Divided socio-natures

Essays on the co-construction of science, society, and the global environment

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PhD-thesis
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Preface and acknowledgements

> Whether or no it be for the general good, life is robbery. […] The robber requires justification (A. N. Whitehead, *Process and Reality*, 1929).

In the course of researching and writing this thesis, and in digging into the work of Bruno Latour, I have encountered some intellectually brilliant but rather misanthropic figures – notably Michel Serres, author of books on human parasitism vis-à-vis an ailing natural environment. Despite sharing Serres' interest in ecological crises, however, this thesis is not written on the basis of a cynical attitude. Rather, in the spirit of Whitehead's famous quote, the thesis represents my attempt to justify the time, efforts, gifts and favours extended me over the years by a range of people (and non-humans), inside and outside of academia. My accumulated debt runs high, and I am well aware that some debts are beyond repaying – beyond justification, as it were. Nevertheless, in the spirit of good conventions, let me list a few of the more important ones here.

The one debt most clearly beyond justification is owed to my supervisor, Professor Margareta Bertilsson. Margareta has always shown a remarkable (and often undeserved) faith in my capabilities as a would-be sociologist, even at times when she may not have agreed with my more idiosyncratic choices (I suspect this includes my entire interest in Latour, whom she insists unapprovingly on calling a 'postmodernist'). Such generosity, I believe, is a rare quality in a supervisor. Furthermore, perhaps unbeknownst to herself, she has managed to deeply influence important aspects of my sociology. In the process of writing, I have returned, again and again, to ideas picked up from her long-standing interest in American pragmatist philosophy and social science. While I have not always understood or taken her advice, in retrospect I thus realize just how much my work is indebted to her never failing respect for the craft of sociological inquiry.

At the risk of eliciting some overly romanticized connotations, the making of this PhD thesis has been something of an adventure. One reason for this is that my thesis not only speculates on mobile sociology in a globalized world, but is also itself a manifestation hereof. Part of my empirical work started before the official PhD programme, during a one-and-a-half year stay, from late 2005 to early 2007, at the School of Arts and Sciences, Tohoku University, in the city of Sendai in Northeast Japan. I wish to acknowledge the generous financial support from *Monbukagakusho*, the Japanese Ministry for Education, Sports, Culture, Science and Technology, in making this stay possible.

More than anyone in Japan, I am grateful to my supervisor, Professor Hasegawa Koichi, who patiently introduced this clueless Westerner to the world of Japanese environmental sociology. Amongst his many invaluable favours, Hasegawa-*sensei* introduced me to Mr. Atsushi Ishii, expert on Japanese whaling, without whose insiders' knowledge and generosity in sharing information my own studies might have failed miserably. My special gratitude extends also to Kumiko Tsuchida, a graduate student at Tohoku who for reasons of bi-lingual skills (which she no doubt regretted having admitted to!) became an indispensable help in my attempts to conduct sociological research while only then learning the rudiments of the Japanese language. For fear of missing too many, let me say that I am deeply grateful to everyone at the Tohoku sociology department for making my stay such a memorable experience – *domoo arigatoo gozaimashita*.

In autumn 2008, I spent half a year with the Program on Science, Technology and Society (STS) at Harvard University, in Boston USA – financed by a grant from the Danish Agency of Science, Technology and Innovation, whose support I gratefully acknowledge. This programme is run by Professor Sheila Jasanoff, a founding figure of the trans-disciplinary field of science studies (STS) and a great source of intellectual inspiration in my work. My understanding of science has benefited tremendously from the field of STS, and I am grateful to Professor Jasanoff for her support and her stimulating vision of more democratic science-society relations. The fact that I had such wonderful STS co-visitors at Harvard – who will know by themselves whom I am thinking of – only made the stay more pleasant.

As if this level of mobility was not enough (or already much too much CO_2 in climatic terms), I have visited other parts of the world for shorter periods of time. In February of 2008, I had the opportunity to participate as student observer in the Delhi Sustainable Development Summit (DSDS), held bi-annually in this Indian mega-city. As a sociologist working on climate change, getting a chance to listen to heads of states, high-ranking UN officials, world-renowned scientists, and large-scale transnational NGOs is invaluable. I wish to acknowledge the financial and moral support of TERI, the Delhi-based organizing university. Before going to Delhi, I had settled on global carbon markets as my second empirical case study, meaning that I was able to make the most of my Indian visit in terms of empirical research.

In June of 2009, I was generously invited back to Japan, this time by Mr. Morita Atsuro, assistant professor of Anthropology at Osaka University and engaged participant in Japanese STS research. In my understanding, this was one of those unlikely encounters that only chaos theory might predict: while we had talked briefly during an international conference in 2008, it was really only *after* I arrived in Osaka that we discovered how much we share in terms of science studies interests and theoretical assumptions – unlikely as this is, being situated on opposite sides of the globe. I consider it a sign of benign globalization, and I am very grateful to Morita-*san* for showing me the worlds of Kansai STS and Japanese anthropology of nature(s) – good luck on your 'silent revolution'!

Ending this list of mobility-related debts, I wish to thank Professor Kirsten Hastrup, and her team of climate anthropologists at the Copenhagen 'Waterworlds' research centre, for bringing me along on the *Climate Change and Scales of Sustainability* PhD school in Greenland in September of 2009. This was a great learning experience: to be able to encounter this amazing (if not socially unproblematic) region of the world, in the company of experts from a variety of fields – from Arctic anthropology to archaeology – gives 'trans-disciplinarity' a new dimension. At first, I was wondering what a sociologist without any credentials in Arctic research was doing there; but it turned out that thinking ('Latourian style') about polar bears as social actors made for a strong connection. Unfortunately, I did not have time to include the polar bears in this thesis; but I want to thank all participants in the summer school for this valuable lesson in trans-disciplinary dialogue.

To get down to ground again, I also want to extend my acknowledgements to colleagues and friends in the Copenhagen region. First of all, I am grateful to the Department of Sociology, Copenhagen University, for enabling this PhD thesis to emerge, financially and institutionally. Any sociologist of science will acknowledge that good colleagues are essential for the conduct of research. In particular, having PhD-level colleagues with a keen sense of the ironies and pleasures of life in academia is more important than we students tend to realize; warm-felt thanks to all of you. Also warm-felt thanks to you special PhD people in what started out as the 'Social Things' reading group, and which has by now

become the ideal experimental setting for testing our collective limitations in dreaming up new social science imaginations (again, you know who you are).

As may be clear, the world of science studies (STS) has gradually become my second (post)-disciplinary home in writing this sociological thesis. The same proviso applies here: without good colleagues scattered around institutionally in the Copenhagen area – arguably the new European centre of STS research! – I would not have had the encouragement to engage these interesting debates. Without singling anyone out, let me say that if you have been around DASTS, the Danish Association of STS, over the past few years, we may very well have talked and (ex-)changed perspectives – in that case, thank you. One person in particular must be singled out: thanks, Torben (Elgaard Jensen), for sticking up with me long enough for us to get the Latour book finished to good effect. On my part, quite a bit of what we learned is reflected in this thesis.

Time to end the justifications. Or rather: from now on, all (failed) justifications fall back on myself – in the shape of mistakes, myths and half-truths, all monsters bound to accumulate when you go plundering in the actor-networks of Bruno Latour, STS, and beyond. When reading this thesis and encountering such monsters, the reader may want to keep in mind that – while social science demands justification just as strictly as other forms of 'robbery' – Whitehead may still be right in suggesting that a creative thief can at times go far, in sociology no less than in life more generally.

Summary chapter:

Towards a mobile sociology of the global environment

> Partiality only works as a connection: a part by itself is a whole (Marilyn Strathern, *Partial Connections*, 2004:xxix).

> You can add up the parts, you won't have the sum. […] There is a crack in everything, that's how the light gets in (Leonard Cohen, *Anthem*, 1992).

