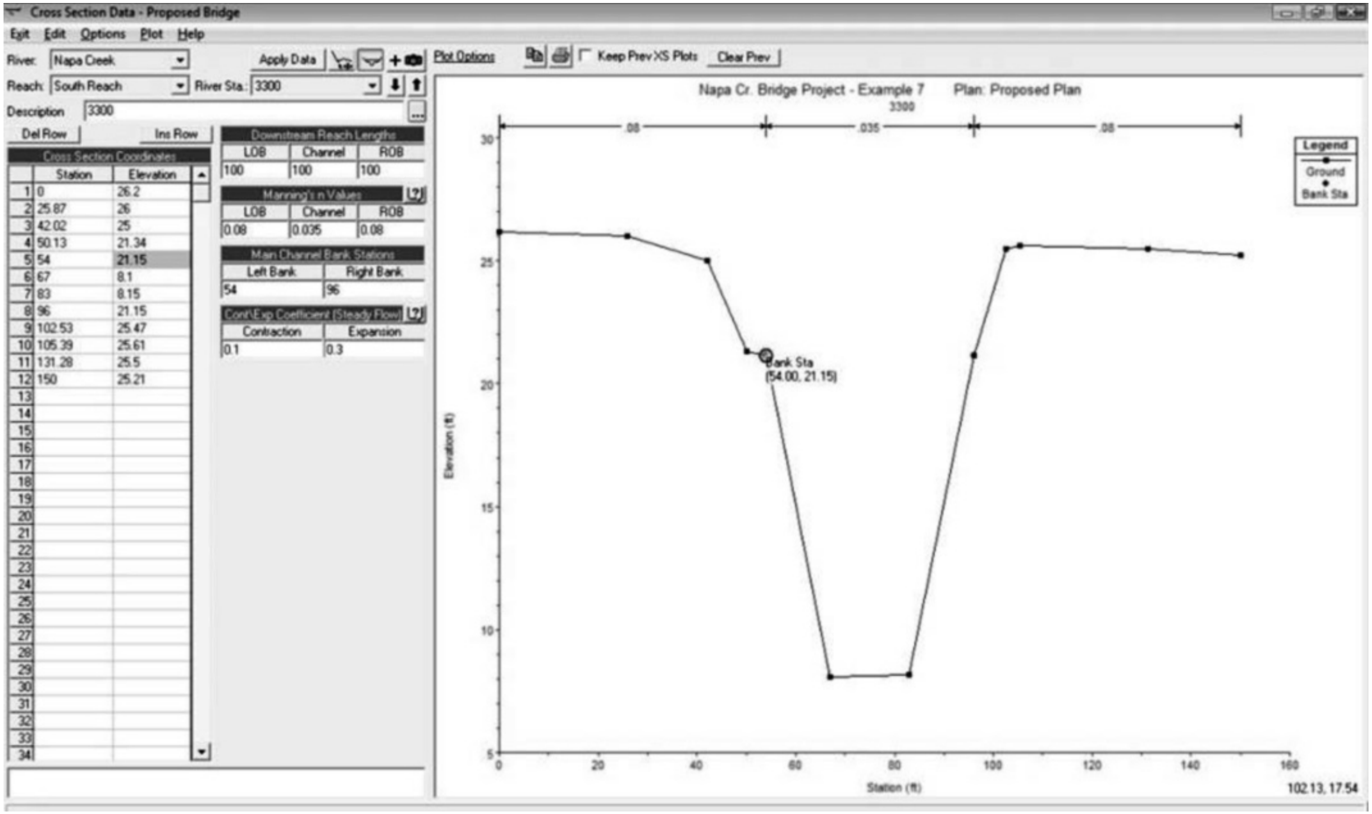

Figure 8.3 Cross-section data. The left bank station is highlighted at 21.15 ft elevation, which is the height at which the flow of water becomes "flooding".

Providing HEC-RAS with this stylized geometry of a river channel cross-section gives a first impression of the reductive work that characterizes the movement from perceptual to computerized model. We have to decide both where and how often to put in a vertical section across the stream and the resulting topographical slices must then become data points, again involving reductive choices about location and resolution. The perceptual model puts no restriction on the movement of water, for example, but that is an impossible degree of complexity for HEC-RAS and we will have to justify the way we build up our geometry in relation to this simplification. Nonetheless, our perceptual model already represents a world reduced to the movement of water through a landscape (an anticipatable nature), which gives the task of further simplifying that landscape into the one-dimensional geometry of HEC-RAS a clear and tangible format. In this way a clearly bounded nature must be anticipated as a precondition for the modelling enterprise, not least when we move on to the next step of our tutorial, in which we have to flood the geometry with something.

Water is fed into the model in the form of "hydrographs": the actual flow of the river must become points in time with corresponding discharges (ft^3/s). By specifying the so-called "boundary conditions"—what happens, that is, before the first and after the last cross-sections in the system—we first tell the model at what rate the water comes in, and at what rate it will be able to get out again. Figure 8.4 shows a plot of a hydrograph with the increase and subsequent decrease in discharge over time at a specific cross-section. I am not aware of it yet, but by treating water in this way, as mass and momentum, I am aligning it neatly with the hydrodynamic equations constituting the machinery of the software. Like petrol for a petrol engine, refined into something quite different than the crude oil from which it came, I can now pour hydrographed water onto my geometrized landscape.

The key components of this high-octane virtual water, mass and momentum, is packed into the letter Q, discharge, shown on the vertical axis of Figure 8.4 as flow in ft^3/s. As I key in the boundary conditions I am embodying this very particular notion of water as a relation of speed and mass. I am complying with the machinery—a compliance which cannot be taken for granted. According to the historian Asit Biswas, Q was not always so servile and attuned. To the Roman emperor Nerva's commissioner of waterworks, for example, discharge was a far more refractory concept. Or rather, it was supposed to be simple, equalling the width times the depth (the cross-sectional area) of the flow (Q=A) but as the commissioner was making plans for the water supply he encountered a persistent misfit between his calculations and his observations. There was simply not the same amount of water coming out of the aqueducts and fountains as he had estimated. Discharge was, to use the Stengerian metaphor of "dialogue" (Prigogine & Stengers 1985: 4), talking back, putting Q=A at risk in the process. A compromise was reached by introducing "unknown leaks" and "ordinary Romans" who, in the words of Biswas, "seemed to be experts in

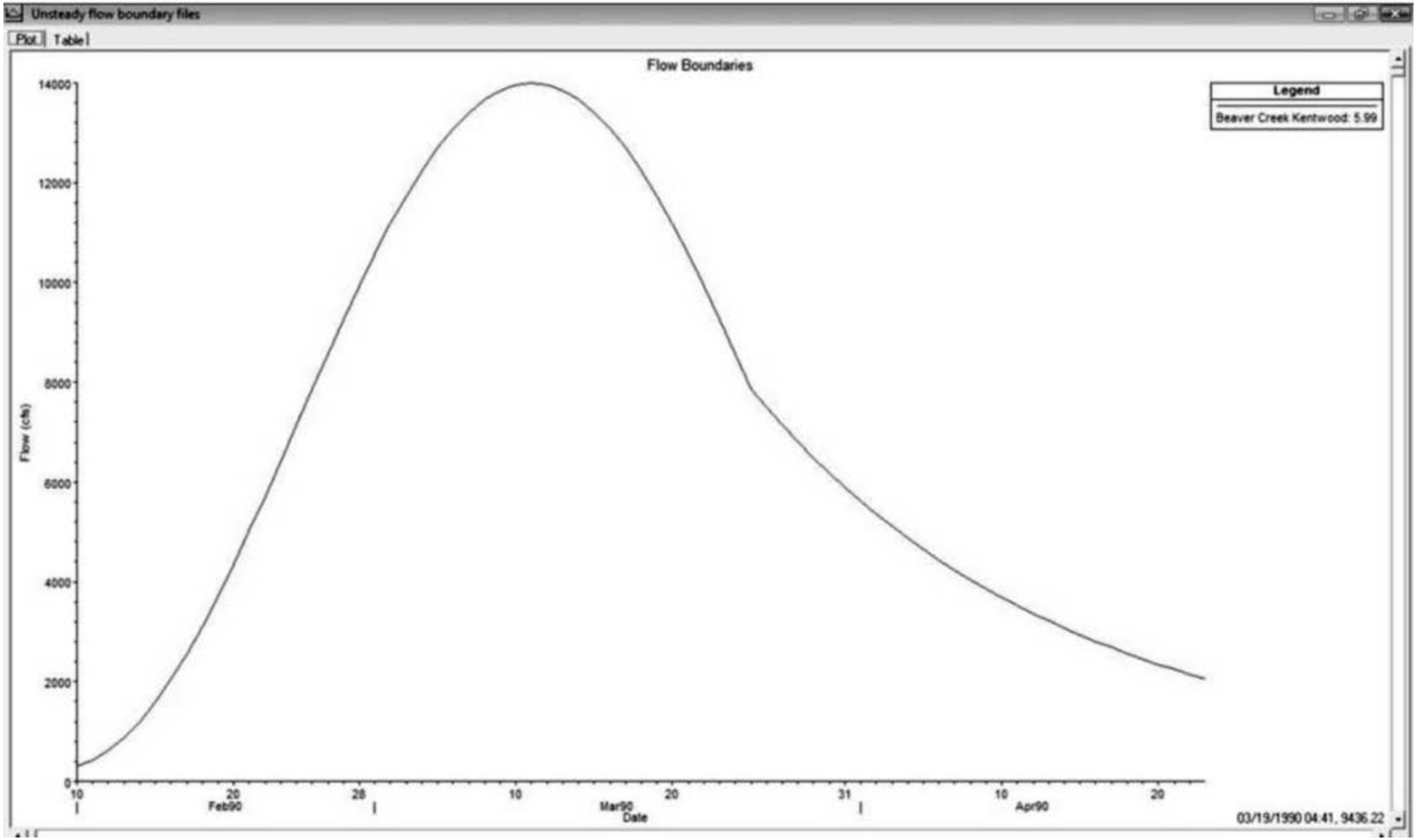

Figure 8.4 Flow hydrograph with discharge as flow in ft³/s (Q=A·V) shown on the vertical axis.

tapping water without 'bothering' the authorities" (1970: 71). As the commissioner and his measurements could not on their own make Q=A further allies had to be drawn in as buttresses: the statement would only make sense under the condition that a mix of shady transactions and reputed wastage provided the necessary constructional leeway. Hence propped up, Q remained A until the end of the 16^{th} century where, after the flooding of Rome in 1598, Giovan Fontana da Meli undertook the task of tracing the water flowing into the Tiber from its system of tributaries. At any given point he did this by measuring the width and the depth (A without V) of a cross-sectional slice of the flow.

The perseverance of this assemblage of discharge is somewhat puzzling, not just because of the effort it took to maintain it, but because of what it backgrounded in the process. Alternative dialogues on the measurement of Q were taking form simultaneously already in the first century A.D. The Greek scholar and mechanical engineer Hero of Alexandria, for example, had such a conversation with a sundial and a lead pipe in the Egyptian desert where he was testing the use of siphons for irrigational purposes and needed a way to determine the amount of water supplied by a spring. In other words: he needed a measure of Q. The lead pipe, which was rectangular in shape, could be used to divert the water into a dug-out reservoir which allowed for both the measurement of width and depth of a flow cross-section and the amount of water supplied. Had it not been for the sundial, Q could in principle have remained conflated with A, but with the introduction of time it became clear that the reservoir filled up in different tempi, even though A was constant:

> It is to be noted that in order to know how much water the spring supplies it does not suffice to find the area of the cross-section of the flow, which in this case we say is 12 square digits. It is necessary also to find the speed of the flow, for the swifter is the flow, the more water the spring supplies, and the slower it is, the less. (Hero; quoted in Biswas 1970: 87)

What was invented, namely velocity (V), was essentially a replacement for the thievish Romans and the leaky pipes needed to uphold the simple relation of Q=A. Instead Q could now be A·V without any further reinforcements. Curiously, this did not happen until 1628 and when I go to the trouble of historicizing the concept of discharge and exhibiting some of its contingency it is to make clear that as I key in the boundary conditions I am embodying a very particular notion of water as a relation of speed and mass which cannot be taken for granted. Providing the model with a hydrograph entails both an explicit simplification process from perceptual to computerized model and an implicit simplification represented by the perceptual model and its anticipatable nature. Deciding on the boundary conditions thus involves taking everything that does not take place in the bottom right-hand corner of Figure 8.1 and turning it into an estimated input of water. That means deciding on a discharge by making assumptions about things like rainfall, evapotranspiration, and storage. This is the making of manifest absence and forms part of the way we justify our modelling. What the historicity of a concept like discharge shows us, however, is that the frame within which we conduct these final simplifications into hydrographed water is itself a result of negotiations and successive attempts at stabilization. If the perceptual model supports the idea that modelling is about anticipating nature, then it is because it enables the notion of a nature thus bounded. The somewhat prosthetic character of this nature is not just historical but something that is ongoing and requires work. Discharge is merely a component in the formalized version of this bounded nature which makes up the hydrodynamic equations that lie under the hood in HEC-RAS and these equations have their own anticipations: they expect us to keep translating the world into a computable format.

ANTICIPATING NATURE #3: THE PARADOX OF PRESCRIPT

What is so far missing from my account of trying to make the model run is some sort of engine. I have learned to turn a riverine landscape into geometry and I have tentatively mastered the refinement of water into discharge, but for what reason? It is in the engine room of HEC-RAS that the reduction to one dimension is orchestrated, and it is subsequently here that the specs for hydrographs and geometries are churned out. The machinery at work is composed from the Newtonian principles of mass and momentum conservation. They come formalized and calculable by a computer in the form of the hydrodynamic Saint-Venant equations, which basically relate variables like cross-sectional area (A), time (t), incoming discharge (Q), length of river (x), additional flux (q), slope (S), velocity (V), or gravity (g) to one another in order to catalyse the model run. Mass conservation, for example, dictates that the amount of water that goes into the model must be equal to the amount of water that comes out of it, and this dictates the relationship between the station/elevation data and the hydrograph, which I keyed in. If the channel gets narrower, water levels will have to come up, and vice versa. Similarly, momentum conservation obliges the model to keep the velocity of the hydrograph stable unless the slope or the roughness of the channel warrants otherwise. The important thing is that these differential equations are absolutely powerless without us, the modellers. They relate elements, but they do not render these relationships calculable. For example: it might be that the time it takes water to flow (v) through a certain cross-sectional area (A) could change—which would allow discharge between two points to change without compromising the principle of mass conversation—but before such a change could be modelled, the world would have to be translated not only into these expressions, but into versions of them which have calculable values.

Between the columns with station/elevation coordinates in Figure 8.3 and the graphic outline of the cross-section are some further tables to be filled out. One of them is titled “Main Channel Bank Stations”—this is where we have to decide on the limits for what should count as flooding; another is asking for “Manning’s n Values”. This n value, our instructor tells us, represents a “roughness coefficient”. But whereas plotting the cross-section as a series of coordinates seemed if not straightforward then at least a familiar way of treating a landscape—echoing embodied experiences with maps and orienteering, for example—I have no sounding board for “roughness coefficients”; it evokes no response. Manning’s n, it turns out, serves as a kind of remedy for the smooth sterility of the straight lines making up the geometry with little resemblance to the irregular, deposited, and vegetated condition of floodplains and riverbeds. What n conveys to the model is a notion of things like trees, brush, grass, tarmac, debris, gravel, ruggedness, cleanliness, messiness ,or smoothness, and it sums it all up in a measure of “roughness”. (The story of how Manning’s n, named after the Irish engineer Robert Manning who proposed it in 1889, became the preferred measure of roughness in hydraulic engineering, though in some respects an unlikely candidate, is convincingly told by Whatmore & Landström [2009].) In Figure 8.3 the n-value has been set to 0.035 below the banks and 0.08 above them. According to HEC’s hydraulic reference manual this means “clean, straight, full, no rifts, or deep pools” but with “more stones and weeds” in the channel, and somewhere between “light brush and tree in summer” and “heavy stand of timber, few down trees, little undergrowth, flow below branches” on the floodplain (Brunner 2008/ ch 3:14). In a still-quoted book from the 1950s on the subject of “Open-Channel Hydraulics”, the process of coming up with a roughness coefficient was envisaged like this:

> At the present stage of knowledge, to select a value of n actually means to estimate the resistance to flow in a given channel, which is really a matter of intangibles. To veteran engineers, this means the exercise of sound engineering judgement and experience; for beginners, it can be no more than a guess, and different individuals will obtain different results. (Chow 1959: 101)

What this “sound engineering judgement” more precisely refers to remains unclear, but it could be speculated that a disciplining of the body is also involved here. Some guidelines are suggested for the uninitiated:

> (1) to understand the factors that affect the value of n and thus to acquire a basic knowledge of the problem and narrow the wide range of guesswork, (2) to consult a table of typical n values for channels of various types, (3) to examine and become acquainted with the appearance of some typical channels whose coefficients are known, and (4) to

> determine the value of n by an analytical procedure based on the theoretical velocity distribution in channel cross section and on the data of either velocity or roughness measurement. (Chow 1959: 101)

Choosing an n value is still done predominantly by analogy and with the aid of guidebooks and tables like the one in the HEC-RAS manual quoted above. Chow's book itself contains an early example of a photographic reference with n values progressing through a set of 24 riverine scenarios. The first photo represents the n value 0.012 (minimum roughness) and depicts a smooth surfaced concrete slab canal. In the fourth photo the concrete canal is covered with a fine layer of algae and drifting sand—n increases to 0.018. Towards the last photo, and an n value of 0.150, the irregularity increases, more vegetation blurs the transition from river channel to flood plain, and on photo 24 trees have fallen over into the river and debris is drifting along the bed (Chow 1959). For the purpose of our training session, where we are simply told what values of n to use, no attempt is made to develop our potentially foetal "sound engineering judgement". Yet, it does become clear to me that rivers can have "roughness", a concept which was not on my radar before I embarked on this exercise. Obviously, having seen rivers, I knew about vegetated banks or gravelish beds, but I never thought about them in terms of attenuation before. One thing is learning to assign the correct value of n, but it presupposes learning to think about the concept of n in the first place. The cross-section data sheet in Figure 8.3 requires a mental or bodily correspondent to make sense as it cannot, on its own, perform the task of turning gravel into roughness coefficients or channels into station/elevation data.

Tied in and related through a set of formalisms the composites of the model are made to do something meaningful. Geometries and flows have been grinded to support this meaningfulness, the model seems to close itself around a working whole, the reification of a specific subset of hydrodynamics. I know from the hydrological cycle on the flip chart and from the perceptual sketches of the environment in my field notes that things are missing. Everything is not here. But these are absences for which I have been taught how to account—the co-conspiring parts of an arrangement to make the model speak on behalf of a world much wider than itself. As our instructor points out, there would be no point in models if they were replicating in every particular the things modelled. The point in models, in other words, is their transformation of the things they model into simpler forms. It is this specific and purposive process of doing away with things which I have been able to engage in through my apprenticeship.

So, it is clear that HEC-RAS is not working without me. Obviously so, in the sense that unless I start the programme and make use of it, it will remain inert, but also perhaps less obviously in the more radical sense that HEC can be no more of a flood model without me, than I can be a flood modeller without HEC. What we (me, the trainee, and the software) have stepped into—training to plot channels as data points, turn vegetation into n values, and regard water as mass and momentum—is a transformative process in which hyphenated model-modellers are becoming. “Human bodies have been transformed by and into a horse’s body”, writes Vinciane Despret (2004:115) on the somewhat different, yet related example of skilled riders. “Who influences and who is influenced, in this story, are questions that can no longer receive a clear answer” (ibid.). The first notes I have made of cross-sections and hydrographs in my field diary do not have the neat finish of the HEC-RAS interface, of course; in that respect they are distinctly human, but they are not too different from it either. Despret uses the term “anthropo-zoo-genesis”—it might be rephrased as “anthropo-techno-genesis” to fit the context. The point is that through my apprenticeship I not only developed a new way of thinking and talking about modelling, but a way of amplifying “other sensory, bodily or affective registers” (Whatmore 2006: 607) which eventually enabled the model-modeller to model.

What I am trying to account for, then, is the way in which this anthropotechnic hybrid has become effective. As noted, efficiency relies on absence, on limiting the extent of the model so as to make it model rather than replicate. What emerges from my experimental engagement in the becoming of a model-modeller is the location of what seems to be a disjuncture between perceptual and computational model. There are key moments at which things are left out in order to “circulate reference” (Latour 1999) between the hydrodynamic equations and the riverine environment, and some of them, the moments where the modeller becomes able to motivate his choice of model by leaping across the gap into one-dimensionality, seem particularly interesting. They contribute to a feeling that the disjuncture between perceptual and computational model might not be so much of a disjuncture after all. There is a definite sense of continuity in translating vegetation into roughness and water into discharge, but it requires us to think about the model not only as a set of equations which have been tweaked, discretized, and parameterized into a functioning piece of machinery with a navigable interface, but as a constellation including human hard- and software (flesh, skill, perception, judgement) as well. Here, it seems, is the focal point of the anthropo-techno-genesis: the acquisition of the translational capacity to render the world in formats which can actually be modelled.

EMANCIPATING NATURE

The great irony of this suggestion is of course that any nature which can either be anticipated or have anticipations of its own—any clearly demarcated nature in the style of the bicameral collective—relies on a thoroughly post-natural hybrid to sustain itself. The paradoxical outcome is that the act of anticipating nature in any ontologically discrete form requires the emancipation of nature from being thus bounded. In the words of Michel Serres:

> Floods take the world back to disorder, to primal chaos, to time zero, right back to nature, in the sense of things about to be born, in a nascent state. (Serres 1995: 51)

No doubt a poignant metaphor when we are talking about actual floods and their destructive/creative effects, but it has some purchase on the virtual condition of flood modelling as well. In order for the model to model, things have to be born outside the confines of a prosthetic and anticipatable nature. And essentially this deliverance represents an altogether different kind of nature; a nature emancipated from any sort of pre-emptive ambitions, grounding capacities, or prescriptive demands; a nature in the sense of things about to be born. In this chapter I have described the birth of a model-modeller and its anticipations. Although it passed away in its very early infancy—I only ever became an informed novice in the art of flood simulation—it hopefully provides a glimpse of the political ecology on which the professional anticipation of floods rely. Claiming that a simulation is "realistic", that "this could happen", as an informant once put it, demands the implication of, and exerts the transformation of, a constituency far beyond micro-processors, discrete equations, and hydrological cycles.

NOTES

1. As a group we had been brought together by the RELU-funded Understanding Environmental Knowledge Controversies project (see also Whatmore 2009; Lane et al. 2011b; Lane et al. 2011a). I would like to thank the other members of the team for their valuable comments, their engaging attitudes, and for bringing me up to scratch on all aspects of the UK flood issue: Sarah Whatmore, Catharina Landström, and Gillian Willis at the University of Oxford, Stuart Lane and Nick Odoni at Durham University, Neil Ward at the University of East Anglia, and Sue Bradley at Newcastle University.
2. In her analysis of event generators used in particle physics, Martina Merz (1999) deliberately omits the word 'model' when referring to the software. Instead she reserves the term for the programmers' conceptual idea of the physics they are trying to put to work in the form of computer code in order not to blur the distinction. It could very well have made sense to do something similar here. However, my fluctuating and less well-defined use of the word 'model' reflects the multiplex usage encountered in the field.