Global environmental conflict: the whole is another part

This thesis deals, in a variety of ways, with one overarching analytical question: *how to analyze, from a sociological point of view, the importance of scientific knowledge in contemporary disputes on the fate of threatened nature in 'world risk society'?* Immediately, this question conjures a number of issues. On the side of society, it refers to what is by now a taken-for-granted part of Euro-American public knowledge: the world is facing a number of serious global environmental threats, where scientists play pivotal roles in defining the parameters of public and political action. Climate change is simply the latest addition to a long list of potential natural calamities. On the side of sociology, reference is made to what is arguably the most important contribution so far to understanding this global predicament: Ulrich Beck's well-known *Zeitdiagnosis* of the world risk society (1992; 1999). More than anyone, Beck has been responsible for bringing global environmental concern into the mainstream of sociology, giving rise to important research agendas (Jensen & Blok 2008). This thesis is indebted to his groundbreaking sociological work.

The risk society part of the question, however, is put in inverted commas, in order to prefigure what is perhaps the main analytical narrative of this thesis. In brief, it runs as follows: for sociology to become seriously engaged with contemporary global environmental crises, we need to explore a post-risk society theoretical agenda, inspired not least by developments in so-called 'science and technology studies' (STS). In particular, we need to embrace what has become known as ‘actor-network theory’ (ANT), including the work of its primary spokesperson, French sociologist of science Bruno Latour.[1] This is not a matter of analytical reversals, since already, fruitful and mutually respectful dialogues are taking place between Beck and Latour (Latour 2003, 2004c; Beck 2004). The work of bridge building undertaken in this thesis, then, by no means start from scratch. Nevertheless, foregrounding Latour and backgrounding Beck – as does this thesis – will have important consequences for sociology. Exploring these consequences has been the main trajectory followed in researching and writing the thesis; and this summary chapter is an attempt to outline some lessons learned.

In a certain sense, then, this dissertation has a well circumscribed aim, placing it within the borderlands of two subfields of inquiry, known as environmental sociology and sociology of science, respectively. The guiding idea is to test the conditions under which the discipline of sociology can be made relevant to an understanding of the complex and conflictual ecological issues proliferating in current-day societies. To some extent, we are thus dealing with familiar territory, epitomized in the idea – expressed by a variety of important sociologists – that ecological issues constitute a hallmark of our present societal state of late-, reflexive-, post-, or a-modernity (Beck 1992; Giddens 1990; Luhmann 1989; Latour 1993). At the same time, however, the implicit claim is that sociology is still not yet fully relevant to an in-depth understanding of this situation. It is important to note from the outset that this thesis does not conceive itself as puzzle solving in ‘normal’ sociology (cf. Kuhn 1962); instead, it attempts to co-articulate, with Latour and others, a new mode of sociological inquiry, adequate to global ecological conflicts.

At the same time, these starting premises imply that the thesis deals with what is arguably one of the major expressions of the ‘totality’ of our current-day social condition: the idea that global environmental problems may be threatening all life on the Planet in a not-too-distant future. Ecological concern, in short, is densely woven into textures of globalization narratives, universal

1. Actor-network theory (ANT), it should be noted from the outset, represents a collective theoretical endeavour: besides Latour, ANT is customarily attributed to the work of his long-time co-worker Michel Callon, together with British sociologist John Law. The work of all three authors will be discussed extensively in this thesis, and some of their internal theoretical differences highlighted. In this summary chapter, however, the term ‘ANT’ will often work as shorthand for the collective enterprise.

claims, and grand ideas of planetary holism (Yearley 1996). As a modern science, historically tied to the formation of nation-states, this situation entails ambiguities for sociology, in attempting to analyze the trans-boundary flows of contemporary social life beyond societies (Urry 2000; Beck 2006). This thesis engages the ambiguities of 'local' and 'global', by turning globalized scientific knowledge practices into an empirical object of sociological study, requiring new mobile methods of inquiry as well as careful attention to the particularities of universalizing discourses.

In one of his routine provocations to fellow sociologists, Bruno Latour is alleged to have once commented to the effect that "there is no environmental sociology before you do sociology of science".[2] The significance of this statement – clearly chauvinistic in part – could hardly be overestimated. Under modern conditions, natural science is *the* authoritative source of knowledge when it comes to interpreting the signs of nature (Yearley 2008). This is nowhere more obvious than in widespread everyday experiences of controversy surrounding expertise and counter-expertise in the arenas of public concern with environmental risks. *Who gets to speak for the environment, and with what degree of public-political credibility?* These questions carry society-wide importance; and since the answer is often 'scientists', Latour's point provides impetus for this thesis: *how to do environmental sociology in ways that take scientific knowledges seriously?* My claim is that ANT provides a valuable starting point, because it puts relations between nature and culture, science and politics, at the centre of sociological attention (Schinkel 2007).

It would be misleading, however, to convey the impression that what lies ahead is simply about 'applying' Latourian ANT thinking to empirical issues – for two reasons. First, as will become clear, this is simply not how ANT works as a sociological theory (indeed, it is not how any theory works, according to ANT). Instead, the methodological slogan of ANT is always to 'follow the actors'; which means that sociology needs to take empiricism seriously! This thesis is based on two empirical case studies into conflicting relations between science, NGOs, and public action in the environmental domain: one on long-standing controversies surrounding Japanese whaling, and one on controversies

2. This is a quote from memory of an oral presentation by a French sociologist, citing a personal conversation with Latour. 'All translation is transformation', and there is no way for me to confirm the exact quote.

surrounding global carbon markets. The first case occupies a small corner of the wider assemblage of biodiversity concern; the second a small concern of climate change concern. In many ways, the two cases are incommensurable. What tie them together are similarities in analytical questioning, helping to bring out contrasts across two empirical domains. This is an important methodological point, which will be expanded later on.

The second reason this thesis is not simply about ‘applying’ ANT to empirical issues is that, in the process, ambiguities and shortcomings of Latourian ‘sociology of associations’ (2005) will emerge, requiring some form of theoretical reconstruction. Indeed, the entire thesis can be read as performing such reconstructions, minor and major, depending on the analytical issues at hand. Reconstruction here should not be equated with critique, especially not as this is understood in a deconstructive vein; instead, it implies practical work of conceptual refinement, undertaken with some specific analytical goal in view. In this summary chapter, I articulate certain reservations towards Latourian thinking at the level of ‘general’ sociological methodology, by juxtaposing his standpoints with those of established sociological traditions. My sympathetic criticisms are by no means meant to dismantle ANT as a promising sociological programme. Rather, in ‘Latourifying sociology’, I attempt to simultaneously ‘sociologize Latour’.

Before proceeding, I want to introduce a meta-analytical trope running through and co-organizing much of this summary chapter: the trope of non-holism, or more precisely, the conviction that ‘the whole is just another part’. Sociologist John Law, part of the ANT family, refers to this as an attitude of ‘modest sociology’ (1994); and in a similar vein, Donna Haraway, leading protagonist of feminist STS, speaks of ‘situated knowledges’ (1988). Still, social anthropologist Marilyn Strathern is closer to what I mean by non-holism when she coins the expression ‘partial connections’ (2004): in a world of complexity, analytical connections are always partial, never forming a new totality.[3] My meta-analytical attempt in this introduction is to respect this important insight.

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3. By complexity, I do not refer to the clichéd claim that ‘the social world is too complex to understand in its entirety’. While this is true, following Latour (1996b), a state of affairs should be seen as ‘complex’ only if, at any one juncture in a trajectory, two (or more) forks

Partial connections are manifested at all levels of this thesis, in attempts to link concerns often expressed in different sociological contexts. Empirically, for instance, biological connections between whales and climate change are esoteric at best; however, from a sociological point of view, their juxtaposition may still yield new insights. Similarly, it is not of paramount importance to my project to establish that we live in a *risk* society – that is, to imply this diagnosis as somehow privileged in summing-up all of contemporary life. No doubt, in different respects, we simultaneously live in risk, knowledge, reflexive, network, globalizing, liquid, and neo-liberalizing societies.[4] What matters sociologically is that global environmental risks matter importantly to a vast array of contemporary social actors, human and non-human (to jump ahead). For this reason alone, they warrant closer sociological scrutiny, empirically and theoretically.

The remainder of this summary chapter will be structured as follows. In the next section, I offer a brief overview of the six sociological essays making up the majority of this thesis, suggesting ways in which to think of their (partial) connections. Then, by way of reflecting on the meta-analytical underpinnings of the thesis, I attempt to position my substantial concerns in a dense picture of the relevant contexts of contemporary sociology. Picking up threads from this picture, I then turn – in four successive sections – to an exposition of the main meta-theoretical, methodical, empirical, and normative claims advanced throughout the thesis. This is also where elements of self-criticism will enter the text, as I discuss the strengths and weaknesses of commitments embedded in my approach. I end this introduction by summarizing key themes to keep in mind while reading the essays – which remain, after all, the main body of the thesis.

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will lead into different emergent patterns. This notion of complexity is tied to the complexity sciences, and relates to fractal distinctions (see Abbott 2001).

4. I am glossing here a number of well-known sociological *Zeitdiagnosen*, associated with the names of, for instance, Ulrich Beck, Nico Stehr, Manuel Castells, Zygmunt Bauman and Nicolas Rose.

Setting the scene: a preview of the articles and their trajectories

Under certain descriptions, it is not too far-fetched to liken the sociological researcher to a private detective: just like the latter, the sociologist is in the business of assembling clues spread by actors in the social territory, gradually stitching them into a coherent narrative (see Bertilsson 2004). Indeed, the analogy with private detectives is especially appropriate when working sociologically with Bruno Latour, an ethnographer who has after all made a point of retelling the story of a failed techno-scientific public transport project in Paris in the idiom of a crime novel (Latour 1996a; see Austrin & Farnsworth 2005). While the detective analogy may thus prove valuable at the level of method, however, it is not obvious that crime novels should form the stylistic blueprint for a sociological PhD thesis. Indeed, experimentation of such an order might prove risky, given what we know to be a good rule of thumb in social science: a little bit of heuristics (like analogy) goes a long way – too much is likely to simply take you off the chart (see Abbott 2004).

For the purposes of this summary chapter, I have chosen to reverse the narrative order of the crime novel: right away, I will set forth, in broad outline, some of the major themes and conclusions running through each of the six essays forming the body of this thesis. Simultaneously, I am going to suggest a compact analytical trajectory – or perhaps rather a set of analytical trajectories – which the reader may find when reading across the essays, including some clues contained in the order in which the essays have been assembled. For the moment, I make no claim to totality: the articles have neither been researched nor written in order to make up a seamless whole, empirically or theoretically. Rather, each of the essays represents their own particular take at some aspect of the overall analytical inquiry, in terms of elucidating sociologically the embeddedness of scientific knowledges in conflicts over global environmental risks. In this sense, to rephrase the quote from Strathern, each article taken alone is its own totality, its own argument. It is only when reading across the essays and making connections between them that certain trajectories, however partial, stand out.[5]

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5. The reader may have noted that I refer interchangeably to 'articles' and 'essays' in reference to the texts. While I am not strongly committed to either term, I will attempt to speak of articles when referring to each text individually (to signal their completeness),

In some sense, given the formal characteristics of this type of article-based PhD thesis, what was just said is entirely banal: for each article to be published in an international peer-reviewed journal (as these articles either are or will be[6]), they simply need to form self-enclosed argumentative entities. What is perhaps less banal is the claim that, when stitched together one after the other in this thesis, they will still not easily form a new totality – beyond the formal requirement of being a whole thesis. Again, this need not come as a surprise. Indeed, it has become a cliché of poststructuralist methodology to claim that all we can do is to construct 'perspectives' on an object, the implication being, strictly speaking, that analytical objects are endlessly malleable, held together only by sticking to one epistemological frame.[7] Presumably, readers may think that the six essays do not form any new totality, for the reason that they represent different epistemological perspectives. This, however, is emphatically *not* what I intend to say in this thesis.

To reiterate the trope of partiality, the six essays presented in this thesis do not form an easily coherent whole since – although strongly connected in numerous ways – their connections do not exhaust what could potentially be said about their own contexts of sociological knowledge. More details could always be added, as could indeed more contexts (see Strathern 2002). To be clear, this notion of partiality, or situated-ness (Haraway 1988), on the part of knowledge is *not* to be interpreted as a failure of analytical rigour. Rather, it is simply a precondition for all knowledge making, sociological and otherwise. Symmetrically, it is not to be interpreted in the postmodernist idiom of deconstruction, as if we were somehow trapped inside arbitrary language-games, stuck within our own epistemological perspectives. Connectedness goes all the way, and of course sociologists can get in touch with social realities! Or, as Bruno Latour puts this point (2000a): the great thing about a standpoint

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whereas I will speak of essays when referring to them in the context of each other (to signal their interrelations and partiality).

6. Or so I hope! At the time of writing (January 2010), two articles have been published, and two are accepted for publication. The remaining two is still under peer review. In going through the list of articles, footnotes will indicate their respective statuses.

7. Neither is this a new idea: epistemological perspectivism traces to the philosophy of Nietzsche, strongly mediated in sociology by the work of Foucault. In Danish sociology, it has recently become something of a default methodology, summed up in the catchall term 'analytical strategy' (Åkerstrøm Andersen 1999).

(‘perspective’) is that you can always change it if you want. Throughout these six essays, I take the liberty of changing standpoints a few times.[8]

To provisionally speak the language of theory and data, and to start by stating the obvious: across the essays, references will be made to two sets of data, each assembled in the shape of ethnographic case studies. The six essays have been ordered in such a way as to represent this clear discontinuity at the level of empirical social reality: whereas the first four essays build, in different ways, on data relating to Japanese whaling controversies, the following two essays switch empirical context, building on data related to carbon markets and climate change. As already noted, this empirical discontinuity is mediated by analytical continuities: not only at the level of questions posed (scientific knowledge in global environmental controversies), but also at the level of theoretical language employed (primarily ANT). It is also mediated, as I will argue shortly, by a particular understanding of the way in which sociological theory and data relates to a wider societal settlement, and hence the ways in which sociological inquiry is socially ‘performative’ (Law & Urry 2004). Setting forth this settlement will help articulate connections between my own empirical studies and wider concerns of contemporary sociology.

Now, a similar kind of trajectory through the essays as the one just outlined – referring to the level of empirical data – could be told at other levels as well, not least the level of ‘sociological theory’. This trajectory, however, is more complicated, and this entire summary chapter is a sustained attempt to articulate it at the level of ‘general’ sociology. For this reason, I believe it will be beneficial to start this work of theoretical reconstruction not with the connections, but rather with the parts themselves, by outlining the main analytical questions and theoretical answers pursued in each of the different articles of this thesis. As will become clear, there are strong theoretical connections across these individual arguments; but there are also theoretical variations on similar topics as well as theoretical ‘progressions’, in the trajectory thus drawn up. Later on in this summary chapter, I will step further back and attempt a contextualization of the theoretical choices that is presented here simply as ‘the right fit’.

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8. I elaborate on ‘standpoints’ and the implied sociological territory in the next section of this introduction.