REFERENCES

ABI 2000. Inland Flood Risk: Issues Facing the Insurance Industry. London: Association of British Insurers.
ABI 2007. Summer Floods 2007: Learning the Lessons. London: Association of British Insurers.

Anderson, B. 2006. "Becoming and Being Hopeful: Towards a Theory of Affect." Environment and Planning D: Society and Space 24, 733–52.
Bijker, W.E. 2005. The Politics of Water: A Dutch Thing to Keep the Water Out or Not", in B. Latour & P. Weibel, Making Things Public: Atmospheres of Democracy, Cambridge, MA: MIT Press.
Biswas, A.S. 1970. History of Hydrology. Amsterdam: North-Holland Publishing Company.
Brown, J.D. & S.L. Damery 2002. "Managing Flood Risk in the UK: Towards an Integration of Social and Technical Perspectives." Trans Inst Br Geogr 27, 412–26.
Brunner, G.W. 2008. HEC-RAS River Analysis System—Hydraulic Reference Manual. Davis, CA: US Army Corps of Engineers.
Chow, V.T. 1959. Open-Channel Hydraulics. New York: MacGraw-Hill
Crichton, D. 2005. "Flood Risk & Insurance in England and Wales: Are There Lessons to Be Learned from Scotland?" Befield Hazard Research Center technical paper.
Crichton, D. 2008. "Role of Insurance in Reducing Flood Risk." The Geneva Papers 33, 117–32.
DEFRA 2004. Making Space for Water, Developing a New Government Strategy for Flood and Coastal Erosion Risk Management in England: A Consultation Exercise. London: Defra Publications.
Despret, V. 2004. "The Body We Care For: Figures of Anthropo-zoo-genesis." Body & Society 10, (2), 111–34.
EA 2009. Flooding in England: A National Assessment of Flood Risk. Bristol: Environment Agency.
Frost, L. & D. Knight 2002. "Catchment and River Basin Management", in G. Fleming, ed. Flood Risk Management: Learning to Live with Rivers, London: Thomas Telford, (p.51–90).
Kellman, I. 2001. "The Autumn 2000 Floods in England and Flood Management." Weather 56 (10), 346–60.
Lane, S.N., C. Landström & S. Whatmore 2011a. "Imagining Flood Futures: Risk Assessment and Management in Practice." Philosophical Transactions of the Royal Society 369, 1784–1806.
Lane, S.N., N. Odoni, C. Landström, S. Whatmore, N. Ward & S. Bradley 2011b. "Doing Flood Risk Science Differently: An Experiment in Radical Scientific Method." Transactions of the Instititute of British Geographers 36, 15–36.
Latour, B. 1999. Pandora's Hope: Essays on the Reality of Science Studies. Cambridge, MA: Harvard University Press.
Latour, B. 2004. Politics of Nature. Cambridge, MA: Harvard University Press.
Law, J. 2004. After Method: Mess in Social Science Research. Abingdon: Routledge.
Merz, M. 1999. "Multiplex and Unfolding: Computer Simulation in Particle Physics." Science in Context, 12, 2, 293-316
Pitt Review 2008. Learning Lessons from the 2007 Floods. London: The Cabinet Office.
Prigogine, I. & I. Stengers 1985. Order Out of Chaos: Man's New Dialogue with Nature. London: Flamingo.
Serres, M. 1995. Genesis. Ann Arbor: The University of Michigan Press.
Strathern, M. 1992. After Nature: English Kinship in the Late Twentieth Century. Cambridge: Cambridge University Press.
Whatmore, S.J. 2002. Hybrid Geographies: Natures Cultures Spaces. London: SAGE Publications.
Whatmore, S.J. 2006. "Materialist Returns: Practising Cultural Geography in and for a More-Than-Human World." Cultural Geographies 13, 600–609.

Whatmore, S.J. 2009. "Mapping Knowledge Conroversies: Science, Democracy and the Redistribution of Expertise." Progress in Human Geography 33 (5), 587–98.

Whatmore, S.J. & C. Landström 2009. "Manning's n: Putting Roughness to Work", in M. Morgan & P. Howlett, eds. How Well Do Facts Travel, Cambridge: Cambridge University Press.

9 Modelling Ice

A Field Diary of Anticipation on the Greenland Ice Sheet

Martin Skrydstrup

Prediction is very difficult, especially if it's about the future.

Niels Bohr

THE QUESTION

Should we defer human decision-making about our environment to the authority of climate modelling? When we raise this general question about environmental epistocracy with reference to large-scale climate models (so-called "general circulation models" or GCMs) the problem becomes one of the credibility and trustworthiness of predictive climate modelling and whether political decisions about the environment can be based on such models (Hulme & Dessai 2008; Hulme, Chapter 2, this volume). In this chapter, I raise the same general question about predictive modelling and decision-making, but situate the question, as you will see, in a nascent and elementary form: a small community of only 36 people completely isolated from the rest of the world, situated on the ice sheet of Greenland. In the summer of 2010, this community was trusted with the daunting task of drilling through the ice cap and hopefully reach bedrock before the 90-day field campaign was over and the weather would make drilling activities impossible. Drilling deep polar ice cores is a high-risk undertaking, because "there is a split second between success and failure", as the field leader explained to me. The drill can get stuck; weather conditions can cut supply lines; sickness can easily spread; and people can get injured while working with the saw or other heavy-duty equipment in an extreme Arctic environment. What makes up the bulk of this chapter is a field diary about the decisions and course of events taken by the NEEM community facing the risks mentioned above. I try to convey the moods and moves of the NEEM community, which in the face of incalculable risks tied to the unforeseen in the abyss of the borehole decided to deploy predictive modelling to inform decision-making about their future action. As we shall see, the inconclusive result of this predictive modelling experiment on the ice was overridden and rendered obsolete, by the community's own emplotment of the sequence of events and ultimately by its own definition of nature. In this sense, this chapter is a modest attempt to further qualify and push Kirsten Hastrup's argument that modelling is a form of "experimental expectation," which is never outside the realm of the social (Hastrup, Chapter 1, this volume).

STS, ANTHROPOLOGY, AND CLIMATE MODELLING

What do we know about the role of models in science and more specifically, climatology? In the Science and Technology Studies (STS) literature, models have been seen to bridge much further afield than between experiment and theory and have attracted sustained scholarly attention, because models seem to be at the cross-roads of the science-policy interface (Petersen 2000). This seems obvious in the sense that models are fed with scientific observations and run on algorithms, but produce visualizations and predict various collective futures for society at large. This perhaps comes to the fore in climate science, where "modelling has emerged as a fundamental organizing principle for the global epistemic community that surrounds the climate change issue" (Sundberg 2007). However, modelling is also seen as a form of boundary-making in and of itself. Sundberg shows how such boundary-making is at work in what she calls "subworlds" in meteorology (Sundberg 2006). "The view of modelling as 'theoretical' and experimentation as 'technical' associates modelling with conceptual work and experimentation with less prestigious gadget work (hands-on work) and it is another illustration of boundary work that serves to demarcate between modelling and experimenting" (Sundberg 2006: 60). Sundberg argues that: "The constant re-production of the boundary between modelling and experimenting is salient in the scientists' accounts" (ibid.: 63). Thus, paradoxically, climate models are said to embody both the bridge and the boundary between two worlds conventionally kept apart, science and politics; theory and experiment.

More specifically, the STS literature on modelling has established that modelling has produced a number of abstractions such as the "global mean temperature" (ibid.) and I would be tempted to add the "2-Degree Threshold", although such an argument would require a book to develop. However, most work seems to target modelling as a highly contingent and insecure practice. Critical perspectives on the indeterminacies and uncertainties of climate modelling practices have been exposed and established (Shackley et al. 1998; Winsberg 1999; Lahsen 2005). Brian Wynne has argued that the grand question about whether general circulations models (GCMs) actually can predict the future still remains unanswered: "The original perfectly explicit founding question, 'Is long-term climate prediction scientifically do-able?' has been answered by default, and is no longer explicitly posed. Strictly speaking, we still do not know the answer" (Wynne 2010). Tightly connected to this issue we have important work on the evaluation and performativity of climate models (Oreskes et al. 1994; Edwards 1999). Ethnographies have focused on the computer model/human agency interface, such as Gary Allan Fine's ethnography of meteorology as "public science" foregrounding the interrelations between model accuracy and human skill as being at the heart of forecasting practice (Fine 2007). Related to this line of work, we also have significant work on the credibility and authority of modelling, where prominent scholars have pursued the question of how climate models gain and exercise authority (Jasanoff 2004; Lahsen 2005; Hulme 2010). One of the unresolved problems in the STS literature is whether and to what extent modelling is a "predictive truth machine" (to borrow from Brian Wynne) or a declaration of truth and is best studied as coupled to collective futures of society at large, or as a "useful heuristics/tool", that is as "technolog[y] for investigation" (Morgan & Morrison 1999) and most adequately studied as a technological artefact in its own right with a relative autonomy. Leaving this issue aside, there is wide consensus in the STS literature that climate models both are boundary-making devices between observation/experimentation and theory/simulation, and they also instantiate the relations between science and society. The STS literature has also brought home that climate models do not spring from the head of Zeus; they are man-made, inherently unstable, and each and every one of them has a particular history and is embedded in various epistemic communities.

If climate modelling seems the home turf of STS, anthropology has been rather reluctant to take on the issue. One rare exception is Anna Tsing's groundbreaking work *Friction* (Tsing 2005), which touches upon the cultural features of global climate models at some length. Her first observation is that "the global scale takes precedence—because it is the scale of the model" (ibid.: 103). This finding resonates with the STS literature, where it is argued that modelling produces abstractions such as "global mean temperature". Her second observation is that "models breed more models" (ibid.: 104), because models are made more reliable by way of incorporating uncertainties into them, which implies remodelling them. Finally, she observes that "models must be charismatic and pedagogical" (ibid.: 105), because the success of climate modelling depends upon involving policymakers into the simulation of modelling. Generally, Tsing's observations are in line with the STS perspectives pitched above and emerge from on-the-fly observations in conference settings.

The approach I advocate here differs somewhat from the STS literature and Tsing's pioneering work insofar as it revolves around an ethnography of the social life of modelling in real-time scientific practice. We shall see that timing and temporality is the name of the game. Predictive modelling is deployed in a small scientific community to master the immediate future in an extreme environment (the Greenland ice sheet), facing incalculable risks. The community itself identifies these risks as tied to the drilling process (the drill can get stuck, drilling fluid can ruin the integrity and interpretation of the data, etc.), which may threaten the success of the entire scientific project. However, there are other risks, such as hazardous working conditions, extreme weather conditions, and a labour-intensive project, carried out in an extreme environment, where people cannot survive very long without the aid of technology. The itinerary follows the chronology of the real-time ethnography: first we encounter informed guess work in the form of betting about the bedrock; then predictive modelling is deployed, which unfolds as a debate about interpretation of the inherent “noisy data” from the borehole. Noisy here means signals, which according to my interlocutors carry “stochastic variables”, i.e. information which is randomly generated by the experimental set-up. The interpretation and modelling of the signal from the borehole takes the form of a debate between three key interlocutors: a modeller, an engineer, and a scientist. Ultimately, the results of the predictive modelling are rendered insignificant and irrelevant by the social life in camp and ultimately by the community’s own definition of nature.

THE GRAND NEEM BEDROCK BET

When I arrived at the NEEM camp towards the end of June 2010, anticipation was already running high. This anticipation had been built up by three consecutive field seasons. In the summer of 2007, a select group from the Glaciology Group at the Niels Bohr Institute made a transverse on the ice cap from the previous NGRIP (North GReenland Ice Core Project) drilling site to the new site further north called NEEM (North EEMian Ice Core Drilling Project), where they constructed a provisional camp and a runway for flights. During the next field season in the summer of 2008, the NEEM camp was built and a number of VIPs were invited, such as Thomas Friedman from the *New York Times*, EU minister of Climate and Energy, Connie Hedegaard, and the Chairman of the IPCC Rajenda Pachauri, among others. The heat was on and the scientific community now anticipated groundbreaking new climate science from the NEEM project. In the next field season, drilling got underway and on 14 August 2009 a new world record in ice core drilling was set with 1,757.84 metres of ice core in one season.[1] When the anthropologist arrived towards the end of June 2010, the drillers’ depth was 2,270.01 metres and with an expected depth to bedrock in the range of 2,500–600 metres, climax was close and excitement and anticipation ran high in camp.

With the arrival of my Hercules plane, the camp population peaked at 36 people—and the balance between work and leisure in camp shifted somewhat in favour of the latter. The weather was bright and clear and recreational activities blossomed. People played volleyball in T-shirts, skied cross-country on prepared tracks, flew kites, and a select few even went kite-skiing after dinner, when the wind conditions were right. A good glass of Bourgogne and hot chocolate from Switzerland were enjoyed on the benches outside the Main Dome in the company of novels, comics, flashy magazines, and sci-fi literature. During my first two weeks in camp, the camp climate seemed closer to a playground in the Swiss Alps around Easter time than what I had imagined a research base in the High Arctic would look like.

Below the life of surface leisure, 20 metres under the ice, the ice core drilling was progressing smoothly. On a good day, the drilling team made eight runs from the trench to the bottom of the borehole per day and production peaked at over 20 metres of ice core per day. Now and then, the drilling team encountered difficulties, such as the "chips" (by-products of the drilling) filling the borehole, or the gradient of the drill would exceed three degrees, which made the pull on the wire cable come close to the lower tolerance level. However, through their long experience from other drillings, the team overcame the challenges with ingenuity and the technical skill of selecting the appropriate cutter on the drill for the shifting properties of ice in the hole. Keeping up with the high production rate throughout the first two weeks of July, the NEEM team broke new records in ice core drilling.

By mid-July, the morale in camp was high. With an estimated 200 metres of ice left to drill, the camp population seemed confident that they would experience the historical moment of hitting bedrock—the benchmark of the operational science plan and the more popular success criterion of the project in the eyes of national funding agencies and world media. In this mood of anticipation, the "Grand NEEM Bedrock Bet" opened for entries. On a grand chart nailed to the billboard by the stairs in the Main Dome, each member of the NEEM community could enter her or his estimate of the date we would hit bedrock and the logger's depth. Bets could be scribbled down in camp or emailed from afar. The chart shared "relevant information", such as the radio echo sounding images, which estimated the depth to 2,545 (+/-20 metres),[2] the current production curves of the NEEM drilling, the temperature in the borehole, and the estimate of the basalt-melting rate.[3]

Almost every time I passed the Grand NEEM Bedrock chart, a new bet had been entered and people would stop for a while to carefully study it. Before entering my own bet, I examined the 40-somewhat entries on the chart carefully and discussed the information with a couple of scientists. My interlocutors gave me very technical advice about the temperature in the borehole and the many difficulties of drilling close to bedrock, where the drill could get stuck, because the ice was almost at the melting point, due to the proximity to the bottom, from where heat radiated. However, when I studied their entries, they seemed less correlated with the technical data that they explained to me, and more of an arbitrary assessment. In fact, studying their entries a pattern seemed to emerge: the date they were scheduled to leave camp was the date they had singled out as their best estimate for when we would hit bedrock. Not everybody, though; one technician had entered a very different date, namely the beginning of the next field campaign in 2011 (4 May 2011 to be precise). However, generally there was a consistent correlation between camp exit date and bedrock-hitting date. To the anthropologist, it seemed as if the corroboration of technical information was overridden by the social fact of their camp exit date in their anticipation of nature.

By Sunday, 11 July at midnight, NEEM time, the PI in camp took the chart down and announced the next day at lunch that the "Grand NEEM Bedrock Bet" was now closed for entries and that the winner could hopefully be crowned in November at the Steering Committee Meeting in Copenhagen. Then, the weather began to get worse. On 14 July people in camp were awake most of the night, because the storm was howling so loudly, and I myself feared that my tent would be blown away. When I went out that morning, the camp-scape had changed; even the toilet hole was gone and virtually unidentifiable. The storm continued and in the Main Dome over breakfast, people began to discuss whether it qualified as a so-called "whiteout".

The drilling slowed down. The drillers produced about one metre per run, which only made about five to seven metres a day. Not because of the weather, since the drilling trench was entrenched 20 metres below the howling wind and thus immune to weather conditions. The drillers estimated they had 100 metres to go, which meant that given the current production speed, bedrock was not on the horizon of the 2010 field campaign. In the afternoon, the drillers mounted new cutters on the drill. Just before bedtime, I caught up with the vice-PI, who walked around restlessly with a toothbrush in his hand. He was on his way to check the latest results from the drilling trench on the online monitor at the ground floor of the Main Dome. When we arrived, a group of 10–12 scientists had gathered watching the online monitor in suspense and anticipation. When the result showed 3.40 metres, the group rejoiced and celebrated. This was a clear indication that the drillers were now back on track and racing towards bedrock with the same Godspeed as before the "whiteout", re-establishing the camp's confidence in success.

The next day, on 15 July, the storm had gained in strength. When I looked out from my tent, I did not have visual contact with the Main Dome, about 250 metres away. Later, the visibility improved somewhat and I was able to make breakfast in the Main Dome. The monitor showed the following message: "Weather forecast same—Please do not leave your tents if you cannot see a next way point. You will be picked up! Have a Terrific Thursday!". Animated by the weather, stories were told over breakfast about "whiteouts", which lasted several weeks. In such conditions, rope had to connect the individual tents in the camp. Stories abounded about scientists elsewhere who had gone mad during prolonged "whiteouts" and driven away on "skidoos" (snow scooters) without GPS into the wild. With tracks disappearing in seconds, their intent was suicidal. However, the good news was that the new cutters on the drill did the trick and the production rate in the drilling trench was not hindered by the bad weather. At breakfast, it began to dawn on everybody that we were approaching bedrock and had to prepare for "landing" of the project.

PREDICTING RISK

The next day, 16 July, the storm was still hurling outside. At breakfast we made conversation about the piles of snow accumulating in our tents and what to do when one needs to relieve oneself when a storm is ranging outside the tent. Losing email contact with the outside world further added to the feeling of suspense and splendid isolation in camp. When the PI joined our table, still wearing full Arctic attire fresh from the drilling trench, the quaint stories were replaced with silence. After a while, the cook broke the silence and asked her about the latest from the drilling trench: "We drilled a core on 3.40 metres this morning", she said, and continued, "which means that with eight runs a day, we penetrate more than 20 metres a day. I have calculated that with this speed, we will reach bedrock on Tuesday (20 July). I have been thinking hard since 6 o'clock this morning and I've reached the decision that we shall stop the drilling this weekend to do a seismic logging of the borehole". I asked her why she wanted to delay drilling, when we were not sure we would reach bedrock this season. "You know", she said, "there is a split second between success and catastrophe. I do not want to sell the skin before the bear has been shot. Anything can still happen. Now, the ice in the borehole is almost at the melting point. The drill can get stuck any second". Then she added that with the current drilling speed, there was a lot of ice to log. This was one of the tasks of the anthropologist and I found my co-logger from Sweden and we hurried down in the drilling trench.

At lunch the same day, the PI formally announced her decision to stop drilling for the entire camp population: "As some of you know, the drillers have deployed asymmetrical shoes and steep cutters, which has given us a remarkable speed. This is the first time an ice core has been drilled with that speed so close to the bottom. So, I would say that from all points of view, the NEEM project is really, really running well. It is running so well, that we now initiate the landing procedure of the project. This means that we need to know how far we are from the bed. So, we need to log the hole to be able to see what is the pressure in the borehole and the pressure of the ice, so we can adjust the liquid level in the hole, so we don't get 40 metres of water into the borehole. So, we'll log the hole tonight and the plan is to stop the drilling and use Jakob's seismic logger and the Danish deep logger to check all these things and get ready for our landing". After this speech, people seemed concerned and relieved at the same time. In the afternoon, as the drilling rig stayed on the top of the surface and the sonar was lowered into the abyss, the online monitor reported no progress in the drilling trench. An eerie feeling of non-production (equalling non-progress) spread in camp. Some of my interlocutors tried to make sense of the PI's decision to deploy the sonar and said it was "her way of improvising", because it was not in the science-plan and the seismic logger was a prototype. Others said she was "so damn sure we would reach bedrock that she had time to do Jakob a favour and field test his prototype". By the look of the PI's face that same morning, when she conveyed to us that there was a split second between success and failure, there could be no doubt that her decision to stop drilling and deploy the Swiss precision instrument was a mode of predicting risk in order to enable human agency in that split second, which seemed the only responsible action.

The decision of the PI was also a textbook case of when and why Western naturalism makes recourse to experimental modelling. In her decision, the PI followed the rule prescribing that when direct measurements of nature are impossible or impractical, then one defer to experimental modelling. Lowering an acoustic sonar into the borehole to produce data about the remaining depth to and the physical conditions of bedrock, which had to be subsequently modelled, was a mode of obtaining knowledge about nature which was held to be less reliable and accurate than direct observations or measurements. Generally, we may say that models in science are most often used when it is either impossible or impractical to observe or create experimental conditions in which scientists can directly measure their object of study. In the camp, the deployment of the sonar and the subsequent modelling of its recordings were considered a poor substitute for direct measurements made impossible by physical inaccessibility.

Ultimately, the sonar was deployed to predict the uncertainty the NEEM community faced on the ice cap. The PI needed to know the unknown abyss in order to plan and prepare for these contingent uncertainties, which potentially threatened the landing of the entire project. In other words, the recordings of the sonar and their subsequent modelling made uncertainty manageable. This way to govern uncertainties by way of anticipating what nature/bedrock would be and look like, and prepare for how the camp should "initiate the landing procedure", seemed to reduce stress in camp. The PI's decision transformed the time between the lowering of the sonar (i.e. the now) and the hitting of bedrock (i.e. the immediate future) into a promising interregnum. Instead of suffering the pains of uncertainty and anxiety, the camp population now anticipated nature with excitement and confidence.

ECHOES FROM THE ABYSS

Friday night, I climbed down the icy stairs to the drilling trench to observe the logistics involved in lowering an acoustic sonar/seismic logger engineered in Switzerland (referred to as the "pinger" in camp) into the borehole. At the time this was done, the logger's depth indicated 2,495 metres.

The sonar consists of two parts: the "pinger" and the hydrophone. The first is a battery-driven electromagnetic hammer producing a sound signal of about 10 milliseconds' duration at a frequency of 5–10 kHz every 2nd second. The latter part consists of a ceramic disk, producing an electrical signal by pressure changes in the liquid, i.e. when sound hits the disk. The pinger hangs on steel ropes 50 centimetres below the hydrophone. The signal from the pinger travels down in the borehole and is partly reflected at the end of the borehole. The reflected signal travels up again and is partly reflected by the pinger and then travels towards the bottom of the hole again. So, the signal travels back and forth a few times until it dissolves by consecutive attenuation. The part of the signal that is not reflected at the end of the borehole continues its travel to the next obstacle, conceivably the ice bottom/

bedrock interface. Here it is again partly reflected and heads back towards the hydrophone (please see Figure 9.1). The Swiss constructor of the pinger explained to me that in addition to this rather complex amplitude of signals, there were "some internal spurious signals," e.g. from the hammer hitting its resting position in the pinger. In order to calculate and associate these various signals to their origin, one needs to know the speed of the sound in the liquid in the borehole and in the ice of the borehole. The first was known to the Swiss constructor through a separate experiment that he had conducted beforehand; the latter can be found in the scientific literature.

The echo from the bedrock can be detected when placing the sonar at such depth that the echoes from the bottom of the borehole do not interfere with the signal from the bedrock. Thus, the pinger emits an acoustic signal that is reflected by the borehole bottom/ice bottom/bedrock interface and recorded by the hydrophone. From the travel time of the signal in the drill liquid/ice sheet, the remaining thickness of the ice can be modelled.

By midnight, the pinger was at the bottom of the borehole emitting and recording acoustic signals and measuring response times. An hour later, the instrument came up from the abyss. One of the drillers explained to me that this was a kind of field test of a prototype, which could have enormous potential, not least for the oil industry. The PI, who had joined us in the drilling trench,

Figure 9.1 The Swiss constructor of the sonar device Dr Jakob Schwander in the sub-surface drilling trench ready to send the "pinger" down the borehole towards the abyss. Photographer: Dr Kenji Kawamura.

trusted a modeller from Germany with the task of interpreting the complex data set yielded by the Swiss instrument. As I accompanied the German modeller from the drilling trench to the Main Dome, I saw that the weather had cleared and the Sun was now shining from a clear blue sky. It was past 1 AM and most of the camp population were in their sleeping bags. While walking, the modeller explained to me that the pinger had not measured the distance to bedrock, but generated a set of "proxy data". He explained that proxy data were "quasi-measurements" and "inherently noisy", because variability was already in the data. I was reminded of the GCMs, which were also fed with proxy data stemming from pollen, tree rings, marine sediments, or ice cores. Data produced from these lines of evidence represents circumstantial approximations of our past climate (not direct measurements of past weather, such as temperature, wind speed, pressure, etc.). The signal amplitude of pollen can vary because of other factors than climate, just as the signal amplitude of the sonic signal from the pinger could vary because of other factors than those pertaining to the physical make-up of the borehole. In other words, the German modeller on the Greenland ice sheet seemed to be faced with a difficult interpretative task, similar to his colleagues in Western metropolises tinkering and tuning GCMs.

SOCIAL DISSENT IN THE MODELLING OF ICE

When we entered the Main Dome, the modeller was joined by an electrician from Denmark, who had operated the pinger in the drilling trench, a senior ice core scientist from Iceland, and a young Danish researcher working on the gas composition of ice cores. It seemed that the five of us, benched in the Main Dome, were the only ones awake in camp at 2 AM in the morning. The German modeller began to decode the signals on his *IBM Thinkpad* and run model simulations projecting different graphs, while the young researcher and the electrician watched the monitor closely. The senior scientist relaxed with an American chocolate chip cookie, tired after many hours of drilling in -18°C. Their common efforts at 2 AM in the morning were directed at estimating the remaining depth to bedrock, based on the interpretation and modelling of the signals on the *Thinkpad*. Or more precisely, they sought to filter the inherently "noisy data" from the "real signal". In this experimental set-up, noisy data were understood as stochastic signals and more generally as a residual category to the "real signal" from the borehole. The ensuing conversation is about how best to model the remaining depth in this hole accurately and realistically:

Modeller—This peak is the first one and now the question is which one is what. . .and I guess this one is the 40-metre and this one is the?

Scientist—70 metres.

Modeller—Yes, exactly.

Scientist—And this one is the. . .

Modeller—Ah, I didn't check, but that is the reflection area. . .that's multiple. . .ah, it looks like it's also multiple here. . .ah, so the 40 could be excluded. . .the 40-metre looks like it is a double of this one here. . .

Scientist—Yeah!

Modeller—OK, so the 70 would be the best guess then!?

Scientist—But what is that one?

Modeller—We don't know what that is, but it is always there. . .so, I would bet for 70. . .so, my best guess is 70. . .yes, because the peak before the 40 is the double of the speed, which is here.

Engineer—What about the 55-metre?

Modeller—OK, if we take the 50-metre example. . .then first peak, second peak. . .well that's the 40-metre and the 70 would be around here.

Scientist and Engineer—WAIT, wait, wait. . .what is that?!

Modeller—No idea, but we always had it.

Scientist—Yeah, but you can't just neglect it. . .it's way bigger than the noise!

Modeller—Yes, but I mean, it's between the. . .here, here's the whole signal then. . .I mean they can be anything. . .they cannot be the bottom, because they are closer at. . .

Engineer—OK, here is the ice bottom and here is the bedrock [pointing confidently to the two peaks of the graph on the computer monitor].

All—[Laughter].

Modeller—OK, then, we can do this and what do we get [performing calculations and running simulations]. . .we get a distance to the other of 20-metre, but we are at 50 metres high. . .ooops!

Modeller and Engineer—[Laughter].

Modeller—OK, if it is below 25 metres, then it is hidden in the other peak and we have no chance of seeing it. . .

Engineer—I would like you to model this for 55 metres.

Modeller—OK then. . .we can put the 55-metre in. . .maybe that is not a bad idea, to make some hypotheses and put the figures on and then we can see what stays consistent.

Engineer—Yeah. . .as in politics it is not bad to define your goal first. . .you need to know what you are looking for!

Scientist—You need to know what you want to know, first of all!

Modeller—You know, we can still leave it to the builder of this instrument to find out. . .we can just send him an email. . .maybe that is the best, then we don't have to. . .

What we see here is first and foremost that modelling is always mediated by technology and embedded in a social field (Hastrup, Chapter 1, this volume). It seems as if the modeller is set for 70 metres and operates with a pragmatic outlook to cut other estimates out; the engineer would prefer to see the 50-metre range come out, whereas the scientist has taken on the role as the cautious and sceptic auditor of the modelling methodology. What is shared is that they all seem to approach modelling with a seamless, but nevertheless reflective, continuum between what is defined as "politics" (goal) and "science" (knowledge). None of them subscribes to any naïve empirical positivism, at times attributed to scientists. They all know that models are *in simulacra*, i.e. simplified, tricky, and artefactual refractions of reality, rather than representations of reality. At the same time, they are looking for what they call "realistic signals", i.e. information which mirrors nature. In this sense, the entrenched dichotomy set up in much of the literature between models as heuristic work devices versus models as real representation of nature (Morgan & Morrison 1999; Petersen 2000) seems false in this case. The model deployed to calculate the remaining depth to bedrock at NEEM seemed to be both.

The modeller runs yet another simulation and the 70-metre estimate of remaining ice to bedrock is corroborated once again and the following conversation follows:[4]

Engineer—70 metres. . .argh! [He looks tired of the bare thought of ice which remains to be drilled]

Scientist—But can it [the pinger] even reach that far? Then you would have to look for a very small signal, right? [Turning towards the engineer]

Engineer—Hmmm [nodding].

Modeller—Yeah, the instrument is just built for 50 metres, so. . .

Engineer—Actually we should try and apply a Rita-G-algorithm on it,[5] because the signal would decay with distance, so you could apply a factor to the distance and add it up.

Modeller—Ahh, you mean. . .

Engineer—The linear signal would have a slope like this [drawing waves in the air with his hands]. . .you could give it a slope like this to have comparable signals. . .

Modeller—You mean to blow it up?

Engineer—Yeah. . .I don't know what the factor should be.

Modeller—You mean to blow it up in this direction [pointing in the air]. . .would it get better then?

Engineer—It would give you a more easy reading of the data.

Modeller—OK, I can make something linear. . .I am not sure if. . .

Engineer—Some factor times the distance. . .

Modeller—that is not a problem. . .so we can just make. . .hmm, what should we do. . .if we lower 1/10. . .[running simulations on his laptop].

Scientist—That's a good idea.

Modeller—OK, 200 . . . then . . .

Engineer—Actually, my guess is that the 55-metre peak would show up really nice and clean.

Engineer and Modeller—[Laughter].

Modeller—OK, first we make the 55-metre peak and colour it into all plots. . . we can do this.

All—[Laughter].

Modeller—OK, 55 metres below the other . . . so it is 10.1 peak . . . OK, it is not around zero, it has a drift there . . . I think here [point ing to the graph on the computer] you get to the binary noise . . . here you can see the resolution of the AD converter.[6]

Engineer—Oh shit!

Modeller—Here you have a drift. . . so I don't think it helps too much. . . I don't think we get more information.

Engineer—So, this is the direct signal? [pointing to the graph]

Modeller—OK, now we play . . . first . . . second one . . . still the same . . . we have these three choices. . . this is the multiple A-piece line . . . it is 40-metre, no 70-metre . . . so this was the one in the really low resolution . . . ok, again blown up . . . so, it's 70 here, if I go here it is 70 again . . .

Engineer—I don't like the 70. . . you're a really bad modeller!

Engineer and Modeller—[Laughter].

What does it mean to be a good modeller? Here on the ice sheet it apparently means to estimate the amount of labour left (remaining ice to be drilled) in optimistic numbers. This finding would divert slightly from what Sundberg found in her study of modellers: "My point is that the different way experimentalists and modellers work with and understand simulation models and data shapes what it means to be a 'good' simulation model or 'good' data. The contents of these qualities depend on whether you measure or simulate" (Sundberg 2006). However, what is striking about this modelling experiment is that our three friends do not feed the model with various data sets and then run their simulations. Rather, they seem to be testing the model, i.e. they run different hypotheses to look for overall consistency. Through their conversation, we gain appreciation that modelling is also a technique of visualization,[7] involving both the interpretation of plots and rescaling of graphs (the application of the "Rita-G-algorithm"). Much of the conversation pertained to how to tune the model better for the task at hand, through scale jumps and recalibrations, which would alter resolutions. When resolution became too high, distortion or "noise" e.g. in the form of the analogue/digital conversion, became visible. It seemed that different scales and calculating techniques were deployed essentially as a way to filter a "noisy signal" and make interpretations more convincing. Most generally, the modelling efforts turned on improving or amplifying the "real signals" and reducing the "noise" (e.g. in the form of the stochastic signals, but generally what the scientists understood as residual categories of the "real signal"). In this sense, the model played the role of an epistemic object, i.e. a question-generating tool of investigation (Morgan & Morrison 1999).

What conclusions did the community ultimately draw from this modelling attempt? The wording of the official field diary on 16 July, when the sonar was lowered into the hole, was the following: “By midnight the logger was at the bottom of the borehole, but there were no confirmative conclusions about the ice thickness”.[8] The next day, the modeller emailed the data set yielded by the sonar to the Swiss constructor in Bern, who estimated that bedrock was either 70 metres or 110 metres away from the current depth of the borehole. This resulted in the following official entry the next day: “The result of the seismic logging last evening was not definitive, but it suggests that bedrock may be some 20 m deeper than the radar prediction of 2545m”,[9] which implicated an estimate of 85 metres.[10] Given this uncertainty and range of the estimates, that very evening, the cook was sitting in the Main Dome after having prepared dinner, making fun of science: “If the world’s best scientists cannot figure out if there is 30, 50, or 70 metres to bedrock, what can they possibly know about the climate. Why don’t they just piss in the borehole and see what happens!”. Although the cook was not the most popular person in camp, especially among the vegetarians, his vulgar language somehow echoed the general mood of the camp, of a failed predictive modelling attempt and the waste of precious drilling time.

A few days later, the Swiss constructor of the sonar was flown into camp. He reinterpreted the data and came up with 65 metres to bedrock. However, this figure never made it to the official field record. During his stay in the camp he performed another sonar logging, and on this basis told me that he recorded no signal from the ice bottom/bedrock interface. He accounted for this in the following way: “You cannot hear your echo when standing 2 metres in front of a wall!”, implying that we were now so close to bedrock that any seismic logging attempt would be futile. Jokes were usually cracked about most things in camp. In the men’s sauna on 24 July, one of the jokes pertained to the sonar logging. The rumour had it that the latest prediction was 1.6 metres, but that the earlier one had indicated 9 metres. A person in the sauna said: “We should put it on *Ebay* and democratize it! The average guess would come closer to the real depth, than any single expert’s individual assessment”. Generally, it seemed as if the mood of the camp was that this predictive modelling attempt had largely failed.

ENCOUNTERING EVIDENCE

That same weekend as the predictive modelling exercise was turned into a joke in the camp sauna, the drillers began to encounter physical evidence of bedrock in the form of a 5-centimetres-long stone, embedded in so-called “silty layers” of ice consisting of black and brown particles in the core. This meant that drilling progressed very slowly. The PI decided to deploy a grinder drill, which is capable of penetrating everything, causing considerable collateral damage, i.e. destroying the ice as scientific evidence (which is why this drill was named "Arnold" after a governor in California). It was obvious that this was not an easy decision for the PI. After serious efforts in the drilling trench, a 10-centimetres crystal-clear ice core came up from the abyss. This was enigmatic to the drillers. They continued their efforts, but could not penetrate any further. The next day, 27 July, bedrock was reached, i.e. defined at 2,537.36 metres. A celebration was held in the drilling trench and some two hours later the entire camp population rode 2.5 kilometres on skidoos away from camp to the shallow core drill site, where the celebration continued (a different drilling to obtain samples of firn air in the upper sections of the ice sheet). The point of having it here was that the distance from this shallow core site to the Main Dome mirrored the distance from the surface to bedrock, making the traversed distance very tangible. The weather was sunny and everyone was relieved. In the evening, pre-dinner drinks were served in the *Chill Out Lounge* or the *Ice Cave*, followed by a celebratory evening dinner served in the Main Dome. The NEEM project was officially terminated and a press release conveyed the overwhelming success. The next day, 28 July, 23 people left NEEM in a U.S. Hercules, which equalled two-thirds of the entire camp population. Thus, I would argue that the reaching of bedrock on 27 July at 2,537.36 metres instantiated the epitome of what Edwin Ardener has called "the collapse of measurement into definition" (Ardener 2006). In other words, predictive modelling was overridden by the collective definition of nature, which seemed part of a much larger plot.

CONSTRAINT OR CONTINGENCE?

So, did bedrock just happen or was it defined as a product of constraint (time, resources, skills, technical instrumentation, etc.)? In crafting this ethnographic account, I have paid heed to Peter Galison and Michel Pickering's conspicuous controversy over what constitutes *constraint* and *contingence* in science. Without going into the details of this dispute, both Galison and Pickering sought to address the question of how we characterize properly the complexity and difficulty of real-time scientific practice. My ethnography of scientific practice in the NEEM camp has attempted to account for the real-time scientific practice in the anticipation of nature. To recap: in the first two weeks of July 2010, we followed qualified guesswork based on social exit dates, rather than temperature and other cool numerical facts (the Grand NEEM Bedrock Bet); in mid- and late July predictive modelling was launched, based on remote sensing in the borehole producing numerical data corroborated, calibrated, scaled, and simulated on an IBM Thinkpad computer; and towards the end of July, these uncertain predictions were overruled by continued drill production, the encountering of evidence of bedrock, and ultimately the collective definition of bedrock, when "Arnold" (the grinder drill head) could not penetrate deeper into the abyss. To assert that this scientific practice is patchy, contingent, interrupted, and heterogeneous, is to me the least interesting aspect of it and a rather trivial point to make. To attend to "the constraints on experimentalist' conclusions [sic] that are imposed by the skills and techniques of their work" (Galison 1987) was the sort of "constraint talk" Pickering objected to: "To identify cultural elements as constraints is precisely to lose sight of the openness of their future extension . . . Whatever obstacles do arise in practice are not 'already there' to begin with; instead they genuinely emerge in time" (Pickering 1995). Pickering's view of contingency in scientific practice coincides with his conception of modelling: "a fundamental aspect of modelling, namely, that it is an open-ended process having no determinate destination" (ibid.). I would argue that we might qualify the key question of whether bedrock just happened or it was imposed by the constraints of time, skills, instruments, resources, etc. in camp, by way of attending to Hastrup's account of the process of modelling (Hastrup, Chapter 1, this volume).

Hastrup identifies five general components of modelling: observation, formalization, experimentation, projection, and action. These sit nicely with the modelling sequence we have seen unfold on the ice sheet: we have the *observation* in the form of the lowering of the pinger as a form of object agency to produce data, where observation is literally beyond reach; the scientists then proceeded to *experimentation*, which revolved around the scaling of the data on the IBM Thinkpad, which evidently took place in the realm of the social; the outcome of experimentation is *projection* here given in numerical form as 40, 70, or 110 metres to ice bottom/bedrock. With Ardener, we see that when prediction is truly important, it fails (Ardener 2006), because this form of predictive modelling experiment involved singularity (it was a field test of a prototype) and stochastic variabilities (the multiple reflections of the signal from the abyss). Thus, the projection did not rest on repetition and was therefore doomed to fail, as Ardener has shown. Finally, the outcome of the projection was translated into *action,* namely continued drilling. Now, action "is also a function of one's understanding of the plot in which one takes part, including its temporal and spatial extension" (Hastrup 2004). Building on Hastrup's theory of agency, I would call the *action* taken on the ice sheet in the wake of *projection* a form of *emplotment*. By this I mean a form of bricolage or extremely skilful manipulation to make do of whatever emerges in a sequence of events to align the social conventions of success with the collective definition of nature.

The concept of emplotment was developed by Paul Ricoeur in his important work *Time and Narrative* (2004). Ricoeur understands emplotment as a configuration of time drawing together events, agents, and objects into a situation of imaginative order with internal consistency (Ricoeur 2004: 150). This is accomplished by endowing the connections between the different elements with necessity and explanatory status. Thus, emplotment renders objects, agents, and occurrences into a meaningful whole that takes place in a network that constitutes the narrative's response to why, how, who, where, when, etc. Emplotment establishes intelligibility and credibility, where uncertainty and unintelligibility reigned. Thus, I would argue that reaching bedrock on 27 July at 2,537.36 metres did not just happen, neither was it a product of constraints on part of the human, technological, resources, time, etc. The NEEM community reached bedrock on 27 July because of the PI's extremely skilful ability to align the social exit dates from camp (i.e. being in camp when bedrock was reached) with the collective definition of nature (i.e. the success criterion of the science). Thus, in my attempt to account for "real-time scientific practice" I would argue that bedrock was reached on 27 July, not because of constraint (apparatus, human resources, capital), nor because of contingence (it just happened), but because the PI was endowed with an eminent sense of emplotment.

MODES OF ANTICIPATING NATURE

What are the implications of this argument for modes of anticipating nature? Well, to begin with I would draw attention to the distinction climate modellers themselves uphold, namely that between *prediction* and *projection*. "Prediction is for the weather what projection is for the climate",[11] as one modeller in Copenhagen coined it. Thus, to follow the concepts of the actors themselves, *prediction* is an estimate of an exact event (rain on Wednesday morning) based on simulation modelling, whereas *projection* is an estimate of future general states based on assumptions about how the world will evolve (CO_2 emissions, land use, deforestation, etc.). In a radio interview Willi Dansgaard illustrated this difference between weather prediction and climate projection,[12] stating that *prediction* is an estimate of where the ball is on the court after 70 minutes of play (which is considered impossible), whereas *projection* means the numerical outcome of the game, which can be calculated with some probability (see also Dansgaard 2004) . I would be inclined to argue that this line of reasoning about the two modes of anticipation seems to permeate much of climate science.

In the NEEM camp, we seem to have different anticipatory modes of forecasting nature. The Grand NEEM Bedrock Bet represents a form of informed judgement, where the social field of the camp seems to take preeminence over the cool facts of nature in the bet entries made by the scientists. Then we have the attempts of modelling the remaining distance to the ice bottom/bedrock. The predicament of this mode of prediction, as Ardener knew, is that prediction can never succeed if it is not based on repetition. However, the prototype of the pinger produced stochastic echoes from the abyss, which were essentially non-repetitious; in fact they seemed chaotic. As we saw, this mode of prediction failed and most significantly, was overridden by social action. I have conceived this action as a form of emplotment, which generally resonates with the phenomenology of Alfred

Schutz: "In every action we know the goal in advance in the form of an anticipation that is 'empty', in the sense of vague . . . and [we] seek by our action to bring it step by step to concrete realization" (Schutz 1997). The PI in the camp was acutely aware of the ultimate goal of bedrock in every step she took, from her arrival in camp on 27 June until the collective definition of bedrock precisely a month later. Like the process of modelling itself identified earlier, these three modes of anticipating nature at NEEM (informed judgement, the predicament of prediction and emplotment, where the latter overrides the two formers) seem to be intimately linked. As Hastrup concludes: "They constitute an iterative process of arguing about nature and social action that is never devoid of interest; politics and policy are at work throughout" (Hastrup, Chapter 1, this volume).

ZOOMING OUT, SCALING UP

Allow me to return to the question raised at the outset: should we defer human decision-making about the future environment to the authority of climate modelling? In our time of climate change and tipping points, James Lovelock (2009) has called for an environmental epistocracy (or more precisely a *Gaian epistocracy*), where political authority is vested firmly in science and democracy is temporarily suspended. If we had license to zoom out and scale up from a small cosmos in the form of a scientific community facing risk on the ice sheet of Greenland to Gaia at large, the answer to Lovelock and other proponents of the rule of science would be that the uncertainties of climate modelling by and of itself makes scientists (albeit in an off-guard mode in the men's sauna) call for democracy, rather than epistocracy. However, as we saw in the NEEM camp, the present will likely catch up with and override the futures imagined by predictive climate modelling. Allow me an allegory: if the social life of modelling at NEEM sitting on top of the planet is indicative for the worlds in which decision-makers take their toughest and most difficult political decisions about our environment, then these worlds are not made up of model algorithms, but of the dates these decision-makers enter and leave their positions.

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NOTES

1. Existing world record was 1,751.51 metres set at NGRIP in 1999.
2. These data were recorded in an earlier field season, to identify the best site for obtaining "undisturbed" Eemian ice, i.e. unfolded ice.
3. The basalt bottom beneath the ice sheet is warm, causing the ice to melt from below.
4. A simulation brings a model to life and shows how a particular object or phenomenon will behave. A steady-state simulation provides information about the system at an instant in time (usually at equilibrium, if it exists). A dynamic simulation provides information over time.
5. Rita Granberry is a curvy American model of African and Italian descent, born in New York, 1979; a "Rita-G-algorithm" is camp slang for smoothening "noisy data" into aesthetically pleasing curves.
6. Meaning the analogue/digital converter, which produces distortion.
7. This is probably why climate modelling lends itself so well and translates so seamlessly to the film medium, as we have seen in *The Age of Stupid* (2009), *The Day after Tomorrow* (2004), and *An Inconvenient Truth* (2006).
8. http://neem.dk/field_diaries_folder/uk_diaries_2010/2010-07-16/ (accessed 17 June 2011).
9. http://neem.dk/field_diaries_folder/uk_diaries_2010/2010-07-17/ (accessed 17 June 2011).
10. The "driller's depth" when the sonar logging was performed was 2,480 metres. The logger's depth was 2,495 metres.
11. Oral communication, NBI, March 2011 (Tim Brucher).
12. Willi Dansgaard (1923–2011) is regarded as the founder of modern ice core research.