- Article 1: *Contesting global norms: politics of identity in Japanese pro-whaling counter-mobilization*[9]

This article picks up in the middle of my first empirical case study: controversies surrounding Japanese so-called 'scientific' whaling. In outlining the historical legacies shaping what has arguably been *the* definitive 'global environmental conflict' since the 1970s, the article attempts to show how and why Japanese scientific and political elites continue to defend their whaling practices, against an (almost) international norm of whale protectionism. This, it should be noted, is a question of considerable interest to social scientists and whale politics practitioners alike. My answer, in brief, relies on symbolic interactionist theories of social movements, traceable to the work of Goffman, in arguing that we need to understand the construction of a morally imbued pro-whaling identity. This identity mixes 'master-frames' of scientific rationality and cultural authenticity in countering the claims of transnational ('Western') environmental movements of anti-whalers, symbolized by Greenpeace. In brief, staying close to empirical data from Japan, the article presents a sociologically informed interpretation of the case, articulated at the level of collective identities. Science, in this conflictual setting, is shown to work discursively towards ends that are as much 'social' and 'political' as they are 'scientific' – unsurprisingly, we might add, from the standpoint of contemporary sociology of science.

- Article 2: *Actor-networking ceta-sociality; or, what is sociological about contemporary whales?*[10]

This article undertakes a rather different analytical task, by stepping back from the empirical details of on-going whaling controversy. The central theoretical question pursued by this article is: how do non-human entities become social actors; and, to use the whale case, how do they become actors of global social importance? This question may sound strange to some sociological ears, but to

9. Article published in the journal *Global Environmental Politics* 8(2), 2008.
10. Article published in *Distinktion: Scandinavian Journal of Social Theory* no. 15, 2007.

readers familiar with basic tenets of ANT, it should be recognizably speaking to core concerns of this corpus of theorizing. Building on the ANT-inspired notion that non-human entities *are* indeed social actors, I make the case for singling out non-human animals as particularly 'agential' social entities, drawing on a growing sociological, and interdisciplinary social science, interest in human-animal relations. In semi-empirical fashion, I illustrate these theoretical claims via selected fields of contemporary human-whale interaction in Euro-American societies: whale watch tourism, Greenpeace activism, and 'ethnographic' whale biology. Via these and other material-semiotic human-whale encounters, I argue, whales have been elevated to significant social recognition in this part of the world – helping us understand the flip-side, so to speak, of controversies over Japanese 'scientific' whaling.

- Article 3: *Mapping the Super-Whale: towards a mobile ethnography of situated globalities*[11]

In this article, I once again resume the task of mapping Japanese whaling controversies, but this time from a distinctly methodological vantage point. The question pursued in this article is simple, but far-reaching: to what extent is it possible, for a sociology conceived in mobile terms, to undertake ethnography at the global scale of analysis? This question is already hotly debated in the borderlands between sociology and anthropology, flying under banners such as 'multi-sited ethnography' (Marcus 1995). Drawing on my own experiences in researching – from an ethnographic positioning within Japan – the global-scale connections making up this environmental controversy, I attempt to articulate an ANT-inspired alternative to existing methodologies, one that I dub 'ethno-socio-cartography'. I situate this discussion in the context of calls for more 'mobile methods', frequently heard in contemporary sociology, but associated most vividly with the seminal work of British sociologist John Urry (2000; 2007).

11. Article accepted for publication in the journal *Mobilities* (expected publication Spring 2011).

- Article 4: *War of the whales: post-sovereign science and agonistic cosmopolitics in Japanese-global whaling assemblages*[12]

This article represents a final look (on my part) at the politics of the Japanese whaling controversy, in a sense looping back to problems left unanswered by the first article. If we (now) accept the important ANT insight that whales are not simply 'social constructions', articulated within 'discursive frames', but should rather be seen as material-semiotic social actors in their own right – what are then the analytical consequences in terms of understanding science-politics relations in this case, and, by extension, in other cases of global environmental conflict? This is a question increasingly preoccupying not only Bruno Latour (2004b), but also the entire field of science studies (STS); and in this article, I apply seminal insights from these debates, summed up in the notion of 'cosmopolitics' (Latour 2004c), to the case of whales. The possible sociological implications are too numerous to sum up here; suffice to say that Latour's cosmopolitics substantially reshuffles established 'modernist' relations amongst science, society and nature, in ways highly suggestive of future sociological and societal agendas.

- Article 5: *Clash of the eco-sciences: carbon marketization, environmental NGOs, and performativity as politics*[13]

With this article, we leave whales behind and enter the second empirical field: the on-going construction of global-scale markets in greenhouse gas emissions, currently a major issue in international climate change politics (itself, of course, a major issue in international politics). In parallel to the first whaling article, this article attempts to outline some basic sociological parameters pertaining to science-society relations as played out in the construction and contestation of these so-called carbon markets. Once again building on central ANT tenets – this time in the shape of Latour's co-worker, Michel Callon, and his 'performativity of economics' thesis (Callon 1998) – the article represents an

12. Article accepted for publication in *Science, Technology, & Human Values* (expected publication Spring 2010).
13. Article submitted for review in the journal *Economy and Society* (January 2010)

attempt to reconstruct this thesis, in order to analytically acknowledge the political activities of environmental NGOs in countering the 'market framing' of climate change. These claims are backed up by empirical illustrations, pertaining amongst other to Indian environmentalists. In knowledge political terms, I interpret these clashes in the context of what Latour calls 'the war of the eco-sciences' (2004b) between economics and ecology, a major rift in the history of modern environmentalism.

- Article 6: *Topologies of climate change: actor-network theory, relational-scalar analytics, and carbon market overflows*[14]

This article represents an attempt to dig one theoretical step deeper into the sociological problematic conjured by the phenomenon of climate change. Just as the Japanese whaling controversy forces sociology to take non-human agency to heart, climate change forces sociologists to fundamentally rethink certain socio-spatial assumptions wedded into their standard theorizing. Unsurprisingly (by now), my attempt at a re-theorization of socio-spatial relations of climate change is undertaken by way of ANT, this time not, however, the 'standard' ANT of Latour but rather what has become known as 'post-ANT' (Gad & Bruun Jensen 2009). Put very briefly, the idea here is to think topologically about socio-spatial relations, in what may in part be thought of as an analytical alternative to the narrative of 'globalization'. Climate change, I argue, constitutes an important litmus test of this re-theorization, as illustrated empirically in the article by way of carbon markets.

In the context of this thesis, the reasons underlying the particular selection and ordering of the six articles should now be clear. There is an obvious 'empirical logic': the first four articles deal with one set of empirical references (to Japanese whaling controversies), while the next two deal with a second set of references (to carbon markets and climate change). Second, within each of the two sets of empirically ordered essays, a more theoretical 'logic' is

14. Article under review in the journal *Environment & Planning D: Society and Space* (January 2010)

simultaneously at work – going, one might tentatively suggest, from relatively simpler to relatively more complex analytical-sociological issues. This scaling, however – from the simpler to the more complex – is by no means straightforward (cf. Mol & Law 2002); rather, it requires more meta-analytical justification, which I attempt later in this introduction. Part of this justification, I shall argue, requires us to speak more precisely about 'theory', 'generalization' and 'abstraction'. Again, this exercise will enable me to connect my own concerns to those of sociology more generally, while still maintaining that the thesis does not add up to a theoretical whole.

Having thus worked my way through the major organizing principles underlying the specific knowledge claims of the thesis, I hope to have set the scene in order to turn now to issues of more general relevance to a sociological audience. For the remainder of this introduction, my aim is to contextualize – or, more precisely, to 'self-contextualize' – the meta-theoretical, methodical, empirical, and normative claims advanced throughout the essays. Before taking on this task directly, however, I need to establish in more detail some of the meta-analytical parameters – at the level of the epistemology and ontology of sociology – within which my claims are to be interpreted. This is no easy task, particular since, reflexively, there is a need to take into account certain lessons from science studies (STS), similar in principle to the ones 'applied' in this thesis to my empirical subjects (like whale biologists). Again, my suggestion will be that thinking in terms of partial connections will give us a suitably complex picture of the relevant contexts of sociology.