REFERENCES

Ardener, E. 2006 [1989]. The Voice of Prophecy and Other Essays. Oxford: Blackwell.

Dansgaard, W. 2004. Frozen Annals: Greenland Ice Sheet Research. Odder, Denmark: Narayana Press.

Edwards, P.N. 1999. "Global Climate Science, Uncertainty and Politics: Data-Laden Models, Model-Filtered Data." Science as Culture 8 (4), 437–72.

Fine, G.A. 2007. Authors of the Storm: Meteorologists and the Culture of Prediction. Chicago: University of Chicago Press.
Galison, P. 1987. How Experiments End. Chicago: University of Chicago Press.
Hastrup, K. 2004. Action: Anthropology in the Company of Shakespeare. Copenhagen: Museum Tusculanum.
Hulme,M. and Dessai,S. (2008) "Negotiating future climates: a critical review of the development of climate scenarios for the UK." Environmental Science and Policy 11(1), 54-70.
Hulme, M. 2010. "Problems with Making and Governing Global Kinds of Knowledge." Global Environmental Change 20 (4), 558–64.
Jasanoff, S., ed. 2004. States of Knowledge: The Co-production of Science and Social Order. London: Routledge.
Lahsen, M. 2005. "Seductive Simulations? Uncertainty Distribution around Climate Models." Social Studies of Science 35 (6), 895–922.
Lovelock, J. 2009. "Through Gaian eyes". Ideas: in the name of science. Cayley, D. ed. Pp. 109-128. Fredericton, New Brunswick: Goose Lane.
Morgan, M.S. & M. Morrison, eds. 1999. Models as Mediators: Perspectives on Natural and Social Science. Cambridge: Cambridge University Press.
Oreskes, N., K. Shrader-Freshette & K. Belitz. 1994. "Verification, Validation, and Confirmation of Numerical Models in the Earth Sciences." Science 263, 641–46.
Petersen, A.C. 2000. "REVIEW: Models as Technological Artefacts." Social Studies of Science 30 (5), 793–99.
Pickering, A. 1995. "Beyond Constraint: The Temporality of Practice and the Historicity of Knowledge", in Buchwald, ed. Scientific Practice: Theories and Stories of Doing Physics. Chicago: University of Chicago Press.
Ricoeur, P. 2004. Memory, History, Forgetting. Chicago: University of Chicago Press.
Schutz, A. 1997. The Phenomenology of the Social World. Illinois: Northwestern University Press.
Shackley, S., P. Young, S. Parkinson & B. Wynne. 1998. "Uncertainty, Complexity and Concepts of Good Science in Climate Change Modelling: Are GCMs the Best Tools?" Climatic Change 38, 159–205.
Sundberg, M. 2006. "Credulous Modellers and Suspicious Experimentalists? Comparison of Model Output and Data in Meteorological Simulation Modelling." Science Studies 19 (1), 52–68.
Sundberg, M. 2007. "Parameterizations as Boundary Objects on the Climate Arena." Social Studies of Science 37 (3), 473–88.
Tsing, A.L. 2005. Friction: An Ethnography of Global Connection. Princeton, NJ: Princeton University Press.
Winsberg, E. 1999. "Sanctioning Models: The Epistemology of Models." Science in Context 12 (2), 275–92.
Wynne, B. 2010. "Strange Weather, Again: Climate Science as Political Act." Theory Culture Society 27 (2–3), 289–305.

10 Predictability in Question

On Climate Modelling in Physics

Peter D. Ditlevsen

The Climate is what we expect, weather is what we get.

Mark Twain

"Climate" has the same etymological root as "inclination", referring to the inclination of the Sun as the primary determining factor for the temperature. This dependence of climate on latitude was already well known by the ancient Greeks. Climate variations beyond the season have probably been noted even before then, when agriculture made humans strongly dependent on weather conditions. In more modern times the understanding of climate variations was part of the great achievements in natural history in the middle of the 19th century. Three discoveries changed the general view of a steady world. One was the discovery by Richard Owen (1841) and others of the fossilized remains of the no-longer-existing giant dinosaurs; the second and most important was Charles Darwin's expedition on the Beagle in 1839 leading to the discovery of evolution of the species (Darwin 1859). The third was Louis Agassiz' discovery of the ice ages (Agassiz 1840).

Often the great discoveries lie right below our noses if we are capable of asking the right questions. Agassiz attributed the occurrence of huge boulders far away from their possible bedrock source to the action of past glaciers, by analogy to the action of present Alpine glaciers, rather than the work of the Biblical flood. As was the case for Darwin's theories it took more than a quarter of a century for the scientific establishment to accept the existence of ice ages. Understanding the cause for ice ages was for more than a century considered the major challenge in climate theory. Today, we still do not fully understand the mechanisms governing the ice ages, but focus in climate research has changed.

The year 1957 was appointed International Geophysical Year, where a broad range of fundamental research on the physical properties in the Earth was initiated. Among these was monitoring of atmospheric CO_2 concentration at the Mauna Loa observatory in Hawaii (Keeling et al., 1976), a location remote from any major emission source. The measurements have shown a steady increase in atmospheric CO_2 originating from biomass— and fossil fuel burning. The urge for deeper understanding and modelling of the climate and increased scientific resources has come with the public awareness that anthropogenic greenhouse gas emissions cause climate changes and possibly inflict natural hazards with societal implications. There is thus a moral dimension in the assessments of climate change and mitigation measures. Climate science has in the last few decades therefore received large attention. The Intergovernmental Panel on Climate Change (IPCC), which is a UN body, has not only had a strong impact on policy-making—it received the Nobel Peace Prize in 2007—it has also been influential in the way scientists conduct climate research (Hulme and Dessai, 2008). In fact other natural hazards like earthquakes, tsunamis, and volcanoes have at present caused much higher immediate casualties than climate changes, but they are free of the moral burden, and are highly unpredictable. On the other hand, with the global economy relying heavily on fossil fuel burning, strong economic and political interests have given a few scientists, "climate deniers", refuting the existence of anthropogenic greenhouse warming, disproportionate attention.

In this chapter, I shall refrain from the heated semi-scientific debate on anthropogenic climate change, which seems to be slowly settling. For some time the debate was quite polarized; the term "climate denier" has negative connotations alluding to religious deniers of natural selection, which are not to be taken seriously in the scientific community. On the other hand, using the term "climate sceptics" instead would imply that those scientists, who are concerned about global warming, do not conduct the sound practice of scepticism towards their own and others' scientific results.

The potential impacts of climate change on societies call for not only action but also predictions and future projections. In this sense the climate sciences anticipate Nature as a calculable entity, where decision-making can be based on rationality and reliable model predictions. This is partly due to the successes of weather prediction models. When these numerical models became practical with the development of the computer after the Second World War, it was not known how far into the future a prediction was possible. Today we know that weather predictions are fundamentally limited, but we anticipate that climate predictions can be made much further into the future than the range of a weather forecast.

THE GREENHOUSE EFFECT

The greenhouse effect is very well understood based on basic laws of physics. French mathematician and physicist Joseph Fourier asked the simple question: when the Sun constantly heats the Earth by short wave radiation, why does its temperature not increase steadily? Fourier realized, in a paper published in 1824, that the Earth itself radiates heat back into space. By experiments he could estimate this long wave radiation and found that the Earth's surface was warmer than it should be in order to balance the incoming solar radiation. He then argued that the atmosphere acts as a greenhouse keeping the surface warm. He was right about the warming, but not that the air acts as the glass in a greenhouse; however, the term has stuck ever since. It was not until 1896 that the physical explanation for the greenhouse effect was given by the Swedish physicist and chemist Svante Arrhenius (1896). He measured the absorption of the long wave radiation by the CO_2 in the atmosphere, observing the long wave radiation from the Moon. Even though his measurements were not accurate by today's standards, his calculations showed that a doubling of the atmospheric CO_2 concentration would lead to an increase in surface temperature of 4°C. This result stands more or less unchanged after more than a century.

The atmosphere is transparent for visible light (short wave radiation), which is the dominant radiation from the Sun. The transparency is apparent, because we can see very faint stars through the atmosphere at night. The part of the solar radiation not reflected back to space is heating the land and ocean surfaces. This heat is re-emitted partly by evaporating water and partly as long wave radiation into the atmosphere. Water vapour and greenhouse gasses absorb the long wave radiation, thus the atmosphere is not transparent for the long wave radiation emitted by the Earth. The infrared radiation is invisible to us. This is, of course, not a coincidence. Our eyes are developed to sense radiation at exactly the wavelengths where the transparency is highest. Could we see the long wave radiation, we would be looking into a fog when looking into the atmosphere. According to the laws of thermal physics any body will radiate a heat depending in a specific way on its temperature. This goes for any part of the atmosphere, so the unhindered radiation into space will be depending on the temperature of the layers of the atmosphere close to the top, which can be seen from space in the long wave band. This is about three kilometres up in the atmosphere. The temperature at this height is then the temperature at which radiation will be emitted balancing the incoming radiation. This means that the balance between the incoming and outgoing heat determines the temperature three kilometres up in the atmosphere and not the temperature at ground. The difference between the two is the greenhouse effect, which on Earth is about 32°C. If more absorbing molecules are emitted into the atmosphere, the level from where the radiation is emitted to space rises, because, seen from space, the atmosphere becomes even less transparent in the long waves. The temperature, at this new higher level, then rises to balance the incoming radiation, and with an unchanged decrease of temperature with height the ground also heats more.

Understanding the mechanism for the greenhouse warming, and knowing the amount of CO_2 emitted into the atmosphere, it should be very easy to calculate the resulting greenhouse warming. However, the climate system will react to the changes. If the surface warms, more water will be evaporated into the atmosphere. Water vapour is also a greenhouse gas, which in turn will lead to even more heating. This is called a positive feedback, where the original heating is further enhanced. On the other hand, more water vapour in the air might lead to more clouds, which in turn cool the surface, by reflecting the sunlight. This is called a negative feedback, which will dampen the original heating. In order to make quantitative predictions on the greenhouse warming, all the important feedbacks must be calculated, so that not only the direct effects, but all the responses in the system, are taken into account. This is an extremely complex task, because even though we may have a qualitative understanding of the immediate response in one variable to changes in another, the interconnectedness and feedbacks makes it necessary to model everything simultaneously.

CHAOS AND PREDICTABILITY

Climate predictions are of a different kind than weather predictions (Lorenz 1975). In order to understand this difference we shall for a moment digress and consider the weather prediction. A weather prediction is the prediction done for the future based on the conditions observed today and in the past. This is called an initial value problem, the initial value being the state of the system observed at the initial time, now. The prediction could then be based on solving the equations for the fluid-mechanical evolution of the system. Had the system been the Moon and we observe a full moon today, we can predict a new moon in 14 days from now. For that we do not even have to solve the equations of motion, because we know that their general solution is a periodic cycle of 28 days. In case of the weather the situation is much more complicated. The equations of motion for the atmosphere cannot be solved in the same way as the equation of motion for the moon. The equations are such that the atmosphere is chaotic. There are several features in such a dynamical system signifying that it is chaotic. The most general feature is what is called "critical dependence on initial conditions". This means that an infinitesimally small difference in initial conditions in two situations will in time lead to completely different final states. This effect is coined the "butterfly effect" in meteorology, quoted as the fact that "the flap of the wing of a butterfly over Brazil could cause a tornado over Texas". This was the title of a talk given by Edward Lorenz in 1972 on the subject. The term originates from the 1952 short story "A Sound of Thunder" on time travel by Ray Bradbury.

The critical dependence on initial condition (Lorenz 1963) implies that we need to know the initial condition with certainty, which we cannot do, in order to make a future prediction. In a non-chaotic system uncertainty in the initial condition also leads to uncertainty in the predicted final state, but the uncertainty is limited in some proportion to the uncertainty in the initial condition. In the example above, if I did not observe a perfect full moon, my prediction would not be too wrong, because the new moon would be 13 or 15 days away and not my predicted 14 days. In the case of a chaotic system the error will grow exponentially in time until eventually the predicted state and the actual state differ as much as any two randomly chosen states of the system. At this time all information of the initial condition is lost.

To further appreciate the difference between a non-chaotic and a chaotic system let us consider two games. The first game is pool, in which a ball is hit to make a precise orbit across the table with the goal of hitting some other ball into a pocket. The second game is rolling a die with the goal of having a specific face pointing upward when the die comes to a rest. These two games are fundamentally different in the sense that the first game is won by the most skilful player, whereas the last game is won by the luckiest player (any random player). In the first case the rolling of the ball after the initial hit is predictable for a long time. It depends in a calculable way on the hit and position of the ball (the initial condition). In the second case the outcome of rolling the die also depends on exactly how the die left the throwing hand, how it hit the table, how it rolled, etc. Now, because a minute change in how the die hit the table would make it turn right over a corner rather than left, the rolling is highly unpredictable. Both games (dynamical systems) are deterministic and governed by well-known dynamical equations. The first is non-chaotic (or very weakly chaotic) whereas the second game is so strongly chaotic that we consider it random. Considering the outcome of the die rolling as random is a very good model of the process. One can argue that at the fundamental level there is no randomness, but the chaotic nature of the deterministic equations will be indistinguishable from pure mathematical randomness.

The fact that a system is chaotic does not, however, mean that predictions cannot be made within some limited time horizon. In the case of rolling the die, the turning of the faces could be calculated as a function of how fast the die rotates in the air all the way until it hits the table for the first time. The time scale of predictability is thus the time it takes the die from leaving the hand until it hits the table. Likewise, even though the weather is chaotic, skilful predictions can be made within some time scale of predictability. Numerical weather predictions, presented on any TV channel, are based on solving the dynamical equations of the atmosphere in a computer fed with the previously calculated state and new observations. These weather predictions are skilful for several days, but not several weeks. This is a prediction of the first kind.

Let us now return to the example of rolling dice. Even though each throw is completely unpredictable there is still a strong regularity in the statistics of dice throwing. So if our goal was not to predict the next outcome, but the average outcome of a series of throws we obtain a new type of predictability: the average will be close to 3.5, with certainty growing as the number of throws averaged over grows. The climate can be considered as the average state of the weather, so even though we cannot predict the weather beyond weeks, we might be able to predict the climate. This has of course been done at all times in the sense that, based on previous experience (observations), we expect the climate at a given location or a given time of year to be close to the average over the period of experience for that location or time of year.

If we had no previous observations, the climate state could in principle be obtained from running the weather prediction model long enough or many times with different (randomly chosen) initial conditions. This would be pretty similar to throwing the die many times in order to observe the statistics. (Though for the die we would not even have to do the experiment, because the symmetry of the die alone provides all the needed information.) A numerical climate model is intended to do exactly this kind of calculation. Predicting the statistics of a system is called prediction of the second kind. So even though a chaotic system (the weather) is fundamentally unpredictable in the sense of predictability of the first kind, it can be predictable in the sense of predictability of the second kind (the climate).

THE FORECAST MODELS AND THE CLIMATE MODELS

The numerical weather predictions have had a long birth. The physical equations governing the flow of the atmosphere were formulated already by Leonhard Euler (1757), Claude-Louis Navier (1822), and George Gabriel Stokes (1842). The main equation is the Navier-Stokes equation. The general solution of the equation remains today one of the big challenges in physics. The first attempt for a numerical solution was done—by hand—by physicist and pacifist Lewis Fry Richardson while working as an ambulance driver during World War I. The calculation, which was a six-hour forecast for 20 May 1910, was of course not a real forecast but a hind cast (Richardson 1922). It failed for technical reasons in how the observed atmospheric pressure was included, though his method and calculations were essentially correct. The amount of numerical calculations necessary for determining the evolution of the atmosphere from the equations is so huge that the computer is essential for doing the task, especially if it has to be done in time for the forecast to be useful. Parallel to the efforts of calculating the weather, a fundamentally different approach for anticipating the weather changes, especially in terms of the passing cyclones over Europe and North America, was taken by the Norwegian physicist and meteorologist Vilhelm Bjerknes (Friedman 1989). The weather should be understood in terms of physical laws, so even though Bjerknes could not solve the equations for the atmospheric flow, he developed a general theory, "physical hydrodynamics", by identifying the physical cause for generation of winds in the atmosphere (Bjerknes & Solberg 1922). The engine for that is the meeting between the warm tropical air and the cold polar air. The warm air is lighter than the cold air and will thus rise above the cold air. In that process the rising air will cool and the moisture will condensate to rain. The places where cold and warm air masses meet were named fronts. Fronts passing indicate changes in the weather. The Bergen school, headed by Bjerknes, developed a whole new paradigm of meteorology, sometimes named "frontology", by which the coming weather was anticipated through weather maps, with wavy patterns of cold and warm fronts, disseminated to the public in newspapers and TV.

During World War II computer technology advanced, mainly from the demand of code cracking. In the 1950s, mathematician John von Neumann, co-constructer of the ENIAC computer, and meteorologist Jule Charney engaged in numerical weather predictions. It took another 25 years before the numerical weather predictions in the mid-1970s outperformed the more empirical front-system predictions based on hand drawing of weather maps.

The validation of the forecast models, performing predictions of the first kind, in order to verify that relevant physical processes and so on are adequately represented, can be done by observing the skill by simply comparing the predictions with observations. For the climate models, performing predictions of the second kind, the situation is different: in this situation we can only compare with observations going back in time. This means that we examine if the mean state (the climate) predicted by the model compares well with the observed climate. For predictions of a future changing climate we have to make the crucial assumption that the reason that the climate model performs well in simulating the past and present climate is that the model adequately represents the physical laws and equations governing the climate, and thus it will also be adequate in simulating the future climate where conditions have changed.

PHYSICAL PARAMETERIZATION AND MODEL RESOLUTION

In order to appreciate the working of the climate models, we shall briefly consider the rationale behind solving the equations of motion for the atmosphere. The physical nature surrounding us is described by sensible or measureable quantities; the atmosphere is characterized by its temperature, wind, pressure, density, and humidity. These variables can be ascribed measurable values at each point in space and time. Variables with values depending continuously on space and time are named fields. The value of a field, say temperature, at a specific location, say in Copenhagen, changes in time, say from noon to 1 PM. The temperature may rise, because the temperature is higher west of Copenhagen and the wind is blowing from the west bringing in warmer air. The temperature could also be rising because the Sun is heating the air or because the moisture condensates out and falls as rain. Thus the rate-of-change with time in temperature depends on the wind, the rate-of-change with space in the direction where the wind comes from, the rate-of-heating, and the condensation of water. The changes are accounted for in the equations of motion for the flow (Holton 2004). As mentioned before, these equations are complicated and cannot be solved exactly. The way we can solve them approximately is by substituting the rate-of-change of, say, temperature, by finite differences between the temperature at one point in time and at some time before that, say at 12:15 and 12. This difference in temperature will, among many other things, depend on the rate-of-change of temperature with distance. This rate-of-change is substituted by finite differences as well, say the difference between temperature in Copenhagen and Hamburg. We thus advance the evolution of temperature by expressing the temperature in Copenhagen at 12:15 as a complicated function of temperatures, winds, and so on in Copenhagen, Hamburg, and a few other locations at 12. The locations where we define the variables are spread over the globe in a regular grid.

The general circulation models solve the equations of motion for this set of variables in the mesh of grid points distributed over the globe. Locations between the grid points are not represented, so the values of the variables, or fields, are taken as some interpolation between the values in the grid points. The grid points are in present day's climate models typically hundreds or thousands of kilometres apart. Inside the model whatever goes on inside a grid box (cornered by the grid points) must be represented by the few values of the parameters in the grid points. When the results of a model, a huge amount of numbers, is to be interpreted and presented it is done graphically typically in terms of maps of the fields, interpolated smoothly from the grid points to cover the globe overlaid a map of land contours. Now because most countries are smaller than the square grid boxes, the land contours are plotted in a much finer resolution than actually represented in the model. The graphical impression might then lead to anticipation of much more realism in the model output than what is actually substantiated.

One very important physical process in the climate is the formation and evolution of clouds. They strongly alter the radiation; as we all know, it is immediately felt when a cloud blocks the Sun, and, on the contrary, a cloudy winter night is warmer than a clear sky. The clouds also have a greenhouse effect. Cumulus clouds are typically of sizes less than square kilometres and thus much smaller than the grid boxes (which are perhaps 10^5 square kilometres). A cumulus cloud is formed by condensation of moisture in an ascending air mass. An air mass rises if it is lighter than the surrounding air, and the moisture begins to condensate into cloud droplets in a complicated way depending on micro-physical conditions of temperature and aerosols, acting as nucleation seeds. All of these processes within a grid box, in the model, can only depend on the few variables at the grid points contained within the model. The unresolved processes are then represented by some empirically, statistically, or reasoning-based functional relationship with the resolved variables of the model. This is called physical parameterization. The purpose of the physical parameterization is to determine, the other way around, the influence of the unresolved physical processes on the variables of the model. The physical parameterization is one of the Achilles' heels of the climate models. The validity of the functional relationship of a given physical parameterization can only be established by examining how well the climate predicted by the model compares with the observed climate. This is part of the tuning of the models. When then running the models with changed conditions for future projections, we thus have to assume that the parameterizations are still valid under the changed conditions.

As the grid of model resolution is refined, the more variables are included in the computation and the smaller are the computational time steps required (more steps to obtain the same progression). This implies that the higher the resolution of the model is, the more computer power is demanded. This is the case in any numerical solution of a physical problem. One will then ask, what is the sufficient resolution required to solve the problem? In order to answer this question in a meaningful way, we have to specify to what degree of accuracy we want the answer. We thus have to be able to identify an error margin, such that when the model resolution is larger than some limit, the model results stay within the error margin from the correct result. Unfortunately, this cannot be done for the climate models. Each time the resolution is increased, the unresolved physical processes get better resolved, and the parameterizations need retuning. Until the physical processes are truly calculated there is no guarantee of convergence. Another, even more fundamental problem in the limited resolution is the problem of turbulence, which we shall return to later.

COUPLED CLIMATE MODELS

The climate models are not merely forecast models run for a long time. Beside the state of the atmosphere at the beginning of the forecast (the initial conditions) the forecast model is fed with boundary conditions such as the surface temperatures of the ocean. These variables have strong influence on the evolution of the atmosphere, but they only vary a little during the week or so of the forecast and can therefore safely be considered constant. However, for the much longer time span of the climate model the slow variables, which change with seasons, or even on longer time scales, must now be considered as variables of the system. The sea surface temperature is such an important variable in the climate that it is mostly just denoted by its acronym, "SST". The SST for the past century has been measured, mainly along shipping routes, by bucket sampling. With these measurements fed into atmospheric climate models the climate of the last century has been simulated. The models nicely reproduced the warming of the first part of the century, the slight cooling in the 1940s–60s and the subsequent warming. However, all that the models showed was that the atmospheric temperature is so strongly influenced by the heating from the oceans that the prescribed SST completely dominated the signal. The reproduction of the past climate was thus not in itself a validation of the climate modelling, it testified that the models probably responded correctly to the prescribed SST. In order to predict future climate changes the SST also needs to be foreseen. This means that not only the development of the atmosphere but also the development of the ocean temperatures and currents must be calculated.

In contrast to the atmospheric climate models, which were developed along with or from the forecast models, the ocean models cannot be guided by forecasting. For forecasting, the initial state, that is currents, temperatures, and salinities, more or less everywhere, should be known. This would be extremely costly to do, because only the ocean surface can be remotely observed, say from satellites, thus an enormous mesh of ships or buoys should be implemented. Therefore the limited economic interests in this in comparison to the investments in weather forecasting makes ocean forecasting prohibited. The expense of measuring deep in the oceans means that we only have a rough picture even of the climatology (mean state) of the oceans. Ocean general circulation models reproduce this roughly known mean state. These models are fed by the influence from the atmosphere, through heat exchange, surface winds generating waves, and water exchange through evaporation and precipitation. The coupled climate models contain a model for the circulation of the atmosphere and a model for the circulation of the oceans. The parameters in the atmosphere influencing the ocean are calculated in atmospheric models and vice versa. This sounds more straightforward than it is. Now that the two components are free to evolve together, and one part is not kept to the observed climate by the boundary conditions from the other part held fixed, the coupled models in general resulted in unrealistic model climates. This implies that the different parameterizations of physical processes need retuning.

The coupled models are the basis for the future climate projections in IPCC's fourth assessment report (2007). Judgements of the quality of the computer models are based on simulation of the 20th-century variation, where the models are fed with the observed increase in CO_2 and natural variations in volcanism and solar radiation. The global temperature shows larger natural variation, the 1940s rapid warming and subsequent cooling, than any of the models predict. This is seen in the figure taken from the IPCC report (2007), where the black curve is the observed 20th-century temperature. The light gray band is the 5%–95% range of 58 simulations from 14 different climate models. Especially the temperature variation over the oceans is not captured (the black curve falls outside the light gray band in the 1940s).

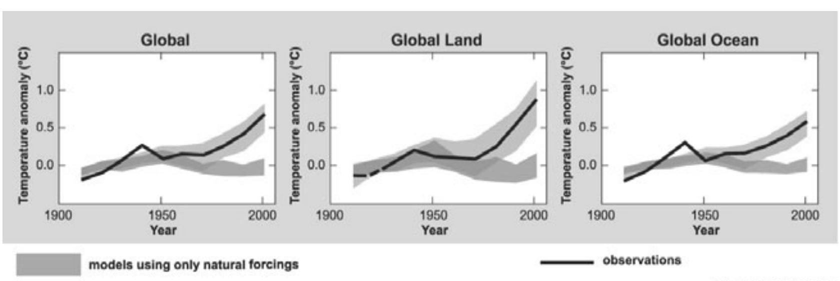

Figure 10.1 Temperature models 1900–2000, reproduced from the IPCC report (2007) with permission. (See text for explanation.)

SCALES OF PREDICTABILITY

The climate is the average of the weather, a practical definition of the climatic variable, say temperature is a running 30-year average of that variable. The distinction between predictability of the first kind, the weather prediction, and prediction of the second kind, the climate projection, is well defined in a mathematical sense. But when it comes to Nature the situation is much more complex. As mentioned above, the climatic global temperature has changed through the 20th century: Are (were) these variations predictable? Or is a period of 30 years too short to reliably determine the "true" climatic mean?

In order to answer this kind of questions, we shortly return to our simple example of throwing a die. Imagine that we do not know the number of pips on any of the sides ("pip" is the name of each of the little dots on the side of the die); we thus have to throw the die a number of times in order to observe the mean, "the climate", taken as the total sum of pips divided by the number of times we throw the die. How many times do we have to throw the die to get a reliable measure of the mean? This question can be answered very precisely in this simple case, everything can be calculated: If we want to know the mean to an accuracy of 0.5 we must throw the die 10 times. If we want to know the mean to an accuracy of 0.1 we must throw the die 250 times. (The accuracy increases inversely proportional to the square root of the number of throws.) Now, imagine a climate change where the pips on the sides of the die change slightly, say the one pip side gets another pip, so there are now two sides with two pips. For this new die the true mean is 3.67 and not the 3.5 as it was for the original die. In order to detect the change in mean, the "climate change", we now have to measure with accuracy large enough to detect the difference between 3.5 and 3.67, thus we will have to throw the die more than about 250 times. If we push this simple analogy to the limit, we imagine that the die is thrown once every month, thus we need to observe for 250 months or about 20 years in order to detect the climate change. That sets a time scale of detection. Another time scale is the time scale of change of the parameters causing the change in the statistics, say, changes in atmospheric greenhouse gas concentration. This time scale of change has to be long in comparison to the time scale of detection if the knowledge of the climate state should be relevant in the times of change. If this is not the case, the specific development of the system in transition is more important. Are we then back in the situation of a prediction of the first kind, where the future must be calculated from the present initial state?

To know if this is possible, we have to know what the time scale of predictability of the first kind is. Here our analogy with the die breaks down, because the die is unpredictable as soon as it hits the table. We must ask what the time scale for predictability in the weather-climate system is: The lows and highs passing in the west wind belt have typical length scales of 1,000 kilometres and time scales of days for passage, thus if it is possible to calculate forward in time the motion of a few passages of highs and lows, they are predictable for about a week. This is also the time scale obtained in practice in weather forecast models. But something has been sneaked in here: we have decided for a length scale of variations, namely the hundreds to thousands of kilometres of variation between highs and lows. If we were interested in, say, the specific direction of the wind in a courtyard, the time scale is completely different. As can be seen when the fallen leaves are carried erratically around in the autumn, the time scale for predictability is of the order of minutes. If we are interested in the development of a specific cumulus cloud, to determine if it will rain on a specific field or the neighbouring field, the predictability is perhaps of the order of one hour. There is thus a close connection between the spatial scales of variation that we want to predict and the time scale of predictability.

If we make a weather prediction of a day or more, which is an initial value problem, we already passed the time limit of predictability for some smaller scales, where we can thus only make predictions of the second kind. When the forecast says "showers", it is not predicting rain at a specific location at a specific time; it is predicting some probability of rain in a statistical sense. We thus have a mixture of predictions of the first kind and the second kind. This would also be the case had the models had such a high resolution that they actually resolved single clouds.

It is not only the spatial scales that determine time scale of predictability. The speed in which things vary is also important. Some parts of the climate system vary very slowly. The overturning of the ocean, which is very important for the exchange of heat and CO_2 between the atmosphere and the ocean, takes hundreds to thousands of years. There is enormous inertia in the oceans, so if we knew the present state of the deep oceanic currents to some high degree of accuracy, the large-scale flow could in principle be predictable (of the first kind) for very long times, even on the climatic time scales. These slow variations in the climate system might be responsible for the natural 20th-century variations, which the state-of-the-art climate models cannot reproduce as a function of the known solar, volcanic, and anthropogenic influences. In order to predict climate changes in the near future the climate models thus might have to accurately resolve and calculate the slow development of ocean flow, ice sheets, vegetation in the sense of an initial value problem.

THE TURBULENCE PROBLEM

In the climate system variables and conditions change on a huge range of scales (Ditlevsen 2004). So when we want to make predictions we have to focus on a specific range of spatial and temporal scales, such that whatever changes very slowly on the scale of focus can be considered constant. As an example, the continents and mountain ranges can safely be considered constant, even though they move and erode on geological time scales. On the other hand, variables, which change much faster than the time scales in focus, can be considered in an average sense. The extreme is the motion of the molecules constituting the atmosphere; the mean motion in a small portion of the atmosphere is the wind, whereas the erratic variations of the molecular velocities on top of the wind are only felt in an average sense. This average motion is the temperature of the air. These two extremes, the geologic variations on millions of years and the molecular motion at the scale of about a millionth of a centimetre are separated from the climate dynamics of days, years, metres, and thousands of kilometres. These gaps in scales are called scale separations.

In the range of weather and climate scales we face the fundamental problem that there are no clear scale separations between scales that are resolved in the climate and weather prediction models and the scales, which must be treated in a statistical sense. In its most fundamental form this is the "turbulence problem". The turbulence problem can be formulated in many ways. Consider a fluid (the air is a fluid in this connection) set in coherent motion at the large scales, which in this connection could be the size of the globe, or the size of some basin. What causes this motion could be the equator-to-pole difference in solar radiation. This motion at large scales will break up into motion at smaller scales, which in turn break up into motions at even smaller scales until eventually the variation in motion is at such a small scale that it is dissipated as heat. In technical terms, the kinetic energy of the flow is cascaded into smaller and smaller scales. The range of scales in the atmosphere, from the size of the planet to the sub-millimetre scale of dissipation is enormous and completely outside reach for resolving in any climate—or weather prediction model. We thus have to make some cut-off in spatial scales, where variations above that scale are resolved and variations below that scale are ignored. The choice of cut-off could be made naturally if there was a clear scale separation, such as the one between the fluid motion and the molecular motion, but that is not the case. The cut-off is rather set by the computational affordability.

Another way of choosing the relevant cut-off would be by improving the resolution until the obtained results do not change appreciably, as we discussed in connection with the physical parameterizations. We are thus expecting a convergence of the predictions as resolution is increased. However, as the resolution is increased, more and more of the flow is resolved and the calculated flow becomes more and more varying, and the convergence is extremely slow. A consequence of this is that the coarse resolution of the climate models implies that that calculated flow is much less variable than the real atmospheric flow, which in turn can also imply that the rare extreme events are underrepresented in the model simulations. This could be another complementary possible cause for the deficiency in the climate models to reproduce the observed variations in the mid-20th-century climate.

PAST CLIMATE AND CLIMATE PROXIES

The question whether the 20th-century climate variations can be attributed to the variation in the solar radiation, aerosols from volcanoes, and increased greenhouse gas concentration, or if there is a component of unpredictable natural variation, is still open. If we are to be sure that the observed global warming can be attributed to the anthropogenic CO_2 increase and not just a coincidental natural variation, we have to evaluate the range of natural variability. In order to do so, we need to know the variability back in time. This can only be done by indirect measures, called proxies, because the time where systematic measurements have been conducted, the instrumental record, almost only covers the industrial era, where the atmospheric CO_2 has been increasing. One proxy is the annual growth rings in trees. Their width and density depend on the weather conditions in the specific year of growth, so in this way a proxy for these weather conditions back in time can be constructed from ancient tree trunks. Other proxies can be obtained from biological sediments in lakes and oceans, corals, or records of crop yield. Each record contains different indirect measures.

A very prominent proxy can be obtained from ice cores in the ice sheets in Greenland and Antarctica. The ice sheet is built through hundreds of thousands of years from deposited snow compacted into ice. The ice sheet is thus a huge sedimentation out of the atmosphere. The main proxy, discovered by the Danish geophysicist Willi Dansgaard, is the depletion of heavy water in the ice in comparison to the ocean waters. Heavy water contains heavy stable isotopes of either oxygen (^{18}O) or hydrogen (^{2}H, deuterium). Heavy water is chemically identical to normal water, but the higher mass makes it a little less volatile. A water molecule found in the ice has undergone the process of evaporating out of the ocean, being transported as gas, being recondensed as ice crystals or water droplets in clouds, re-evaporated and recondensed several times, before eventually falling on top of the ice sheet in a snow flake. All these phase transitions on the way from the ocean to the ice sheet will differentiate between the normal and the heavy water molecules. All the processes from the evaporation somewhere in the ocean, the transport, the cloud formations, and the snowfall depend on the specific weather and climate conditions. When measuring the concentration of heavy water in the ice, some very indirect measure of the climate conditions at the time of deposition is obtained. By empirical correlation in present day's conditions, where the temperature is known, it turns out that there is a linear relationship between the depletion of heavy water and the temperature (Dansgaard 1964). The more depleted, the lower the temperature.

By drilling an ice core at the summit of the ice sheet, a very long proxy record of the past temperature has been obtained. The ice is so old that the very cold climate of the last ice age has been observed. The ice ages were of course known before then, but the ice core records revealed an unexpectedly variable glacial climate (Dansgaard et al., 1993). The climate had apparently flipped very quickly between two very different climate states during the ice age, something that no climate theories had foreseen, and something that the present state-of-the-art climate models cannot reproduce. These events are called Dansgaard-Oeschger events after their discoverers (Hans Oeschger was a Swiss glaciologist working together with Dansgaard). Even though the conditions in the ice age were very different from the conditions today, the findings open the possibility of fast irreversible climate changes either if some threshold, say in greenhouse gas concentration, is exceeded, or if some extreme weather event perturbs the climate enough to change into another state. Such behaviours are described as "tipping points".

TIPPING POINTS AND EXTREME EVENTS

In most of daily life we are used to some degree of proportionality between cause and effect. Say the cause, or "forcing", is some heating and the effect is some melting of ice. If the doubled amount of heating is applied, the amount of melted ice also doubles. However, this simple linear response does not always describe the situation. In some cases, crossing a critical threshold in the forcing will lead to a dramatic and irreversible response. This means that, if the previous sub-threshold value of the forcing is re-established, the system does not return to its original state. One such system is the ice sheet: an ice sheet will build up if the temperature is below zero at the ground for some time. In this case more snow will accumulate than will melt off at the margin. The ice sheet will eventually build up into a two- to three-kilometre mountain of ice. The temperature on top of the ice sheet will be very low, because the atmospheric temperature decreases with altitude, and the top of the ice sheet will be 1.5–2 kilometres above sea level (one-third will be below sea level, due to depression of the solid crust). In two kilometres height the temperature is almost 20 degrees lower than at sea level. Thus, if the temperature changes, so that the temperature at sea level is now 10 degrees, the top is still -10 degrees and the snow accumulated on top of the ice sheet can balance the melt-off at the margin. This is the situation in summer for the ice sheet in Greenland. If it was not there, it could not build up, but because it is there it can be sustained. There are thus two possible stable states for the ice sheet. It can be there in its present extent or it can be completely gone.

Now, if temperature is slowly increasing the ice sheet will shrink in proportion to the additional heating. However, at some point the ice sheet shrinks such that the temperature on top is above the freezing point, and it will melt back completely. The response is no longer proportional to the forcing and the system will reside in its other possible state, the one without an ice sheet. In order to re-establish the ice sheet it is not enough to return the temperature to the level where the ice sheet was previously stable. It is necessary to lower the temperature to freezing at sea level, which is a much lower temperature. This scenario is a strongly non-linear dynamic, where the system undergoes a tipping point or bifurcation. Tipping points in the climate may lead to dramatic changes, as seen in the palaeo-climatic record. If the Greenland ice sheet collapses the global sea level will rise seven metres. If this unfortunate situation should occur, it will not happen overnight, but the actual time scale for this to happen is poorly understood. The recently observed speed of shrinking of the ice sheet has been surprisingly fast in comparison to the present understanding of the ice sheet dynamics.

The climate system probably possesses more tipping points, where sudden changes to new stable configurations happen. The system has multiple stable states, which means that with the given external factors unchanged, the climate can be one of two or more possible different climates. One of the most dramatic examples is related to the state of the Atlantic Ocean circulation, the thermohaline circulation (Broecker 1997), being either in a state where warm water is transported to the north, giving a warm Northern European climate, or a state where this circulation is absent with a much colder Northern Europe.

If we were to predict a future crossing of a tipping point, the dynamical climate system should be modelled with some quantitative accuracy. Although these switches are understood in terms of simple mathematical models, when it comes to the numerical general climate models, from which the IPCC assessments for future warming are derived, it seems that the models do not show a dynamical behaviour which includes tipping points. On the contrary, the general circulation models react very linearly to (the logarithm of) the CO_2 concentration or any other perturbation applied to the model. The reasons for this are not well understood. Two quite complementary effects may cause the discrepancy between the models and our anticipation of climate change based on the palaeo-climatic records.

The first effect is fundamentally related to the way in which we conduct numerical solutions of the governing equations. The governing equations are in the technical mathematical sense “field equations”. This means that the variables we want to predict, temperature or wind, change continuously in space, thus we can ascribe a value for the temperature field or wind field in each and every spatial point for a given time. When we want to describe how these continuous fields develop in time, we can only contain the values of the fields in a finite limited set of points, and we thus reduce the one field equation to a set of equations for the values in the limited set of points. This is the computational grid, which was described above. In reality for most models the fields are split into a discrete set of waves rather than point values, but that is not important here. The procedure is mathematically well controlled and well defined. In the limit of infinitely many points covering the space, the field equation and the set of equations for each point are equivalent and yield exactly the same results.

Now, as we can only handle a limited set of variables, we make a truncation, exactly as discussed earlier when we introduced the physical parameterizations. Instead of increasing the number of points in the calculation, let us imagine that we decrease the number of points in a coarser and coarser grid. In the end, we only obtain a few coupled equations, which in technical terms are called a low order system. It turns out, when simulating such low order systems on a computer, that they exhibit multiple stable solutions and the kind of dynamics we expect for tipping points.

To understand this behaviour, consider the flow in a basin, say a bathtub. We only described the flow by monitoring or calculating in two points in each side of the bathtub near the walls. Close to the wall, the flow is parallel to the wall, say, either to the north or to the south. We know that the bathtub does not move, so the average flow is zero. This also goes for our calculation containing only the two points, so the sum of the flow in the two points will be zero. If the flow in the point to the left is towards the north, it will be towards the south in the point to the right and vice versa. Thus the flow is either clockwise or counter-clockwise in the basin. If furthermore some applied force sets the strength of the flow, we have a system, which can be in one of only two possible states and tipping, by bifurcation, between the two can happen as the force is varied. Increasing the number of points in the calculation, more and more possible flow patterns can be represented, and in the end, with very high resolution, there is a continuous set of possible flows. Thus in this example, the two distinct states and the bifurcation between them are artefacts of the coarse numerical resolution, not representing reality. In this scenario, we would not expect the bifurcations observed in Nature to be caused by the physics described in the climate models, even though low order models do exhibit a bifurcation structure.

The second effect is also related to the numerical resolution in the model. When the resolution is low, no variation on scales smaller than the distance between grid points is represented. This is the same as smoothing out all details, with the consequence that large deviations from the mean are underrepresented. These unresolved extreme events could be important for triggering large excursions in the climate, so large that tipping points may be reached. This could then explain why tipping points are not seen in the climate models. The apparently unrealistically linear response of the models to perturbations is then because of the systematic underrepresentation of the natural variability, especially the extreme events. The extremes are not primarily important for the possibility of triggering climate changes.

For construction safety, insurance, and mitigation measures, risks over long time spans must be estimated. The instrumental records of meteorological observables are for the most part only a little more than a century long, but there are historic recordings of extreme events, flooding, devastating storms, severe winters, droughts, and other extremes influencing living conditions going further back in time. From these recordings we may estimate the risks associated with extreme events. So even though we imagine that we can get a reliable picture of the natural climate by observing the mean over thirty years, such a period is much too short to estimate, say, the storm of the century. This is another reason why reconstructing the past climate prior to the time of the instrumental records is important.

PERSPECTIVES

All through human history we have anticipated the natural weather and climate based on past experiences. Giving up migration and hunting, settling for farming, with the expectation of a life-sustaining crop yield next year relies on a strong anticipation of unchanged climatic conditions. Actually, as seen from the palaeo-climatic record, climate has never before in human existence been as stable as during the last 8,000 years, coinciding with the period of agriculture. This is a striking observation, where drawing a connection between agriculture and climate stability is tempting. That can also very well be, but in order to verify it, it seems to me that previous farming attempts, ruined by climatic changes too big to adapt to, must be found. Nothing indicates that the human intellectual capacity has changed appreciably for the last 100,000 years, so it is a puzzle why agriculture did not arise before. The later enormous population growth relies on the success we have had in taming Nature. Human activity has changed local environments by deforestation and farming, but only since the industrial era has the anthropogenic change had global consequences. Climate is and will be changing due to human activity.

The challenge of predicting how and how much climate will change, and which mitigation measures will be practical and feasible, puts climate science in the eye of the storm. The numerical climate models do a fair job in reproducing the present observed climate so that we rely on their ability to forecast possible future climates as they are fed with different CO_2 emission scenarios. However, if we try to reproduce the distant past's rapid and large climate changes, as documented in the palaeo-climatic records, the models fail. Likewise, the fundamental question of how ice ages arise is still open; the numerical models do not have the capacity or dynamical range to reproduce those either. Whether the deficiency is in dominating physical processes not included or resolved in the models or if it is merely a problem of computational capacity is not clear. The solutions to these questions will probably be fundamentally different from other breakthroughs in science, like Albert Einstein's theory of relativity, which in a single paper completely resolved the issue of the constant speed of light and revolutionized our perception of space and time. The recent consensus about a discernible anthropogenic greenhouse warming did not arise with one or a few deciding findings. It rather grew out of a slow process of mounting and circumstantial evidences. This will probably continue for a while still until we have a practical experience on where the limit, if there is a limit, is to our detailed predictions for the future.

The climate models of today are so complex and computationally heavy that their behaviour cannot easily be understood. They are considered as laboratories, in what by some physicists are considered as the new third way, besides experiments/observations and theory, namely computational physics. In this third way the model development is seen as a more and more precise representation of Nature. The model computer simulations are thus believed to be maps of real or possible manipulations of Nature. However, we are left in the fundamental problem: how detailed a map of Nature should the models be? The enigma is captured in the tale by Jorge Luis Borges (1954, published in English 1972) in which a group of cartographers is assigned to draw a perfect map of the Empire. The question is to what scale it should be drawn in order to capture all the details of the Empire, and after several attempts a 1:1 map is constructed where each and every detail is copied to the map. However, the map turns out to be useless, because unfolded it covers the whole Empire, blocking out the sunlight. It thus ends up in ruins.

The development of climate models over the past three decades has been toward including more and more processes and components of Nature. This has been the standard solution to correcting for insufficiencies or inaccuracies in the model simulations when comparing with observations. The question is if this process ever stops, or if, at any point, the models are accurate or detailed enough. Even being able to understand what "accurate enough" means for anticipating an unpredictable and changing natural world is a challenge.

REFERENCES

Agassiz, L. 1840. Etudes sur les glaciers. Neuchatel: Privately published.

Arrhenius, S. 1896. "On the Influence of Carbonic Acid in the Air upon the Temperature on the Ground." The Philosophical Magazine 41, 237–76.

Bjerknes, J. and Solberg, H. 1922. "Life Cycle of Cyclones and the Polar Front Theory of Atmospheric Circulation." Geofys. Publ. 3, 1–18.

Borges, J.L., "On Exactitude in Science", in "A Universal History of Infamy", Pengiun Classics, 1972.