Topographies of sociology: the many contexts of social science

Historically, it has been a hallmark of the sociology of knowledge to argue that developments at the level of human ideas are closely intertwined with developments at the level of socio-historical context. In order to understand Durkheim's theory of division of labour in society, for instance, we need to understand wider attempts to restore professional corporations at the level of the late-19th century French state (see Boltanski & Thévenot 2006:285ff). Nevertheless, the exact extent to which developments in science – meaning now *natural* sciences – may be *explained* by socio-historical factors has been one of the most interesting and controversial topics in 20th-century sociology (and philosophy). It has given rise to numerous competing theories (Mannheim to Latour and beyond), new sub-disciplines (sociology of science; STS), and even a small intellectual 'war' in the 1990s (known as 'the science wars'; see Bertilsson 2009). It is also centrally placed in meta-distinctions between 'realism' and 'constructivism', each with strong connotations (see Abbott 2001). My attempt here is to provisionally sort out some of these complex issues, for the purpose of clarifying some parameters of this thesis.

Given the complexity of these issues, there is a certain problem of 'recursivity': no matter where we start, it will inevitably feel as if too many things have been left implicit. To paraphrase Strathern (2004:xv), one easily risks creating a sense of disproportion: my attempt to mark out a meta-analytical territory for this thesis will almost inevitably seem insufficient, given the profound nature of insights *already* accumulated on these issues over the years, in sociology, philosophy and elsewhere. There probably is no good remedy for this. Instead, I appeal to the reader to keep in mind two important premises when reading my exposition. First, no claim is made to exhaustiveness; on the contrary, my discussions will be extremely selective. Second, for the moment, judgments on (in)-sufficiency ought not be done at the level of 'general' sociology, let alone 'general' philosophy of science. Instead, it should be kept in mind that my attempt is first and foremost to clarify the meta-analytical commitments of *this* particular thesis. The extent to which such commitments may be generalized is itself a matter for clarification.

It follows from these premises that my exposition eschews a certain model of 'foundationalism': in particular, the notion that certain philosophical genres, like epistemology and ontology, should act as 'foundations' for sociological theory and inquiry. Following Latour (2005), I would argue that the hierarchical model – with philosophy performing as the 'base' of sociology – is not the most interesting or fruitful way of imagining this relation. There are numerous issues here, but to state the bottom line bluntly, I do not believe it is the task of sociologists to solve what are, probably, unsolvable philosophical problems anyway. Neither do I believe that we sociologists need to pick philosophical sides once and for all – as in, for instance, self-descriptions of being a 'phenomenologist', common in contemporary qualitative sociology. In my view, there probably *are* no ready-made epistemological and ontological positions, which would set sociological inquiry straight. To sociologists of science, at least, epistemological and ontological questions should be part of the field of inquiry, not given coordinates.

If there is one generally accepted (philosophical!) label for such attempts to 'sociologize philosophy' – as opposed to 'philophizing sociology' – it is pragmatism, traceable to the work of the classical American pragmatists (see Kivinen & Piiroinen 2006). Given that much of this thesis is inspired by the work of Bruno Latour, it is worth noting that Latour considers himself an unreconstructed pragmatist (see Latour 2008a). What ties this American-French lineage together, arguably, is that scientific inquiry is viewed not as standing outside the realm of the social – as in 'standard' epistemology – but rather as a particular form of social action, a social practice. In this view, concepts are tools for solving particular problems; or as John Dewey put it in 1938, "ideas and beliefs are the same as hands: instruments for coping" (quoted in Kivinen & Piieroinen 2006:308f). Philosophical ideas, no less than scientific ones, are products of communal human activity, dependent on historical and cultural context. If we search for a philosophical house in which to host current-day sociology of science, including this thesis, surely it must be pragmatism (see Bertilsson 2004; 2009).

This exercise in locating philosophical bearings, however, still tells us quite little about the specific argumentative resources needed to establish the overall meta-analytical coordinates of this thesis. If anything, pragmatism invites us to view this issue in a specific way: as a question of locating the relevant disciplinary (and post-disciplinary) dialogues in which the ideas relevant to the research problem at hand are being developed and sharpened (see Camic & Joas 2004). Putting the problem of meta-theory this way makes it both more and less doable. More, because we leave behind the search for foundations – such as 'the true nature of social reality' – and start engaging in on-going conversations amongst sociological practitioners whose ideas we stand to benefit from. Less, because it once again becomes obvious that there simply is no way of engaging all the potentially relevant on-going conversations in contemporary sociology. In this sense, much of what goes into the making of this thesis is bound to remain tacit knowledge, apparently lacking in its context of meaning (see Burke 2002).

My meta-analytical approach to this conundrum is partial connections, this time signalling the partiality of the links I will be capable of drawing between my work and wider concerns of sociology.[15] In an attempt to cover some important ground, I proceed as follows. First, given the reflexive self-implications of this thesis – asking questions about the role of science in a social scientific way – I will start by outlining some points for self-contextualization, at the level of contemporary sociology. Following closely from this self-localization, I will attempt to visualize the analytical trajectory of this thesis – already hinted in the previous section – at the level of what I call a 'sociological settlement', meant to give precision to terms like theory and data. Building on these discussions, the next two sub-sections outline important developments in the sub-fields of sociology, to which this thesis aims to contribute: the 'new' sociology of knowledge (or STS), and environmental sociology. Finally, the section ends with a few notes on the extent to which this thesis can be understood within the 'world risk society' diagnosis, a well-known context of sociological knowledge these days.

Fractal distinctions in sociology: a few clarifications

In the apt language of American sociologist Andrew Abbott, sociology as a discipline has always had an 'interstitial' character, caught in-between intellectual styles pertaining to what C. P. Snow famously dubbed 'the two cultures' of the sciences and the humanities (Abbott 2001:6). Sociology is a discipline of endless social topics and almost as many 'thought-styles'. Nevertheless, Abbott convincingly argues that there are indeed logics to the way sociological knowledge is organized, and such logics rely on what he calls 'fractal distinctions'. A fractal distinction is a conceptual bifurcation, which tends to reproduce itself across any number of different levels, or scales, of knowledge. The best way of illustrating what this entails is to take a look at Abbott's 'methodological manifold' of the sociological discipline:

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15. This is where an analogy exists between Strathern's notion of partiality and an aesthetics of *the collage*: putting disjunctive elements drawn from multiple contexts together on a single plane invites active processes of meaning-making. Interestingly, German *Fluxus* artist Arthur Köpcke named one of his collages 'partial connections' already in 1967! John Law, in outlining his notion of 'mess' in the social sciences (2004), uses a similar aesthetics of the collage of social life. Strathern, however, is less of a 'romantic' than Law; instead of mess, she is dedicated to cultivating a new sense of appropriateness amongst social science practitioners.

Figure 1: The methodological manifold of sociology

Quantitative	versus	*Qualitative*
Positivism		Interpretation
Analysis		Narrative
Realism		Constructionism
Social structure		Culture
Individual level		Emergent level
Transcendent knowledge		Situated knowledge

Source: reproduced from Abbott 2001:28.