Broecker, W.S. 1997. "Thermohaline Circulation, the Achilles Heel of Our Climate System: Will Man-Made CO_2 Upset the Current Balance?" Science 28, 1582–88.

Dansgaard, W. 1964. "Stable Isotopes in Precipitation." Tellus 4, 436–68.

Dansgaard, W. et al. 1993. "Evidence for General Instability of Past Climate from a 250-kyr Ice-Core Record." Nature 364, 218–20.

Darwin, C. 1859. On the Origin of Species by Natural Selection. London: John Murray.

Ditlevsen, P. 2004. Turbulence and Climate Dynamics. Copenhagen: Frydendal.

Euler, L. 1757. "Principes generaux du mouvement des fluides." Mémoires de l'Academie des Sciences de Berlin 11, 274–315.

Fourier, J. 1824. "Remarques générales sur les températures du globe terrestre et des espaces planétaires." Annales de Chimie et de Physique 27, 136–67.

Friedman, R.M. 1989. Appropriating the Weather: Vilhelm Bjerknes and the Construction of a Modern Meteorology. New York: Cornell University Press.

Holton, J.R. 2004. An Introduction to Dynamical Meteorology. Burlington, MA: Elsevier Academic Press.
Hulme M. & S. Dessai 2008. "Negotiating Future Climates for Public Policy: A Critical Assessment of the Development of Climate Scenarios for the UK." Environmental Science and Policy 11, 54–70.
IPCC 2007. Climate Change 2007, Fourth Assessment Report (AR4), Working Group I: The Physical Science Basis. Cambridge: Cambridge University Press.
Keeling, C.D. et al. 1976. "Atmospheric Carbon Dioxide Variations at Mauna Loa Observatory, Hawaii." Tellus 28, 538–51.
Lorenz, E.N. 1963. "Deterministic Nonperiodic Flow." Journal of the Atmospheric Science 20, 130–41.
Lorenz, E.N. 1975. "Climate Predictability", in The Physical Bases of Climate and Climate Modelling, GARP Publication Series 16, Geneva: World Meteorology Organization (p.132–36).
Navier, M. 1823. "Mémoire sur les lois du mouvement des fluides." [Read on 18 Mar and 16 Dec 1822], MAS 6.
Owen, R. 1841. "Report on British Fossil Reptiles." British Association for the Advancement of Science (Plymouth) 11, 60–204.
Richardson, L.F. 1922. Weather Prediction by Numerical Process. Cambridge: Cambridge University Press.
Stokes, G.G. 1842. "On the steady motion of incompressible fluids." In G.G. Stokes Mathematical and Physical Papers, vol. 1, 1–16. Cambridge 1880: Cambridge University Press.

11 Constructing Evidence and Trust

How Did Climate Scientists' Confidence in Their Models and Simulations Emerge?

Matthias Heymann

In recent years the history and philosophy of computer simulation has become an active research area. Practices of computer simulation have been described as "qualitatively different ways of doing science" (Fox Keller 2003: 202), a "significant and permanent addition to the methods of science" (Humphreys 2004: 64), a "revolution in science" (Baker et al. 1977; Schweber & Wächter 2000; Dahan & Aubin 2002), or a "third way in science" (Kaufmann & Smarr 1993; Galison 1996). Also, significant problems of computer simulation have been described, particularly the problem of model validation and uncertainty (Oreskes et al. 1994; Heymann & Kragh 2010; Petersen 2006). The novelty of the methods and the epistemic problems involved in computer simulation also raise the question of how confidence in models and simulations emerged. So far, surprisingly little research has been done on this question. In this chapter I will approach this question for the case of climate simulation in its early period from the mid-1950s to about 1980. During this period climate modelling and simulation became an influential research field. It rapidly and radically changed the meaning of and interest in climate and drew attention to climate change (Weart 2010; Heymann 2009, 2010a).

Different stages can be distinguished in the development and use of climate models since the 1950s. Spencer Weart (2010) has described four stages, of which the first two are relevant to this chapter. The first stage covering the period up until 1965 was dominated by the development of numerical weather forecasting and pioneering work in the development of climate models. According to Weart climate models in this phase gave results "good enough to encourage [modellers] to persevere", but "were still a long way from reproducing the details of the Earth's actual climate zones" (ibid.: 210). Models served as a useful research instrument. Scientists "experimented" with their models by varying parameters and features in simulation runs. In a second stage, lasting roughly from 1965 to 1979, the use of climate models shifted from a pure research tool, which served the investigation of atmospheric processes, to the generation of "credible climate predictions". Influential scientific or science-based reports with a political scope drew attention to the problem of anthropogenic carbon dioxide emissions and climate warming. This made the question of how climate may change in the future all the more urgent (PSAC 1965; Wilson & Matthews 1971; SCEP 1970).

In the 1970s, controversial debates about the likelihood of future climate warming or cooling created additional incentives to use climate models for predictive rather than for heuristic purposes.[1] In this second stage, a new generation of climate scientists like William Kellogg, Stephen Schneider, and James Hansen began to develop a much stronger focus on climate prediction than the preceding generation of climate model developers. The shift from model development as a research tool for heuristic purposes to model use as a predictive tool also implied the emergence of a growing confidence and trust in the state and performance of climate models.

The aim of this chapter is to investigate the construction of confidence and trust in climate models and simulation up until about 1980. The investigation is based on analysis of the climate modellers' scientific papers, and is limited to the emergence of confidence and trust in models by climate modellers and in the climate modelling community (not in the public or other scientific communities). The terms "confidence" and "trust" are taken as almost synonymous as defined by the Oxford English Dictionary. Confidence refers to the "belief in the reliability . . . or ability" of climate models. It is also the term preferred by scientists and used by the IPCC. Trust is somewhat stronger and refers to the "firm belief in the reliability . . . or ability" of climate models.[2] Trust is an important issue in science and has been treated extensively by historians, sociologists, and philosophers of science. It gains particular significance in relation to the emergence of new scientific practices like the experimental method, quantification, or computing (Shapin 1994; Porter 1995; MacKenzie 2001; Gooday 2004). Whereas Shapin has noted that trust in Restoration natural philosophy was in many respects construed as a moral category dependent on personal contact and authority, Gooday has shown that in Victorian times instruments "were in fact regarded as positive bearers of trustworthiness in their own right" (2004: 267). I will refer to confidence and trust exclusively in this latter sense as referring to the trustworthiness of climate models.

Without claiming any comprehensiveness I will present a collection of examples, which show ways in which scientists justified the use of models and model results and both expressed and contributed to the emergence of confidence and trust in models. In the first section I will present how trust in the models had emerged in the course of the 1970s in the community of climate modellers and how such trust supported a consensus on future climate warming in the absence of observational evidence of a warming. I will argue that model validation is an important part of constructing, but not sufficient to fully explain confidence and trust in models. In the second section I will present typical examples of modelling experiences and a common pattern of framing modelling results as a sequence of statement (simulation results) and qualification (uncertainty about these results). I will argue that this framing served as a rhetorical tool in the co-construction of climate models and trust in these models. In a final section I discuss sources of confidence in climate modelling.

ON THE EMERGENCE OF CONFIDENCE IN MODELS

In the late 1970s a consensus emerged among climate scientists that rising concentrations of carbon dioxide in the atmosphere will cause future global warming. A report commissioned by the U.S. National Academy of Science and published in 1979 concluded: "If carbon dioxide continues to increase, the study group finds no reason to doubt that climate changes will result and no reason to believe that these changes will be negligible" (Charney et al. 1979: xiii). This report was prepared by a study group of leading scientists headed by the pioneer of numerical weather forecasting Jule Charney. Its conclusion was based on a detailed investigation of the available knowledge on climate change including the results gained by applying climate models developed by Syukure Manabe and James Hansen. Also in 1979, the WMO World Climate Conference held in Geneva came to similar conclusions in its "Final Declaration": "It is possible that some effects on a regional and global scale may be detectable before the end of this century and become significant before the middle of the next century. This time scale is similar to that required to redirect, if necessary, the operation of many aspects of the world economy, including agriculture and the production of energy" (WMO 1979: 714).

Publications by climate scientists William Kellogg and James Hansen, published in the same period, supported these conclusions. In 1976 the Commission of Atmospheric Science of the WMO asked Kellogg, a leading atmospheric scientist and director of the Laboratory of Atmospheric Sciences at the National Center for Atmospheric Research (NCAR), to prepare a report "on the influence of human activities on climate" (Kellogg 1977: Foreword by Secretary General of WMO). Kellogg was well versed in questions related to climate change. He had been chief organizer of the international Study of Man's Impact on Climate (SMIC) held in Sweden in 1971 and had devoted himself to full-time climate research since 1973 (Kellogg 1987). In 1977, Kellogg's report was published as WMO Technical Note No. 486. In this report, Kellogg discussed the state of the art of climate modelling and provided what he called a "best estimate" of future global mean surface temperature. According to his assessment climate models had been developed "to the point that a . . . prediction can be made" (Kellogg 1977: 7). Based on model calculations he expected a warming of 1°C by the year 2000 (due to an assumed 25% increase of carbon dioxide concentration) and of 3°C by 2050 (due to a doubling of carbon dioxide concentration by that time). According to Kellogg, this prediction involved "an uncertainty of roughly a factor of two" (ibid.). Kellogg summarized his findings in a graph, which shows past mean surface temperature based on observations since about 1870 and future projections of surface temperature until 2050 (Figure 11.1).[3] The most striking feature of this graph is that past temperature variations remained small compared to expected warming in the years and decades to come.

Climate scientist James Hansen and his co-workers at the NASA Institute for Space Studies (later named Goddard Institute of Space Studies—GISS)[4] came to similar conclusions. In a landmark paper published in *Science* in 1981 they predicted climate warming based on model simulations. Hansen was an expert in solar radiation transfer in planetary atmospheres. In 1970, he began modelling radiative transfer in the Earth's atmosphere and shifted to modelling climate on Earth as his primary research field. During the 1970s, Hansen became a leading climate modelling expert. The 1981 paper represented a summary of his modelling work. It provided an extensive discussion of model details and findings. For the first time the authors attempted to show that past climate change was driven by changes of carbon dioxide concentration, aerosols from volcano eruptions, and the variability of solar radiation (see below in the third section). In the latter part of the paper, the authors concluded that the "projected global warming for fast [economic] growth is 3°C to 4.5°C by the end of the next century, depending on the proportion of depleted oil and gas by synfuels" (Hansen et al. 1981: 964). A graph illustrated past mean temperatures based on observations since 1950 and future scenario projections of mean temperature (Figure 11.2).

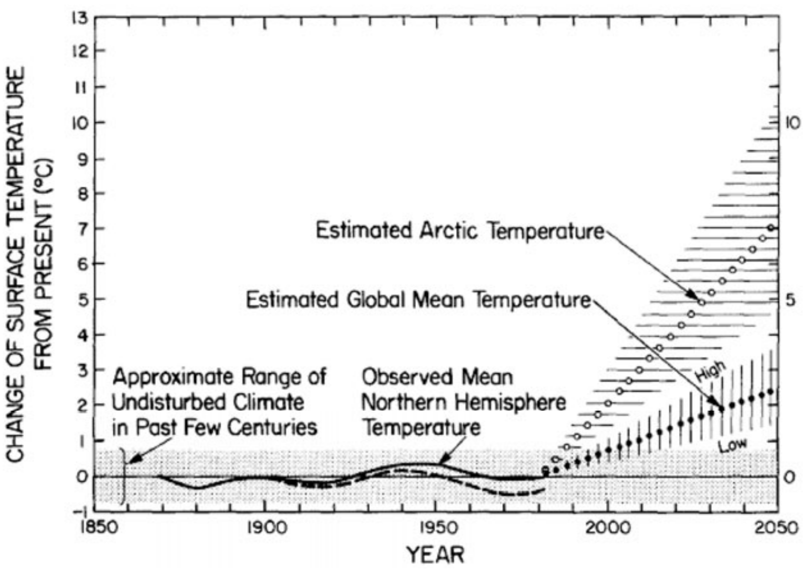

Figure 11.1 Estimates of past and future global average temperature variations (Kellogg 1977: 24). Reprinted with permission from WMO.

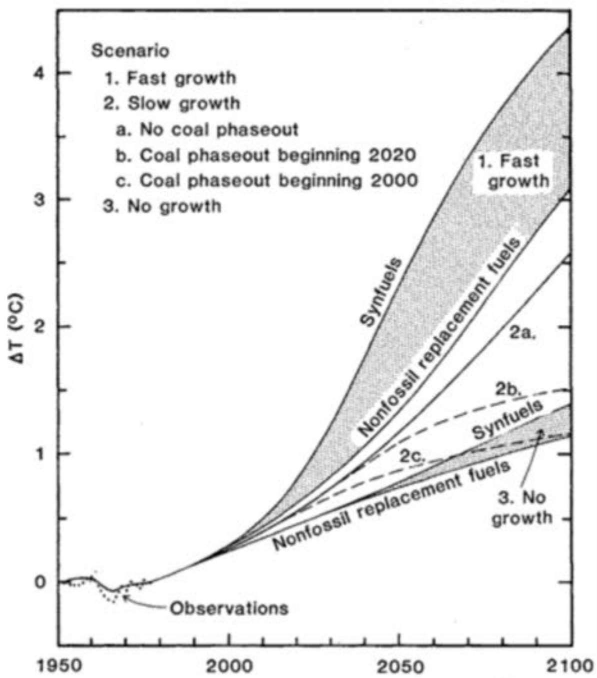

Figure 11.2 Estimates of past and future temperature variations (Hansen et al. 1981: 965). Reprinted with permission from AAAS.

The graphs published by Kellogg and Hansen highlight the crucial role of climate models for climate prediction. Likewise, they indicate the confidence placed in climate simulation. At the time when these graphs and predictions were published, no observational evidence of climate warming existed. Throughout the 1970s, this apparent contradiction had nurtured debate on the question whether a future warming or a future cooling was to be expected (Peterson et al. 2008). Climate models contributed significantly to resolving this question.[5] More pertinently, climate models led scientists like Kellogg and Hansen to the conclusion that this point in time (around 1980) marked a fundamental shift of truly historical dimensions: the shift from a period of stagnating or stable global temperatures to a period of quickly and persistently rising temperatures to levels far beyond those experienced in past centuries. It should be borne in mind that this conclusion was based on a comparatively new research instrument: computer-based climate models. It was reached even though climate modellers frankly discussed significant model weaknesses such as approximations, incompleteness, and uncertainties (e. g. in Hansen et al. 1981). These observations, therefore, raise an important question: How did climate scientists develop confidence in the performance and reliability of climate models?

Climate modellers face the significant problem that an evaluation of models is difficult to achieve. Any computer model involves numerous simplifications and approximations. The coded computer model represents a human-made virtual atmosphere, which scientists hope reflects the characteristic processes and features of the real atmosphere and allows for reliable simulation of atmospheric processes like weather or climate. The quality of the model representation and of model simulations, however, is difficult to establish (Oreskes et al. 1994; Parker 2010). A crucial practice in evaluating the performance and reliability of models is model validation, the testing of a model by comparing simulated and observation-based data. Confidence in models will only emerge if the model is able to reproduce patterns known from observation. But validation procedures alone do not suffice to explain the emergence of confidence in models. Validation is not a clear-cut and defined procedure for all instances and model types; and validation suffers from significant limitations. How often (and for how many cases) does a model have to be validated in order to be considered reliable? And how reliable is a model that may be used to simulate those cases which cannot be validated by observations (for instance, future atmospheres with a different composition of compounds or otherwise different characteristics)? Furthermore, validation may be limited due to a lack of observational data (Oreskes et al. 1994).

Validation also suffers from a number of fundamental problems. The holistic character of computer models produces what Paul Humphreys called "epistemic opacity". According to Humphreys, "the dynamic relationship between the initial and final states of the core simulation is epistemologically opaque, because most steps in the process are not open to direct inspection and verification" (Humphreys 2004: 137–38). As a consequence, characteristics of simulation results cannot clearly be associated with particular parts of or processes in the model. Although the modeller may recognize whether simulation results are "good" or "bad", she or he usually cannot easily establish why the results are bad and which parts of the model (or input data) need to be improved; neither can he decide whether model results are "good" for the right or the wrong reasons (Lenhard & Winsberg 2010). Another fundamental problem of computer simulation is the fact that model uncertainties cannot be described by any objective quantitative measure with a defined statistical meaning. From the very beginning atmospheric and climate modellers were aware of uncertainties and thoroughly discussed them in their papers (see below; for the case of atmospheric pollution modelling see Heymann 2006). But they could not conclude with any certainty how large and how significant these uncertainties were and with which probability a simulation result would be within a certain interval around the real value. Published uncertainty ranges rested on expert estimates.[6]

During the 1970s, validation had not been addressed systematically as a topic in its own right. Modellers were aware of model uncertainties and emphasized the need for model testing by comparing simulation results with observation-based data (e.g. Phillips 1956; Manabe & Strickler 1964; Hansen et al. 1978, 1981). But decisions about what constituted model testing, which procedures it had to involve and which standards it had to meet, usually rested with the modellers and varied among modelling teams. A more intensive debate about validation and validation standards only emerged after climate models had raised strong political and public interest. The IPCC addressed model validation systematically, referring to it with the broader and less suggestive term "model evaluation" (IPCC 1995, Chapter 5; IPCC 2007, Chapter 8). According to Guillemot (2010) validation has remained a contingent practice to this day shaped by local demands, styles, and norms. For the case of climate simulation in France, she observed that no general protocol for the validation of climate models existed. Norms of validation, she asserts, are defined at the same time as the simulation results to be validated.

The emergence of confidence in models cannot simply be explained by model validation. It seems to be more of a question of what the scientists consider an appropriate validation. Furthermore, no validation procedure can guarantee reliability and performance for future applications. At which point, and for what reason, do scientists believe in it? The emergence of confidence in climate models is clearly a complicated process. But some first insights can be gained by an analysis of scientific papers on climate simulation.

THE CO-CONSTRUCTION OF MODELS AND TRUST IN MODELS

Climate modelling and computer-based climate simulation have only recently become topics of growing historical and philosophical research (Edwards 2010; Gramelsberger 2010). Within a few decades simple climate models representing a few atmospheric processes with extremely coarse horizontal and vertical resolution morphed into increasingly sophisticated and complex representations of the atmosphere and manifold geophysical processes linked to it (IPCC 2007: 99).

The prototype model, a first computerized general circulation model (GCM), was developed by Norman Phillips at Princeton in 1955 (Phillips 1956). It consisted of only two vertical layers and a horizontal resolution of 625 kilometres. The model ignored any geographical details and contained no moisture or clouds (Lewis 1998). Phillips assumed an inactive atmosphere with no air movement and uniform temperatures as initial conditions in his simulation. His results, the emergence of flow patterns in the virtual atmosphere, looked very similar to observed flow patterns, however. "It is of course not possible to state definitively that this . . . is a complete representation of the principal energy changes occurring in the atmosphere, since our equations are so simplified", Phillips concluded, "but the verisimilitude of the forecast flow patterns [with observed patterns] suggest quite strongly that it contains a fair element of truth" (Phillips 1956: 154).

Phillips drew considerable reassurance from this experience. The fact that the model included representations of basic physical laws which allowed for the reproduction of patterns familiar from observations was seen as strong confirmation of the modelling approach. Such patterns, which emerge spontaneously out of the computations without being built into the models from the start, are called "emergent features" (Weart 2010).

Phillips' experiment and experience was paradigmatic. Other scientists who took up climate modelling attempted to represent further physical processes in their models and, similar as Phillips, reproduce patterns known from observations. Syukuro Manabe and Robert Strickler, for example, developed a model of radiative transfer in the atmosphere in order to calculate the impact of solar radiation and atmospheric processes like clouds on energy transfer processes in the atmosphere. Their model allowed the calculation of vertical temperature profiles, which resembled observed average temperature profiles and fitted well with the measured data (Figure 11.3). "This comparison", Manabe and Strickler concluded, "shows the degree of similarity of our thermal equilibrium atmosphere to the actual atmosphere" (1964: 374).

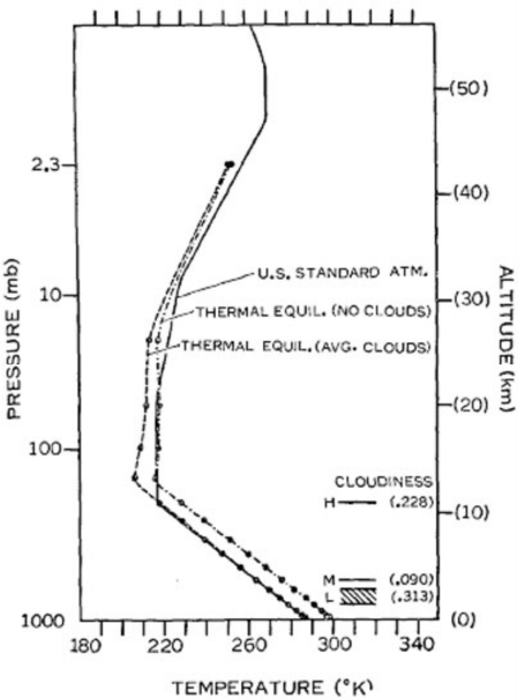

Figure 11.3 Thermal equilibrium of the atmosphere with and without cloudiness according to model simulations (dotted), and the U.S. Standard Atmosphere based on observations (straight line) (Manabe & Strickler 1964: 373). © American Meteorological Society. Reprinted with permission.

Although this result represented a reassurance of model performance, Manabe retained a keen sense of the intricacies and uncertainties of climate models. In a later paper, Manabe and Wetherald presented first climate projections for the case of doubled carbon dioxide concentrations in the atmosphere. According to simulations with their one-dimensional model, global mean temperature would rise roughly 2°C (Manabe & Wetherald 1967). In 1975, Manabe and Wetherald published similar investigations with a much more detailed three-dimensional model. They now used a GCM with nine atmospheric levels and incorporated the movements of water, changes in soil moisture, and snow cover on land. The model still included radical simplifications. In place of actual land and ocean geography it pictured a planet with half damp surface (land) and half wet (a "swamp" ocean) with constant cloudiness (which could not be predicted). With doubled carbon dioxide the model predicted an average warming of around 3.5°C. The authors warned, however, that "it is not advisable to take too seriously" the specific numbers the model produced (Manabe & Wetherald 1975: 13). The impact of the oceans was inadequately represented and the impact of clouds remained "the old vexing problem" (Weart 2010: 212).

In the early 1970s, a new generation of climate modellers such as Stephen Schneider, William Kellogg, and James Hansen entered the field. They introduced a sense of political urgency into climate modelling, pursued higher predictive ambitions, and compromised in model development and use with a strong element of pragmatism. In a popular book published in 1976, Schneider emphasized the uncertainties of climate models, but argued for using them anyway. "Unfortunately, *for the task of estimating the potential impact of human activities on climate the models are just about the only tools we have*", Schneider explained (italics by Schneider) and concluded: "Should we ignore the predictions of uncertain models? . . . I think not—a political judgement, of course" (Schneider 1976: 147–48). "My view is that once we know reasonably well how an individual climatic process works and how it is affected by human activities (e. g., CO_2-radiation effect), we are obliged to use our present models to determine whether the changes induced by these human activities could be large enough to be important to society" (ibid.: 148). Schneider even compared the uncertain climate models with a fortune teller's dirty crystal ball, noting that as the consequences of climate change were expected to be severe, there would be only limited time to "clean the glass before acting on what we believe we see inside" (ibid.: 149).

Hansen also regarded climate modelling as a politically relevant scientific activity and pursued a pragmatic and application-oriented style. In an interview with Spencer Weart he explained the difference between his style and that of the climate modelling pioneer Akio Arakawa. Arakawa wanted to construct the perfect model and kept focusing on improvements in model design. Hansen explained: "He will always be in the design, I think. You know, if you want to do real applications, then you really have to just be willing to go ahead and do something . . . We're taking the model and using it for climate applications. It's hard to have enough time to work on the basic structure of the model and also use it" (Weart 2000).

Kellogg's, Schneider's, and Hansen's emphasis on the political importance of climate modelling did not invalidate the need to develop confidence in models and simulations. A few examples will serve to show how they developed and contributed to the construction of confidence in models. As an engineering student at Columbia University, Schneider was asked by the atmospheric scientist Ichtiaque Rasool of the Goddard Institute of Space Studies to develop a simple one-dimensional radiative convective model in order to investigate whether future warming due to rising carbon dioxide levels or future cooling due to rising aerosol levels was to be expected. Schneider knew little about these models and their deficiencies. "Nonetheless, I was drawn to the power of the idea", he explained in his biographical recollections. "We could actually simulate Earth's temperature and then pollute the model in order to figure out what might happen before we had polluted the actual plane" (Schneider & Flannery 2009: 21).

In a paper in *Science*, Rasool and Schneider described the main features of their model and their simulation results. Based on their simulations they predicted a future cooling (Rasool & Schneider 1971). Confidence in their simulations and findings came from comparison of specific model results with limited observational data. "The values given by the model atmosphere described above for both outgoing and incoming radiation seem to be in close agreement with the values measured by meteorological satellites . . . We conclude, therefore, that the model reflects the present-day conditions of the atmosphere of Earth" (Rasool & Schneider 1971: 139). Rasool and Schneider took the agreement of calculated and measured data as sufficient confirmation of the model to make the much broader claim that it reflected present-day conditions of the atmosphere. As it turned out later, their prediction of future cooling proved utterly wrong. Using a simple one-dimensional model, Rasool and Schneider assumed a distribution of aerosols over the whole planet. A group around William Kellogg showed later that this assumption was mistaken. Most aerosols only existed over land close to the place of production and, thus, contributed much less to cooling than Rasool and Schneider's paper assumed (Kellogg et al. 1975; Kellogg 1977: 17–18).[7]

Schneider's experience with predicting the effect of aerosols taught him about the intricacies of computer modelling. A little later, after he had moved to NCAR, his 1974 overview paper for *Science* written with close collaborator Kellogg expressed a cautious view on climate prediction. Here they emphasized the complexity of atmospheric processes and called it a "monumental challenge" to build a climate model including all relevant feedback mechanisms and simulate climatic change (Kellogg & Schneider 1974: 1164). "So far, we do not have a comprehensive climate theory that can explain—much less predict—these trends and anomalies" (ibid.: 1163). Still, they considered the development and use of climate models useful, in particular when used as heuristic tools to investigate climate processes. As they noted, "while general circulation models (GCM's) are essential tools for evaluating the relative magnitudes of competing feedback processes, they may not be practical tools for long-term climate forecasting for many years (except possibly for seasonal or interannual forecasts)" (ibid.: 1166).

One year later Schneider published another overview paper on what he called "the carbon dioxide climate confusion" and provided an assessment of the predictive capacities of climate models (Schneider 1975). In the abstract he made the following statement: "Based on current understanding of climate theory and modelling it is concluded that a state-of-the-art order-of-magnitude estimate for the global surface temperature increase from a doubling of atmospheric CO_2 content is between 1.5 and 3 K, with an amplification of the global average increase in polar zones" (ibid.: 2060). Right after this statement he added an important qualification. "It is pointed out, however, that this estimate may prove to be high or low by several-fold as a result of climatic feedback mechanisms not properly accounted for in state-of-the-art models" (ibid.). This sequence of statement and qualification is striking. Although the statement appears to give a proper scientific result (the likely increase of temperature within certain ranges), the qualification suggests that this statement is so unreliable that results appear questionable.

Such a sequence of statement and qualification or even statement, qualification, statement is characteristic of scientific writing in papers on climate modelling in the 1970s (and possibly beyond). In his 1977 report, Kellogg provides another example pointing out the importance of a multiplicity of climate models: "It can be seen, then, that there is an entire hierarchy of models of the climate system . . . It is reassuring to see that, when we compare the results of experiments with the same perturbations . . . but using different models, the response is generally found to be either about the same or differs by an amount that can be rationalized in terms of recognized model differences or assumptions" (Kellogg 1977: 9). Also Kellogg adds a qualification right away. "Of course, it is possible that all our models could be utterly wrong in the same way, giving a false sense of confidence", he goes on to rescind on this qualification, "but it seems highly unlikely that we would still be so completely ignorant about any dominant set of processes" (ibid.).

One of the most difficult problems in climate modelling was (and still is) the understanding and representation of clouds (IPCC 2007: 592–93; Weart 2010: 212). Clouds are usually much smaller than the size of a grid element of the model and, thus, cannot be resolved explicitly, but need to be parameterized. In the mid-1970s a group of scientists around Schneider attempted to develop a parameterization for the formation of clouds. For clouds of a low height (which was taken as 3 kilometres) they developed a simple formula including a parameter R*, which they called "effective humidity":

$$C_l = 2.4R_l^* - 1.6$$
$$(0.2 \leqslant C_l \leqslant 0.8;\ C_l = 0.2 \text{ if } w < -2 \text{ cm s}^{-1})$$

The authors explained: "R* is not the actual relative humidity at the cloud level l . . . but rather some effective relative humidity computed on the basis of linear interpolation in mixing ratio and temperature at adjacent grid points (which are 1.5 kilometres above and below the cloud levels)" (Schneider et al. 1978: 2209). This formula was a pragmatic attempt to fit limited observational data, which it did quite well (Figure 11.4). The similarity of patterns of modelled and observational data served as the main argument to fend off critical remarks with regard to the scientific content of the formula. "We are not attempting to rationalize the particular choice of R*, or the limits of C_l . . . or the cutoff value of w used in this version of the NCAR GCM as the best possible, but merely are pointing out the fact that, although R* is not precisely the relative humidity at the cloud level, this does not automatically invalidate the parameterization. Rather, it is more important to test its results in a simulation" (ibid.). Also in the case of a rough parameterization to be put into the climate model, the similarity of calculated and observed patterns served as a justification of the parameterization, even if the scientific understanding was admittedly insufficient.

Similar examples of argumentation and justification can be found in James Hansen's papers on climate modelling. Hansen's group at the Goddard Institute of Space Studies pursued a number of important investigations with a one-dimensional model, because it required much less computer power than a more realistic three-dimensional model (Wang et al. 1976). The authors discussed the chances and problems of using a one-dimensional model. They stated: "It is conceivable that one-dimensional models even provide a good first-order estimate of the effect of the assumed perturbation on the Earth's average thermal structure", and then added the qualification, "but it is difficult to be confident of that in the absence of reliable fully interactive climate models" (ibid.: 687).

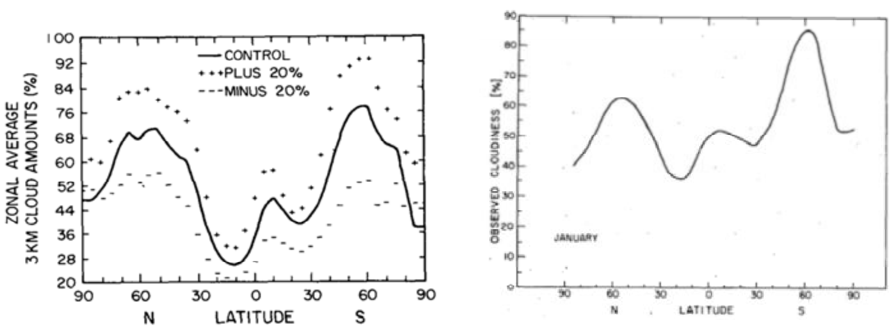

Figure 11.4 Parameterization of cloudiness (left) and observed cloudiness (right) in latitude zones (Schneider et al. 1978: 2209, 2210). © American Meteorological Society. Reprinted with permission.

Hansen and his co-workers attempted to validate their model by testing the impact of a volcano eruption in 1963 on mean temperature. The eruption of Mt Agung in Indonesia in February 1963 caused the release of tremendous amounts of sulphur aerosols which affected global climate significantly. It caused a temporary increase of temperature in the stratosphere and a decrease of temperature in the troposphere (Figure 11.5). In simulation experiments with the GISS model both these temperature changes could be reproduced. The authors concluded: "Despite the uncertainty, the results are in excellent agreement with the observations" (Hansen et al. 1978: 1067).

They added a crucial qualification: "The very close fit to the theoretical curve . . . is almost certainly fortuitous, particularly in view of the noisy appearance of the observed temperatures in earlier and later years . . . We believe that the greatest weakness of our model computations is the absence of interactions of the computed heating with the atmospheric dynamics . . . A related defect of the computations is the omission of potential cloud cover feedbacks" (ibid.).

In spite of these reservations the authors drew an optimistic conclusion: "However, the extent to which the observed climatic effect agrees with that obtained from [the] simple radiative model in fact provides some evidence

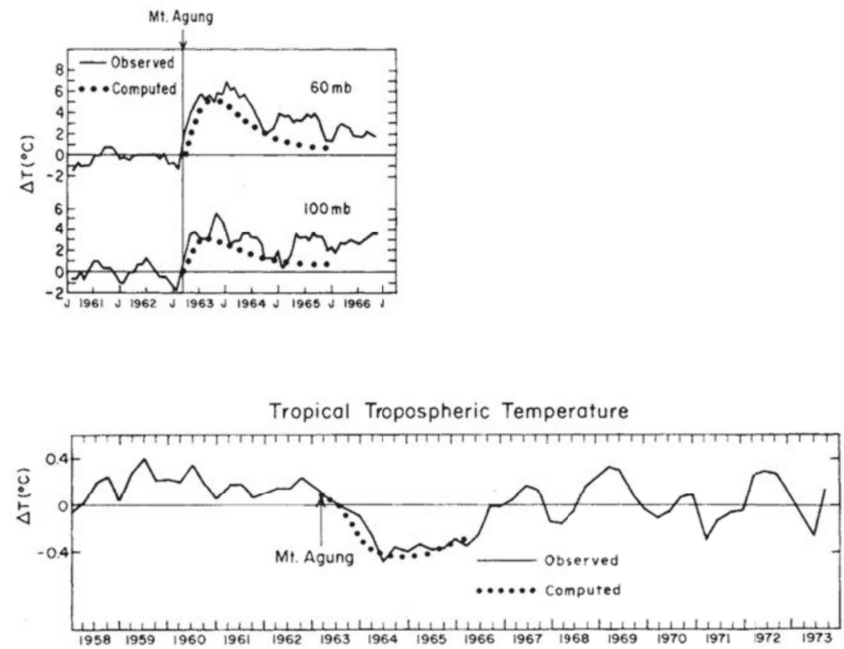

Figure 11.5 Increased temperatures in the stratosphere (above) and decreased temperatures in the troposphere (below) after the eruption of Mt Agung, Indonesia (Hansen et al. 1978: 1065, 1066). Reprinted with permission from AAAS.

that such potential feedbacks do not overwhelm the direct radiative effect" (ibid.). Once again, a most important source of confidence was the "excellent agreement" of calculation and observation.

In 1981, Hansen and co-workers published a landmark paper in *Science*, which was also based on simulation results of the one-dimensional model (Hansen et al. 1981). In this paper the authors listed problems and uncertainties with great accurateness. Just a few examples will suffice here. According to the authors no reliable assessment of vegetation albedo feedback was possible (ibid.: 958–59). The "lack of knowledge of ocean processes partly introduces uncertainties about the time dependence of global warming" (ibid.: 959f). Also, "The impact of tropospheric aerosols on climate is uncertain in sense and magnitude due to their range of composition" (ibid.: 960). Furthermore, "The nature and causes of variability of cloud cover, optical thickness, and altitude distribution are not well known" (ibid.: 960).

In spite of these uncertainties, the authors achieved an excellent agreement of calculated and observation-based data by assuming carbon dioxide concentration, aerosols from volcano eruptions, and variations of solar radiations as the main drivers of climate change (Figure 11.6). They concluded: "The general agreement between modelled and observed temperature trends

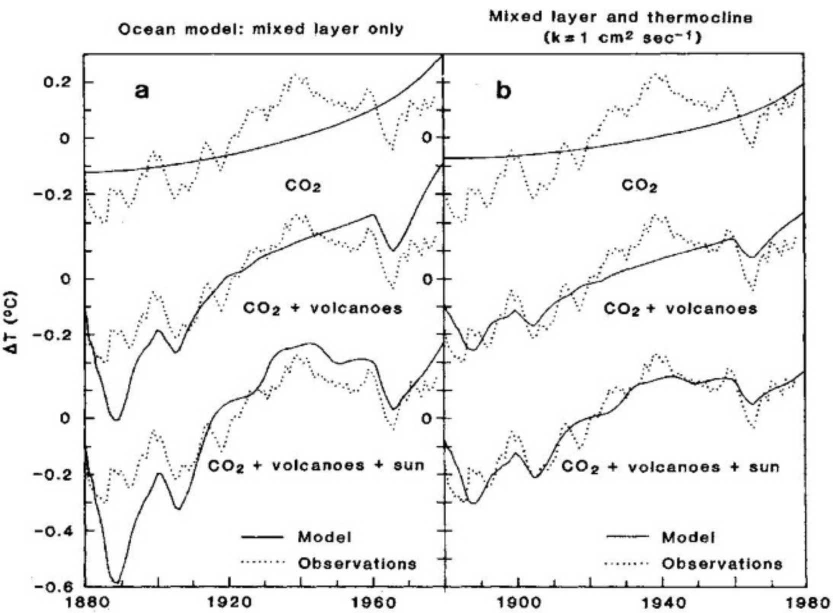

Figure 11.6 Validation experiments of Hansen's one-dimensional climate model with the inclusion of various drivers (CO_2, volcanoes, and solar radiation) and a simplified (left) and explicit ocean model (right) (Hansen et al. 1981: 963). Reprinted with permission from AAAS.

strongly suggests that CO_2 and volcanic aerosols are responsible for much of the global temperature variation in the past century. Key consequences are: (i) *empirical evidence* that much of the global climate variability on time scales of decades to centuries is deterministic and (ii) *improved confidence* in the ability of models to predict future CO_2 climate effects" (ibid.: 964; italics by the authors of the paper).

This conclusion reflects the degree of trust in the climate model and in the climate simulation that had emerged by this time. Although uncertainties mattered and had to be investigated, the authors felt justified to infer, first, "empirical evidence" of deterministic climate variability (in contrast to chaotic weather variability) giving this finding the epistemic status of an observation and, second, "improved confidence" in the ability of models to simulate this variability.[8] At the same time, it provides a telling example of the construction of trust by framing an interpretation, which granted positive simulation results predominant weight over any qualification due to acknowledged (but quantitatively unknown) uncertainties. The rhetorical construction of statement and qualification represented a common pattern. The statement gained visibility and prominence by being supported by the materiality, weight, and precision of quantitative numbers and graphs. The qualification, in contrast, remained diffuse, lacking the materiality, weight, and precision of quantification. Model builders concomitantly engaged in constructing models, constructing evidence, and constructing confidence and trust by framing simulation results in terms of statement and qualification and expressing confidence in the statement. After all, the construction of models only made sense by co-constructing models and trust in models.

SOURCES OF CONFIDENCE IN CLIMATE MODELS AND THE EMERGENCE OF TRUST

Climate scientists quickly recognized the potential of computer simulation and made ample use of it. By the late 1970s, the application of climate models for prognostic purposes had been explored and accepted in the climate modelling community. At the same time the complexity of atmospheric and climate processes and the crude and premature state of climate models had been acknowledged. The impact of clouds or aerosols on climate and the role of the oceans still remained unsolved questions. Modellers also knew that they could never be sure not to have misrepresented relevant processes and missed others altogether. Neither could they be certain that they had reached useful results for the right or the wrong reasons. Still, until the late 1970s, when the consensus about climate change took shape, no systematic and coherent discussion about model uncertainties and standards and problems of validation can be found in the reports and papers. However, there are indications that certain practices and findings contributed to confidence and trust in models, and a number of distinct sources of confidence can be inferred from the argumentation and forms of justification in the scientific papers.

A first source of confidence was the emergent features that the climate models produced in simulation runs. The models were based on physical laws and it proved reassuring that the elements of this established physical theory embedded in the models (even though simplified in many regards) produced the kinds of patterns expected from observations, even though the individual computation steps remained largely opaque to the modeller. Although the patterns were not a complete match to the data from observations, they were considered important evidence that model development was on the right track. A second source of confidence resulted from the experience that quantitative fits between simulated and observation-based data or patterns could be achieved or improved, even if they did not emerge spontaneously. Most achievements in simulation resulted from painstaking experimentation with the model including iterative model- or data-adjustment and tweaking. A striking example was Hansen's achievement to simulate past global mean temperatures over a period of about 100 years (see Figure 11.6). These sources of confidence resemble those the IPCC emphasized in its 2007 report.[9]

The accomplishment of a good fit between observation-based and simulated data was the most important form of model validation and considered a precondition of any meaningful model application. Even though validation usually suffered from limitations of observation-based data particularly in this early period of modelling, it provided significant reassurance and contributed to the confidence in climate models. It has been pointed out that the comparison of simulated and observation-based data was far from straightforward. Climate models (like other atmospheric computer models) only provided highly aggregated average data which could not be compared directly with observational data. Data sets had first to be constructed from the observational data before a comparison with simulated data could be made (Oreskes et al. 1994; for the example of atmospheric chemistry modelling see Heymann 2010b). This limitation, however, was rarely discussed as a fundamental problem in the 1970s.

All models involved approximations, radical simplifications and significant uncertainties, the impact of which on model performance proved hard to fully assess. Model validation could give limited information on strengths and weaknesses of a model. An important additional means of model assessment became model comparison (e.g. Gates 1975). The multiplicity of models with similar behaviour provided an important reassurance and can be regarded a third source of confidence. In striking contrast to physical theory, where physicists usually accept only one consistent theory for a certain domain as a valid scientific result, the contrary was true for the case of "climate theory" (as Kellogg and Schneider called it). Due to its complexity it could only be represented in the form of computer models. And due to the large degree of freedom in constructing climate models and the difficulties of validating and assessing them, it proved meaningless to adhere to the traditional norm in physics of there being one single valid model. The existence of a multiplicity of models proved not a cause for doubt, debate, and distrust in climate theory, but—given their similar behaviour—a source of trust in climate models.

Finally, a fourth and rather paradoxical source of confidence needs to be addressed: the invisibility of uncertainty in simulation results. Although climate modelling involved significant uncertainties and the modellers were well aware of it, no defined measure of uncertainty such as statistically sound uncertainty margins could be constructed (Parker 2010). The extent or range of uncertainty remained unknown. Although single parameters could be subject to sensitivity studies, which gave an idea how sensitive the model was to uncertainties of that parameter, the aggregated uncertainties of a model simulation remained inaccessible. It was literally open whether multiple uncertainties more or less evened out or, in contrast, added up to 10%, 100%, or 1,000%. Climate scientists sometimes provided estimates such as "high or low by several-fold" (Schneider) or "roughly a factor of two" (Kellogg). Such estimates represented the educated guesses of experienced researchers and had no defined statistical meaning.

Ignorance about the extent of uncertainty raised the problem of what to do with it. Although simulation runs produced clear and distinct quantitative results, the uncertainties surrounding such results represented a diffuse and indistinct background, which many scientists honestly discussed to their best knowledge, if only in such a way that they did not compromise basic conclusions from the simulation results.[10] The characteristic construction of arguments in many climate modelling papers, the sequence of, first, a statement expressing a scientific finding, second, a qualification expressing uncertainties related to the statement, and sometimes, third, a reiteration of the statement reflected this relation and made up its rhetorical representation. The statement gives a scientific achievement, which the qualification devaluates only to a certain degree. Though the uncertainty is highlighted and puts a question mark over the scientific findings, these findings are still out in the world. Once the statement is made, it exists and persists. The level of uncertainty surrounding the results, however, is unclear. As a consequence, ignorance about the level of uncertainty tended to produce a more general effective ignorance of uncertainty. It made uncertainties less visible, if not invisible.[11]

Emerging trust in models (growing belief in the ability of models) and the construction of trust in models (building belief in the ability of models) went hand in hand. Here, psychological processes have to be considered, which are beyond the scope of this chapter and can only be touched upon. The limited visibility of model uncertainties has a material component with implications for scientific perceptions. Model runs produced a huge amount of data on various interesting parameters, but they did not produce any real quantitative information on uncertainties. The extensive discussion of uncertainties (e.g. in the papers of Hansen and his co-workers) was a matter of scientific honesty and discipline (maybe also as a response to comments by reviewers), not a direct outcome of climate simulation. Extensive working, experimenting, and playing with simulation models and data created growing familiarity with and trust in these tools. Spencer Weart accurately described the importance of multiple runs of climate models for the scientist's understanding. "In such studies, the global climate was beginning to feel to researchers like a comprehensible physical system, akin to the systems of glassware and chemicals that experimental scientists manipulated on their laboratory benches" (Weart 2010: 211). This condition may have supported a seductive element in simulations, which Myanna Lahsen described (2005).

Although a number of sources of confidence from the analysis of scientific papers can be named, this investigation can only be a starting point. The emergence of confidence and trust involves more than scientific arguments. It is not simply a product of scientific reasoning and rhetorical framing, but a complex social process, for which political, cultural, and biographical contexts play a crucial role (Shapin 1994). Scientific papers can hardly explain why scientists like Kellogg, Schneider, and Hansen developed a stronger political interest and a more application-oriented interest in climate prediction than the preceding generation of pioneers. Personal experiences and motivations as well as broader political contexts in an era of student revolts and environmentalism are likely to have played a significant role in the emergence of trust in climate models and prediction.