Presumably, what Abbott sets out in his methodological manifold represents a set of clustered distinctions that will be immediately recognizable to most practicing sociologists. This, in a sense, is exactly the point. In any number of conversations amongst sociologists, whether at a 'general' theoretical level or at the level of empirical details, some version of these fractal distinctions is likely to show up. In my estimation, Abbott is right to subsume these clusters, or 'elective affinities' (borrowing the term from Weber), under the general heading of 'quantitative' versus 'qualitative'. Few other distinctions are as closely related to sociological identity formations as this one. However, in different contexts, some or other associated distinction may become the centre of attention, as will indeed be the case with the 'realism-constructionism' distinction in this thesis.[16]

There are two reasons for me to reproduce Abbott's methodological manifold in the context of this introduction – beyond the fact that his approach to sociology is highly inspiring. First of all, to state what is probably obvious by now: by having invoked terms such as 'ethnographic case studies', 'detective narratives', 'situated knowledges' and 'constructivism', I have positioned myself solidly in just one half of this manifold – the right side, as it were (pun intended!). To some extent, this is an innocent thing to do, since there is no a

16. Throughout this thesis, and from now on in this section, I will be speaking of 'constructivism', not 'constructionism' (like Abbott). Indeed, I consider attempts to draw such distinctions ill founded, and will instead attempt to give the term constructivism a new set of sociological connotations later on.

priori superiority associated with *any* of these distinctions. Most often, where you position yourself is likely to be seen as a matter of sociological taste; and I find the methodological manifold useful for me in admitting that, indeed, my tastes lean towards the qualitative-narrative-constructivist pole of sociology. To a hardcore quantitative positivist, this entire thesis is likely to seem mumble jumble. Rather than pretending that we find all styles of sociological inquiry equally attractive, interesting or valid, it is probably better to be clear about these divisions.[17]

Now, the second reason for me to reproduce Abbott's methodological manifold relates to the fractal nature of his distinctions – and this point is meant to soften the sociological divisions just drawn up. Put briefly, the reason Abbott depicts these distinctions as fractal, is to avoid the conclusion – sadly widespread amongst sociologists – of saying that they should be absolute, context-independent philosophical baselines, cut in sociological stone. Instead, in line with the pragmatist ethos outlined (Abbott is a pragmatist; see Abbott 2004), these distinctions are fractal to the extent that they serve to orient sociological discussions across any number of contexts, ranging from conversations on the nature of social reality in general (ontology) to conversations on the correct interpretation of particular statistics (methods and data). Another way of saying this is that each distinction – say, between 'realism' and 'constructivism' – is entirely relative to the on-going context of sociological conversation. There simply is no such thing, for instance, as *the* constructivist position – only constructivist argumentative moves.[18]

This conclusion has a number of important implications for my arguments. First of all, it opens up an interesting prospect to consider when working across – as does this thesis – two sub-fields of sociology, which have both been at the

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17. True to pragmatist style, I should stress that my preference in this context for a 'qualitative' style of inquiry is tied to its ability to better elucidate my analytical problems – not to some universalistic standard of superiority. Nevertheless, since 'analytical problems' do not come ready-made off the shelf, either, but is shaped in dialogue with methodological considerations, there is still something to be said for the relative merits of this style across many contexts. I will say more about this in the paragraph on meta-theory.
18. In some sense, this is old news (see, e.g., Demeritt 1998). However, Abbott's point does not lend itself easily to distinctions, say, between 'soft' and 'hard' constructivism. Instead, it invites us to reflect on the conceptual work done by the label constructivism across different contexts of sociology.

forefront of recent 'realism versus constructivism' debates, that is, sociology of science and environmental sociology. The implication seems clear enough: what it means to 'be a constructivist', Abbott tells us, need not be the same thing in each of these sub-sociological conversations – as indeed I will argue they are not. Second, and related, it opens up to an interesting meta-theoretical interpretation of the sociological importance of the work of Bruno Latour, my main source of inspiration. Put very briefly, whereas Latour's work on scientific practices has been routinely caricatured as an 'extreme', 'relativist' and 'post-modern' version of social constructivism, in fact Latour is better seen as having made a *realist* move within an otherwise strongly social constructivist context (see Abbott 2001: chapter 3). This is why Latour is simultaneously criticised by colleagues in STS for having sold out to old-fashioned scientific realism (see Collins & Yearley 1992; Bloor 1999).

More will be said about Latourian constructivism – in terms of how he envisages relations between the social and the material, society and science – later in this section. For now, the important thing is to sum up the merits of Abbott's 'multiple fractal distinctions' model of sociological inquiry, relative to other attempts at social science self-reflection. For instance, compared to John R. Hall's (1999:178) distinction between 'generalizing' and 'particularizing' practices of inquiry – according to which my own approach would seem particularizing – Abbott's model has the merit of avoiding a misleading sense of universal scales against which any particular inquiry can be judged. While there *is* a distinction to be drawn between 'the general' and 'the particular' – or, in Abbott's language, between transcendent and situated knowledges – this is simply *not* a rock-hard distinction (or scale). Indeed, in the meta-theory section later in this introduction, I draw a distinction between the general and the abstract, allowing us to recalibrate these relations, by questioning what it means to generalize in sociology.

These are important points, because what Abbott's model allows is for us to change understanding of the topography of sociology. Hence, for instance, instead of two large groups of quantitative and qualitative sociologists, such approaches must now be located across a variety of contexts, including at the level of specific argumentative steps (such as this thesis). Similarly, and contrary to an often invoked principle in (European) sociology, grand theories – Foucault, Bourdieu, Habermas etc. – will no longer do as our sole contextualizing devices. While ‘Bourdieusian sociology’ may form its own context of conversation, important methodological distinctions – such as between positivism and interpretation – still create internal divisions. Furthermore, theories construct their own trajectories, such that you may make, say, a ‘Latourian’ move from a ‘Bourdieusian’ standpoint (see Schinkel 2007). As already noted, this thesis primarily makes a Latourian move into a sociological territory so far dominated by Ulrich Beck.

To conclude here, Abbott offers a nice metaphor for the topography of sociology: tourists walking around a rectangular-grid city, with east-west and north-south turns corresponding to successive rounds of fractal distinctions (2001:29ff). If the tourist-sociologists keep sticking to the same side of the distinctions – say, realists keep opposing constructivists – only a small part of the city will have been visited after a while; or, only few of the possible knowledges of society will have been explored. Major changes in sociology happens, according to Abbott, when someone starts reshuffling the affiliations between fractal distinctions, with the result that “somebody learns enough from somebody else to wander into a whole new area of the city” (2001:33). I believe there are good reasons for considering Latour amongst these more curious of tourists in contemporary sociology. To elaborate on a comment made by Abbott (2004:125), Latour is a master of ‘problematizing the obvious’, a powerful heuristic tool. When brought to bear on the sociologically obvious – such as distinctions between humans and non-humans, culture and nature, politics and science – new areas of inquiry open up.

A sociological settlement: visualizing the meta-analytical trajectory

To stay within this metaphor of topography, the obvious question to address now would seem to be this: exactly what parts of the sociological city are being visited in this thesis, and what route has been followed in getting there? Rudimentarily, these questions have already been answered: in sum, the thesis deals with questions concerning the role of science in global environmental conflicts, located in the borderlands between environmental sociology and sociology of science, using Bruno Latour and his ANT as a main tool of theoretical navigation. What is still not clear, however, is how to locate this trajectory in a wider territory of theoretical concerns in sociology. Once again, we seem to face a Strathernian problem of proportionality, of too many potential contexts.

This section attempts to locate the specific concerns of the thesis within a diagrammatic rendering of what I will term – using a thinly veiled reference to Latour's theory of modernity (1993; 1999b) – a 'sociological settlement'.[19] This settlement represents my attempt to reconstruct the meaning of, and relations between, particular genres of sociological knowledge making – epistemology, theory, data, and so on. The reason for undertaking such an exercise into issues usually considered tacit knowledge is pragmatic: in writing this introduction, problems have arisen in my attempt to articulate the exact analytical trajectory drawn up by my six essays. In the preceding section, I summarized these trajectories in a provisional language of 'empirical' and 'theoretical' logics of selection, and qualified the theoretical trajectory as going from simpler to more complex analytical questions. My visual model is an attempt to clarify what this means, given that theory and data are not the only categories relevant for scaling sociological knowledge.

Andrew Abbott is very much a sociologist interested in methodology. One implication of this is that my discussion so far, based on his sociological vision of the methodological manifold, has been rather methodology-centred. Methodology, however, is not all there is to sociology, nor is it the sole, or even the main, analytical concern of this thesis – although it is certainly an important concern throughout my essays. What this means is that, in order to visualize the route followed and the territory visited in this thesis, we need to broaden our vision of the relevant genres of sociological knowledge. In particular, in terms of science-society relations, Abbott admits that his own model is 'internalist' (2001:4); it tells us quite little about relations between sociology and the social world. To visualize the topography of this thesis, based as it is on two empirical case studies, we need to expand the methodological manifold into a 'sociological manifold'.