NOTES

1. On the distinction of heuristic and predictive model use see Dahan 2001.
2. The full Oxford English Dictionary definition is "belief in the reliability, truth or ability", but it is uncommon to refer to the term "truth" in relation to climate models. Oxford English Dictionary, available online at http://oxforddictionaries.com/.
3. The climate model provided global mean temperature change for a doubling of CO_2 in 2050. In the graph Kellogg assumed for the lack of more detailed knowledge a linear temperature increase between 1980 and 2050. This graph was republished in Kellogg and Schware 1982, p. 1085, and Kellogg 1987, p. 124.
4. I will use the latter name in the rest of the chapter.
5. CO_2 was believed to contribute to warming whereas aerosols were expected to cause cooling. Climate models indicated a predominance of the warming effect in the longer term.
6. In the case of global mean temperature projections by climate models the order of magnitude of uncertainty ranges given by scientists have not changed significantly since the 1970s (Kellogg 1977: 7; IPCC 2007: 98).
7. The authors even made the claim that aerosols contributed to warming instead of causing a cooling. However, more detailed investigations in subsequent years, among others by Hansen, came to the conclusion that most aerosols did indeed contribute to cooling (Hansen et al. 1981). Until this day the actual effect of aerosols on climate has remained uncertain.
8. The emphasis on deterministic climate variability represented a response to criticism by Edward Lorenz, who raised general doubt about climate

modelling because of the chaotic character of atmospheric processes (Lorenz 1970). Kellogg refuted Lorenz' conclusions by arguing that climatic change can be distinguished from chaotic weather processes and is driven by external factors like CO_2 concentrations of solar radiation in a deterministic way. Whereas weather prediction is impossible for longer periods than a few days, long-term climate prediction represents "a second kind of prediction" that "can be made" (Kellogg 1977: 7).

9. The IPCC listed three "sources of confidence": first, "the fact that model fundamentals are based on established physical laws, such as conservation of mass, energy and momentum, along with a wealth of observations", second, "the ability of models to simulate important aspects of the current climate", and third, "the ability of models to reproduce features of past climates and climate changes" (IPCC 2007: 600–601).
10. The same pattern of argumentation can also be observed for atmospheric pollution modelling (Heymann 2006).
11. The Charney report's conclusion that "the study group finds no reason to doubt that climate changes will result and no reason to believe that these changes will be negligible" (Charney et al. 1979: xiii) provides a typical example.

REFERENCES

Baker, W.O. et al. 1977. "Computers and Research." Science 195, 1134–39.

Charney, J.G. et al. 1979. Carbon Dioxide and Climate: A Scientific Assessment. Washington, DC: National Academy of Sciences.

Dahan, D.A. 2001. "History and Epistemology of Models: Meteorology as a Case Study (1946–1963)." Archive for the History of the Exact Sciences 55, 395–422.

Dahan, A.D. & D. Aubin 2002. "Writing the History of Dynamical Systems and Chaos: Longue Durée and Revolution, Disciplines and Culture." Historia Mathematica 29, 273–339.

Edwards, N. 2010. A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming. Cambridge, MA: MIT Press.

Fox Keller, E. 2003. "Models, Simulation, and 'Computer Experiments'", in H. Radder, ed. The Philosophy of Scientific Experimentation. Pittsburgh: University of Pittsburgh Press (p.198–235).

Galison P. 1996. "Computer Simulations and the Trading Zone", in P. Galison & D. Stump, eds. The Disunity of Science: Boundaries, Context and Power. Stanford: Stanford University Press (p.119–57).

Gates, W.L. 1975. Numerical Modelling of Climate Change: A Review of Problems and Prospects. WMO Report 421. Proceedings of the WMO/IAMAP Symposium on Long-Term Climatic Fluctuations, Norwich, 18–23 August. Geneva: WMO (p.343–54).

Gooday, G.J.N. 2004. *The Morals of Measurement: Accuracy, Irony, and Trust in Late Victorian Electrical Practice*. New York: Cambridge University Press.

Gramelsberger, G. 2010. Computerexperimente: Zum Wandel der Wissenschaft im Zeitalter des Computers. Bielefeld: Transcript.

Guillemot, H. 2010. "Connections between Simulations and Observation in Climate Computer Modelling: Scientists' Practices and 'Bottom-Up Epistemology' Lessons", in M. Heymann and H. Kragh, eds. Modelling and Simulation in Atmospheric and Climate Sciences, special issue, Studies in History and Philosophy of Modern Physics 41, 242–52.

Hansen, J.E. et al. 1978. "Mount Agung Eruption Provides Test of a Global Climatic Perturbation." Science 199, 1065–68.
Hansen, J.E. et al. 1981. "Climate Impact of Increasing Atmospheric Carbon Dioxide." Science 213, 957–66.
Heymann, M. 2006. "Modelling Reality: Practice, Knowledge, and Uncertainty in Atmospheric Transport Simulation." Historical Studies of the Physical and Biological Sciences 37, 49–85.
Heymann, M. 2009. Klimakonstruktionen: Von der klassischen Klimatologie zur Klimaforschung. NTM." Journal of the History of Science, Technology and Medicine 17, 171–97.
Heymann, M. 2010a. "The Evolution of Climate Ideas and Knowledge." Wiley Interdisciplinary Reviews Climate Change 1, 581–97. http://onlinelibrary.wiley.com/doi/10.1002/wcc.61/full.
Heymann, M. 2010b. "Lumping, Testing, Tuning: The Invention of an Artificial Chemistry in Atmospheric Transport Modelling", in M. Heymann and H. Kragh, eds. Modelling and Simulation in Atmospheric and Climate Sciences, special issue, Studies in History and Philosophy of Modern Physics 41, 218–32.
Heymann, M. & H. Kragh, eds. 2010. Modelling and Simulation in Atmospheric and Climate Sciences. Special issue. Studies in History and Philosophy of Modern Physics 41, 193–292.
Humphreys, P. 2004. Extending Ourselves: Computational Sciences, Empiricism, and Scientific Method. Oxford: Oxford University Press.
IPCC 1995. Climate Change 1995, Second Assessment Report, Working Group 1: The Science of Climate Change. Cambridge: Cambridge University Press.
IPCC 2007. Climate Change 2007, Fourth Assessment Report, Working Group I: The Physical Science Basis. Cambridge: Cambridge University Press.
Kaufmann, W.J. & L.L. Smarr 1993. Supercomputing and the Transformation of Science. New York: Scientific American Library.
Kellogg, W.W. 1977. Effects of Human Activities on Global Climate. WMO Technical Note 156. Geneva: WMO.
Kellogg, W.W. 1987. "Mankind's Impact on Climate: The Evolution of an Awareness." Climatic Change 10, 113–36.
Kellogg, W.W., J.A. Coakley & G.W. Grams 1975. Effect of Anthropogenic Aerosols on the Global Climate. WMO Report 421. Proceedings of the WMO/IAMAP Symposium on Long-Term Climatic Fluctuations, Norwich. Geneva: WMO (p.323–30).
Kellogg, W.W. & S.H. Schneider 1974. "Climate Stabilization: For Better or For Worse?" Science 186, 1163–72.
Kellogg, W.W. & R. Schware 1982. "Society, Science, and Climate Change." Foreign Affairs 60, 1076–1109.
Lahsen, M. 2005. "Seductive Simulations? Uncertainty Distribution around Climate Models." Social Studies of Science 35, 895–922.
Lenhard, J. & E. Winsberg 2010. "Holism, Entrenchment, and the Future of Climate Model Pluralism", in M. Heymann and H. Kragh, eds. Modelling and Simulation in Atmospheric and Climate Sciences, special issue, Studies in History and Philosophy of Modern Physics 41, 253–62.
Lewis, J.M. 1998. "Clarifying the Dynamics of the General Circulation: Phillips's 1956 experiment." Bulletin of the American Meteorological Society 79, 39–60.
Lorenz, E. 1970. "Climatic Change as a Mathematical Problem." Journal of Applied Meteorology 9, 325–29.
MacKenzie, D.A. 2001. *Mechanizing Proof: Computing, Risk and Trust*. Cambridge, MA: MIT Press.

Manabe, S. & R.F. Strickler 1964. "Thermal Equilibrium of the Atmosphere with a Convective Adjustment." Journal of the Atmospheric Sciences 21 (4), 361–85.

Manabe, S. & R.T. Wetherald 1967. "Thermal Equilibrium of the Atmosphere with a Given Distribution of Relative Humidity." Journal of the Atmospheric Sciences 24, 241–59.

Manabe, S. & R.T. Wetherald 1975. "The Effects of Doubling CO_2 Concentration on the Climate of a General Circulation Model." Journal of the Atmospheric Sciences 32, 3–15.

Oreskes, N., K. Shrader-Frechette & K. Belitz 1994. "Verification, Validation and Confirmation of Numerical Models in the Earth Sciences." Science 263, 641–46.

Parker, W. 2010. "Predicting Weather and Climate: Uncertainty, Ensembles and Probability", in M. Heymann & H. Kragh, eds. Modelling and Simulation in Atmospheric and Climate Sciences, special issue, Studies in History and Philosophy of Modern Physics 41, 263–72.

Petersen, A.C. 2006. Simulating Nature: A Philosophical Study of Computer-Simulation Uncertainties and Their Role in Climate Science and Policy Advice. Amsterdam: Het Spinhuis.

Peterson, T.C. et al. 2008. "The Myth of the 1970s Global Cooling Scientific Consensus." Bulletin of the American Meteorological Society 89, 1325–37.

Phillips, N. 1956. The General Circulation of the Atmosphere: A Numerical Experiment." Quarterly Journal Royal Meteorological Society 82, 123–64.

Porter T.M. 1995. Trust in Numbers: The Pursuit of Objectivity in Science and Public Life. Princeton, NJ: Princeton University Press.

PSAC [The President's Science Advisory Committee] 1965. Restoring the Quality of Our Environment. Report of the Environmental Pollution Panel. Washington, DC: The White House.

Rasool, S.I., & S.H. Schneider 1971. "Atmospheric Carbon Dioxide and Aerosols: Effects of Large Increases on Global Climate." Science 173, 138–41.

SCEP (Study of Critical Environmental Problems). 1970. Man's Impact on the Global Environment: Assessment and Recommendation for Action. Cambridge, MA: MIT Press.

Schneider, S.H. 1975. "On the Carbon Dioxide Climate Confusion." Journal of Atmospheric Science 32, 2060–66.

Schneider, S.H. 1976. "The Genesis Strategy: Climate and Global Survival." New York: Plenum Press.

Schneider, S.H. et al. 1978. "Cloudiness as a Climatic Feedback Mechanism: Effects on Cloud Amounts of Prescribed Global and Regional Surface Temperature Changes in the NCAR GCM." Journal of the Atmospheric Sciences 35, 2207–21.

Schneider, S.H. & T. Flannery 2009. Science as a Contact Sport: Inside the Battle to Save the Earth's Climate. National Geographic.

Schweber, S., & M. Wächter 2000. "Complex Systems, Modelling and Simulation." Studies in the History and Philosophy of Modern Physics 31, 583–609.

Shapin, S. 1994. A Social History of Truth: Civility and Science in Seventeenth-Century England. Chicago: University of Chicago Press.

Wang, W.C. et al. 1976. "Greenhouse Effects Due to Man-Made Perturbations of Trace Gases." Science 194, 685–90.

Weart, S. 2000. Interview with James Hansen, Center for History of Physics. College Park: American Institute of Physics. Transcript available at http://www.aip.org/history/ohilist/24309_1.html.

Weart, S. 2010. "The Development of General Circulation Models of Climate", in M. Heymann & H. Kragh, eds. Modelling and Simulation in Atmospheric and

Climate Sciences, special issue, Studies in History and Philosophy of Modern Physics 41, 208–17.
Wilson C.L. & W.H. Matthews 1971. Inadvertent Climate Modification. Report from the conference Study of Man's Impact on Climate (SMIC). Stockholm. Cambridge, MA: MIT Press.
WMO [World Meteorological Organization] 1979. Proceedings of the World Climate Conference: A Conference of Experts on Climate and Mankind, Geneva, 12–23 February 1979. Geneva: WMO.

12 Afterword

Reopening the Book of Nature(s)

Martin Skrydstrup

What happens when one puts a physicist and theoretical climate modeller, geographers of various bends, a team of anthropologists working on climate change in the Arctic, Africa, Asia, Andes and Polynesia, and a historian of climate science in the same room? Not only did they create a sense of conceptual laboratory, they were also being inventive about their means of communication and collaboration. They broke into smaller groups, where cross-cutting issues were defined and then presented back in plenum. At the last day of the workshop the participants visualized their thinking about climate modelling on posters, which were exhibited in plenum. The posters mapped different issues of climate modelling spanning from resolution, risk, authority, trust, scales, migrations, and public spectacle. These three days of workshopping for us at *Waterworlds* turned out to be a compelling experiment, carried by an ethos of mutual respect and recognition, which emerged as a model for a new transactional paradigm. This was not multi-disciplinarity, in the sense of scholars speaking from each of their discipline and corner of expertise; for us at *Waterworlds* this watershed event was indicative and exemplary of what Marilyn Strathern has defined as "generic interdisciplinarity" (Strathern 2005; Strathern 2006; see also Hastrup, Chapter 1, this volume).

We maintain "*inter*disciplinarity", not "*multi*-disciplinary", because with *inter* we actually have epistemic trading zones. The event generated spontaneous synergy and genuine innovation out of boundary crossing in the name of knowledge. If creativity is the ability to combine elements and insights from many sources, then these days in January 2011 indeed fostered creativity. Genuine interdisciplinary encounters turn new pages. They map new terrains, where uncertainty and confusion is the order of the day. The workshop was remarkable in the sense that it turned these obvious uncertainties coupled with climate modelling into productive spaces. The participants encountered different disciplinary modes and models of reflecting on and reasoning about uncertainty. This exemplary model of interdisciplinarity proliferated, informed, and enriched each of the chapters, but also more fundamentally created a whole which adds up to more than the sum of the single contributions. In this whole, the careful reader will not only detect original crossings between the boundaries of the natural sciences on the one hand and the social sciences on the other, but also much more subtle relations between the chapters addressing the revolving leitmotif of permutations and punctuations between society and nature. In this afterword, I shall rather awkwardly try and identify some of these common threads and nodes between the chapters and how they relate to the overall unity of the volume.

In her piece on generic interdisciplinarity, Strathern reminds us that contemporary notions of trans-disciplinarity are largely predicated on 20^{th}-century notions of disciplinary boundaries in science. One who practiced interdisciplinarity before it was invented was Charles Sanders Peirce (1839–1914). Trained in chemistry at Harvard, part-time employed in the U.S. Coast Survey, and known to possess one of the most extensive collections of medieval philosophy in North America, Peirce practiced interdisciplinarity almost in splendid isolation. He argued that Galilee did not read in the Book of Nature by way of throwing stones from the leaning tower in Pisa, but rather by way of thought experiments. This brought Peirce to argue that the laws of nature could not be gleaned from the facts of experiments, but rather was inextricably caught up with what he called "inveterate habits": "The one intelligible theory of the universe is that of objective idealism, that matter is effete mind, inveterate habits becoming physical laws" (Peirce 1891). Peirce generalized through abduction, which is a form of reasoning, which contrary to deduction has no grounds in formal logic and contrary to induction has no bearing on probability. Deduction does not add anything new to a line of reasoning, whereas induction and abduction do. Abduction is a form of informed a priori conjecture, i.e. before experience. To make a right guess is dependent upon the fact that humans are products of learning. This was what Peirce had in mind when he described the laws of nature as "inveterate habits"—it is something we have guessed right, or rather gotten right, not because of practical experiments, but because we have learned to perceive the world in a way which enables us to guess right.

This excursion on Peirce as a paradigmatic interdisciplinarian sits well with Kirsten Hastrup's using his notion of "diagrammatic reasoning" as a vehicle for understanding the modes of reasoning by Arctic hunters (Chapter 5, this volume). It also opens the possibility of thinking through indigenous modelling and scientific modelling as a constitutive continuum, based on the premise that neither Arctic hunters nor the climate scientists can separate themselves completely from their modelling of weatherworlds, i.e. their anticipation of nature. If we turn from the Arctic hunters around Thule to Martin Skrydstrup's scientific fact hunters on the ice cap some 500 kilometres away (Chapter 9, this volume), this notion of diagrammatic reasoning travels well, because ice modelling at NEEM also works by visual representations and imageries. And just like the Arctic hunters have to navigate a changing icescape, the scientists have to manoeuvre the changing properties of ice in the borehole. Both communities have to stretch their skills to safely secure subsistence and success. Thus, the Peircian concept of "diagrammatic reasoning" seems to facilitate a constitutive continuity between different forms and scales of knowledge-making about nature.

In his contribution, Mike Hulme envisages a grand research program to answer the pertinent question of how climate models acquire authority in the contemporary world, exercising such power over the academy, policy debates, and the human imagination (Chapter 2, this volume). By way of a sophisticated four-fold typology of climate model reliability, he argues that the aura of authority resides in the interactions between scientific practice, cultural performance, and political interests dressing the models up as "trustworthy witnesses". Each one of these tropes represents a strategic perspective on the question of authority and entails considerable explanatory power. We are convinced that any single explanatory trope would not by and of itself suffice, but are we also convinced that Hulme's election of explanatory representatives is exhaustive? The same goes for the networks, which enable climate models to enter, remain, and travel in society, which are designated as "epistemic", "financial", "political", "discursive", and "performative". Significantly, Hulme's explanatory tropes and ontological networks are *external* to the phenomenon they seek to explain and elucidate.

In Frida Hastrup's contribution we encounter a very different perspective, where the phenomenon, which is sought to be illuminated, is *internal* to the analysis (Chapter 3, this volume). She takes us to a paradox of modelling nature, which is equally pertinent in coastal Tamil Nadu, as it is in Palo Alto or Tokyo, where super computers run simulations: models of nature posit a whole (100% in numerical values) which cannot be known, although this is what they aspire to achieve. Here modelling is a form of knowledge, which is firmly placed in the world, because "world and knower are not separate entities, but creatively modelled in a situational co-production of data and theory, observation and perspective, figure and ground", as she suggests. Frida Hastrup's immanent perspective could in fact contribute to answering Hulme's pertinent question about the social authority of climate models, because "coding precision", "statistical accuracy", and "methodological quality" could be seen as forms of elusive wholeness, which are put to work on the magic premise that 100% gives absolute reliability, credibility, and therefore social and epistemic authority.

The question of possible links, relations, and continuities between climate science and indigenous knowledge runs throughout the volume, but is most directly addressed by Hildegard Diemberger (Chapter 6, this volume), Ásdís Jónsdóttir (Chapter 7, this volume) and Cecilie Rubow (Chapter 4, this volume). While Diemberger, on the strength of her Tibetan ethnography, reminds us that we should not fall into the trap of essentializing indigenous notions of environment, which are both mandible and unequally distributed, Jónsdóttir shows how experts in the CoastAdapt programme perceive local knowledge as supplementary, enabling and disenabling different forms of agency. While environmental knowledge among Tibetan pastoralists is a form of moral stewardship encompassing the physical and the social world, local knowledge in CoastAdapt is framed by expert policies. What we learn here is that climate change in specific places enter into relations with local knowledge, be that as continuums with what Tibetan communities experienced in the past, or as frictions in CoastAdapt, where "science and local knowledge are two fundamentally different ways of approaching one given nature" (Chapter 7, this volume). Most significantly, through Diemberger and Jónsdóttir's chapters we gain insight into local correctives to the grand and sweeping narratives of climate change, such as Schellenhuber's famous declaration of Tibet as the "Achilles heel of the planet".

How such global discourses of climate change are received, appropriated, and mixed with other forms of knowledge in turbulent ways is what Rubow shows us in her contribution (Chapter 4, this volume). What is fascinating about her account from the Cook Islands is that she paints a canvas, which is way more complex than the juxtaposition between scientific and local knowledge. What we have in the Cook Islands is a whirl of different knowledge forms concerned about climate change which loop and form new formations; from tourism to theology, from leadership to lore, from metropolitan science to local policy-based science, and back again. "Real" climate scientists go out of their way to stress the non-linkage between contemporary weather phenomena, such as cyclones, and climate change, but Rubow's ethnography of the "mixed social-natural life of cyclones" shows us the fate of such messages in the co-construction of science, society, and the bad weather ranging the Cook Islands.

What Jónsdóttir hints at and Rubow articulates is elaborated and spelled out in Anders Munk's chapter: that nature can either be anticipated or have anticipations of its own (Chapter 8, this volume). If nature has anticipations on its own behalf, this requires a nature emancipated from any sort of determinism, human aspirations/ambitions, or discursive repercussions—an altogether different kind of nature. Through the initiation of the author into flood risk modelling, we learn that in the classroom computer simulation is "realistic" and "could happen". These notions presuppose that nature exists as a bounded domain of its own out there, which can be adequately and accurately modelled in the computer. Munk's contribution shows us the artificiality of this notion of nature, a notion Bruno Latour termed the "bicameral collective" (Latour 2004). Consequently, this line of enquiry opens the vexing question of multiple natures, but also a perhaps more fundamental question: if nature is emancipated, does it come with agency and free will, beyond the determinism of thermodynamics and the contingences of non-linear systems or chaos theory?

Our physicist and theoretical climate modeller Peter Ditlevsen did not raise this question, but in a way his chapter contributed to an answer (Chapter 10, this volume). In his work we gain insight that the idea about a linear progression in climate modelling towards higher degrees of precision and more accuracy, through the inclusion of ever more components of nature, seems obsolete. We are back to Frida Hastrup's paradox of the elusiveness of the whole (100%) in climate modelling. Ditlevsen argues that nature is unpredictable and will probably remain a challenge for climate modellers. Thus, nature seems to call for multiple sorts of naturalisms, perhaps more versions than ever envisioned by the bicameral collective. Finally, Heymann (Chapter 11, this volume) does us the service to implicitly historicize both Munk's and Ditlevsen's computer simulations, and bring us back to Hulme's opening question about the authority of climate models. From Heymann, we learn that computer simulation of climate from its inception is a Cold War phenomenon taking off in the 1950s. By the late 1970s, scientific consensus was established that climate modelling was the only credible way to produce knowledge about long-term climate projections. With Heymann's enlightening pre-history of climate modelling, we have returned to and revisited Hulme's opening question from a new vista—and we have come full circle.

Behind us we have an itinerary of mutually elucidating perspectives on the grand leitmotif of the relationship between society and nature, accentuated and brought to the fore by climate modelling. Every single one of the contributions highlight—more or less explicitly—that we need to reopen Galilee's Book of Nature(s), albeit now read through a prism of multiple natures and multiple forms of naturalism, which these chapters testify to so strongly. Performing such a rereading we may reimagine Hulme's pertinent question from the outset and venture one provisional answer enabled by the generic interdisciplinarity exemplified by this volume: climate modelling holds such efficacy and authority over Western publics, precisely because it is a form of naturalization of Western naturalism.

REFERENCES

Latour, B. 2004. Politics of Nature: How to Bring the Sciences into Democracy. Cambridge, MA: Harvard University Press.

Peirce, C.S. 1891. "The Architecture of Theories." The Monist Metaphysical Series 1 (2), January, 161–76.

Strathern, M. 2005. "Anthropology and Interdisciplinarity." Arts and Humanities in Higher Education 4, 125–35.

Strathern, M. 2006. "A Community of Critics? Thoughts on New Knowledge." Journal of the Royal Anthropological Institute 12, 191–209.

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