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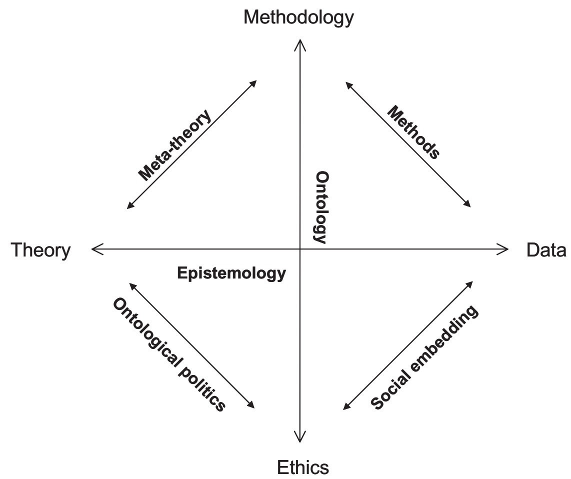
19. My rendering of this sociological settlement is particularly inspired, at least visually, by Latour's rendering of 'the modernist settlement' in his book *Pandora's Hope* (1999b:14). Having thought about it intensively, however, I have been unable to determine with any precision how exactly the two diagrams relate to each other. One thing is certain: I am *not* simply unfolding what Latour calls the 'society' part of his modernist settlement, since this would make sociology unable to say anything ontologically interesting about relations between humans and non-humans. I leave this question for later clarification.

Unsurprisingly, there are numerous ways of thinking about sociology-social world relations. Cutting long histories short, and jumping ahead, for the purposes of my sociological settlement, it seems fair to say that what interests most sociologists about this relation, basically, is the fact that the social world is a world of values (or ethics, or politics). What this means is that sociological inquiry necessarily enters into relations with value-laden empirical problems ('data'), whose contours have already been established long before the arrival of the sociologist. Put differently, sociological problems have the curious character of being 'over-determined' by cultural and political commitments (Brown 2009) – as my field of environmental conflicts illustrate. Indeed, sociology gains much of its attraction from this publicly engaged character, in a dialogical relation that, necessarily, entails some form of ethics in sociology (see Abbott 2007b).

On the backdrop of these reflections, the diagrammatic model of my sociological settlement looks as follows (figure 2):

Figure 2: A sociological settlement (author's illustration)



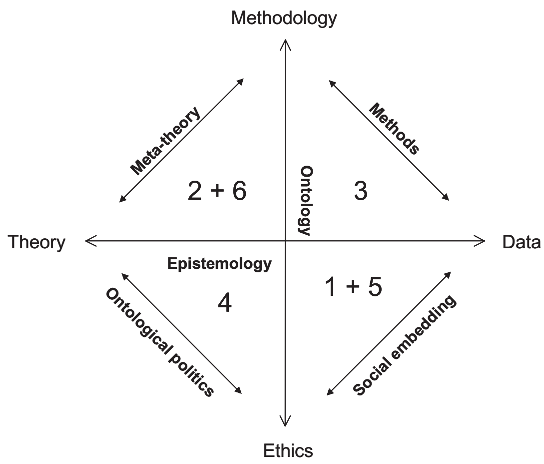
To reiterate, each of these terms designate genres of sociological knowledge; while they tell us something about the 'formal' requirements of sociological inquiry, they say little about its ‘contents’. Second, a corollary of this diagrammatic shape of visualization is that, while each of these genres may be temporarily fore-grounded relative to each other, the model expresses the idea that *any* piece of sociological inquiry positions itself relative to *all* of the genres simultaneously. Another way of saying this is that my sociological settlement is itself a fractal figure: we may use the model to reflect upon any scale of sociology, ranging from the history of the discipline (highly abstract) to a particular set of sociological texts, such as my essays (highly specific). Indeed, this is why the figure holds attraction for me in this thesis context.

In my estimation, the horizontal (epistemological) axis of the diagram should be self-explanatory: pitting sociological epistemology as a matter of ‘matching’ theory and data – inductively, deductively, abductively – is hardly controversial (see Bertilsson 2004). Still, this way of putting things expresses a pragmatist anti-foundationalism: to sociology, epistemology is not what ‘grounds’ the discipline, but rather what ‘flows’ inside any problem-driven inquiry (see Kivinen & Piieroinen 2006). By distinction, the vertical axis may require more justification, since depicting ontology as suspended between methodology and ethics is hardly self-explanatory. The argument is as follows: what this axis expresses is the tension between the social world as it already *is* (towards methodology) and what the social world may *become* (towards ethics). Again, like epistemology, ontology should not be seen as what ‘grounds’ sociology, but rather as what is at stake in any piece of inquiry. Linking ethics to ontology in this way expresses what Latour (2005) calls ‘the good common world’: ultimately, sociology is part of an on-going process of configuring the social world according to both knowledges and values.

In contemporary science and technology studies (STS), relations between science and ontology have acquired the name ‘ontological politics’ (Mol 1999; Law 2002; Latour 2005). The idea here is simple but far-reaching: since the ontology of the social world evolves in on-going processes, and since sociology is necessarily part of these processes, we must attend to the real world ‘enactment’ of sociological knowledges (see Law & Urry 2004). In the diagram, I make a distinction between this question of ontological politics – associating it with the pole of theory – and a standard sense of the ‘social embedding’ of sociology – associating this with the pole of empirical inquiry (data). The idea here is simply to express the ‘over-determined’ character of empirical sociological problems, as always-already part of ethical-political settlements. Again, the distinction between ontological politics and social embedding is one of degree rather than kind; we may think of it in terms of more or less theoretically developed justifications for particular ethical-political knowledge claims. Hence, for instance, the Latourian term 'cosmopolitics' to which article 4 (on whaling) in this thesis is dedicated.

Having outlined the contours of this sociological settlement, we can now turn to what remains the real purpose of this meta-analytical exercise: articulating with more precision the trajectories and territories drawn up by this thesis. For this purpose, I have plotted each of the six articles – referred to by the same numbers as used in the preceding section – within the different territories defined by the axes and lines of the diagram:

Figure 3: The essays of this thesis plotted in the sociological settlement (author's illustration)



To reiterate the basic starting point of this visual representation: articles 1 to 4 deal with the empirical case study of Japanese whaling controversies, whereas articles 5 and 6 deal with the case study of global carbon markets. What the diagram now allows is to clarify what I meant by the 'theoretical' logic pursued within each of these two empirical cases: the trajectory from 'simpler' to more ‘complex’ analytical issues is really a trajectory between different quadrants of this sociological settlement. In the language developed here, it is not really a ‘theoretical’ trajectory at all; instead, we may speak of a meta-analytical trajectory of inquiry going from social embedding to meta-theory, on to methods, and ending in ontological politics. The only strict sense in which issues of ontological politics are more ‘complex’ than issues of social embedding, I would argue, is that they are more foreign to the though-styles of ‘mainstream’ sociology. This is, in my view, where sociology stands to gain most from encounters with STS and Latour.

Using this vocabulary, it is possible for me to articulate not only what is at stake in each of my case studies, but also to start comparing across them in meaningful ways, that is, by reference to their respective quadrant of my sociological settlement. Hence, for instance, it will be clear in which sense the whaling study is more fully developed than the carbon market study: in the whaling case, I have visited two quadrants of sociology which I have not (yet) in the carbon market case. This means that, in terms of my overall analytical question in this thesis, I shall have more options for abstracting insights on the methods for studying, and the ontological politics of, science in global environmental conflicts from my whaling case than from my carbon market case. Nevertheless, I will still be able to compare the two studies at the level of social embedding and meta-theory; which, in my case, primarily means comparing at the level of empirical science-politics relations and at the level of ANT as a sociological theory.

This then also defines the parameters of discussion for the rest of this summary chapter. Before taking on these tasks directly, however, we need to establish a few more background contexts for understanding the analytical questions of the thesis. This can most fruitfully be done, I believe, by taking a closer look at the two sub-disciplines to which this work refers: the ‘new’ sociology of science (or STS), and environmental sociology. What distinguishes the following remarks is that I remain at the level of meta-theory, particularly leaning towards methodology, by focusing on that part of Abbott’s methodological manifold called the ‘realism versus constructivism’ debate. As I will attempt to show, sociology of science and environmental sociology have both had strong debates on constructivism over the past 20-30 years, but in quite different ways. What my thesis attempts, in this context, is to bring the two constructivisms together.

The 'new' sociology of science: on Latourian constructivism

The history of constructivist thinking in the small sub-field of sociology known originally as the sociology of knowledge, and later – since the early 1970s – branching into the 'sociology of scientific knowledge' (SSK), is extensive indeed.[20] My exposition will be idiosyncratic in telling the story from the distinct vantage point of Bruno Latour. What makes Latourian constructivism distinct is that, while indeed 'radically' constructivist in the sense that *everything* is constructed (that is, has a specific history of emergence), his position is by no means another version of 'social constructivism'. What this means is that neither arguments in favour of social constructivism, nor the vast amount of reasonable criticisms (cf. Hacking 1999), has much bearing on Latourian constructivism. This also implies that the very meaning of the term social is put at stake.

Ultimately, the main issue raised by any sociology of science is how we should understand the relation between two complex entities, 'science' and 'society' (see Strathern 2005). To sociologists working *outside* the sociology of science, answering this question is likely to be viewed as a sub-category of a broader question of how modern society is internally differentiated. Hence, to Luhmannians, science is an autopoietic sub-system of the functionally differentiated society, whereas to Bourdieusians, science is a relatively autonomous societal field of symbolic and economic capital distributions. While this may be interesting, the sociology of science has historically developed its own ways of addressing the issue, not bound to 'grand' sociological theorizing. Instead, a number of 'middle range' theories have specifically addressed science-society relations. Incidentally, the most famous of these theories – Merton's on 'the normative structure of science' (1973

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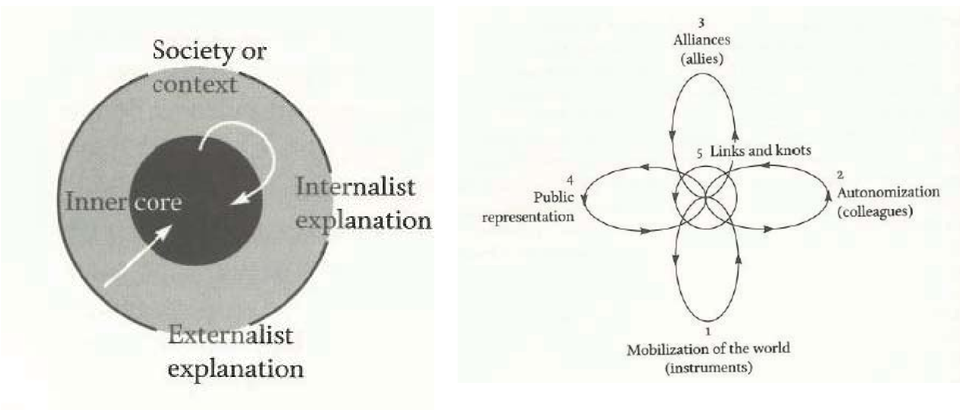
20. Besides Abbott (2001), who sums up this history as seen from the vantage point of 'general' sociology, Barbara Herrnstein-Smith (2006), American linguist and STS scholar, provides a helpful mapping of relations amongst important 'constructivist' science thinkers such as Ludwig Fleck (in the 1930s), Thomas Kuhn (in the 1960s), Michel Foucault (in the 1970s), SSK sociologists David Bloor, Barry Barnes and Steven Woolgar (in the 1980s), and Bruno Latour (in the 1990s).

[1942]) – stems from the same man who first drew this useful distinction between grand theories and theories of the middle range in sociology (Merton 1968 [1949]).

This is not the place to salvage Merton and his passionate defence of the autonomy of science as a normative communal order, and his simultaneous defence of the wider liberal society, against the threats of fascism and communism in early 1940s United States. Suffice to say that, in current-day sociology of scientific knowledge (SSK), including in the field of science and technology studies (STS), leading thinkers, such as Bruno Latour, takes great pride in having overcome the limitations of Mertonian sociology. What this means is that Mertonian sociology of science is heavily criticised for implicitly relying on a strong distinction between the 'inner core' of science, on the one hand, and the social conditions of science, on the other. Hence, in the unfriendly language of Latour (1999b), Merton may have had a theory of the social *organization* of science, and he may have used his theory to explain scientific deviations from truth ('sociology of error') – but he had no theory of scientific truth-production as an altogether social practice. This latter objective is what Latour and other STS scholars are aiming at.

How different theorists attempt to overcome this sharp distinction between the inner core of science and the wider societal context varies greatly amongst different schools of contemporary sociology of science (see Yearley 2005b). Again, I will be idiosyncratic and stick with Latour. His particular take on this issue is illustrated in figure 4, juxtaposing two different models of science-society relations:

Figure 4: Two models of science-society relations (from Latour 1999b: 92, 100)



On the left-hand side, Latour depicts a compact history of the 'old' Merton-style sociology of science: having set up a sharp distinction between the inner core and the social context of science, endless debates can now ensue between 'internalist' and 'externalist' explanation. Whereas the internalists (like Abbott) prefer to explain developments in science by reference to factors such as theoretical breakthroughs, new laboratory equipment and inter-paradigmatic competition amongst scientists, the externalists cling to more 'sociological' factors such as power-laden discourse coalitions (e.g. 'biotechnology is good for growth'), the political economy of research (e.g. 'the neo-liberal university') and dominant societal ideologies (e.g. 'the modern scientific-mechanistic worldview'). Latour's main point is *not* that such explanatory attempts cannot help us make sense of science; rather, it is that the entire distinction between 'inner core' and 'social context' is misguided. This is what the right-hand side model is meant to rectify, depicting what Latour holds to be the 'new' sociology of science, or simply science studies (STS).[21]

What Latour depicts here is a vision of science as a set of recursive social practices – five set of practices to be specific – all of which mix up elements of (what used to be) the 'inner core' and (what used to be) the 'social context'. In other words, and strictly speaking, what Latour is inviting us to do is to abandon the abstract language of 'science-society relations' altogether, by replacing it with a more precise vocabulary for analyzing how scientific practices mobilize and co-creates its own public and political contexts. In Latourian sociology of science (that is, in ANT), scientists are always taken to be powerful world-building actors, busy co-constructing and interlinking ('hybridizing') natural and social worlds (see Latour 1993). This is why Latour has written a book called *The Pasteurization of France* (1988): the sociological

21. From now on in the text, I make no attempt to distinguish sharply between this 'new' sociology of science and the field of science and technology studies (STS). To reiterate, while these sub-fields consist of different sets of internal conversations and disagreements, by sticking to Latourian constructivism, I am anyway trying to pull them closer together; hence the deliberate blurring of the boundary.

point is that ‘Pasteur’ constructs ‘France’ just as much as ‘France’ constructs ‘Pasteur’.[22]

The second point to note is that these recursive sets of scientific practices – all of which are necessary, according to Latour, for scientific facts to emerge – bypasses distinctions between the social and the material. At the level of his theory of science, Latour’s point need not be controversial: any visit to a natural science laboratory is likely to convince us that science is a densely material practice, laboratories brimming with technological artefacts, chemical compounds, experimental set-ups, rats to be sacrificed, monitors, written protocols, and so on and so forth (Latour & Woolgar 1979/1986). To understand even the basics of scientific practice, one needs to abandon the prejudice of keeping social and material ‘factors’ separate. Later on, I am going to talk about a ‘material turn’ in social theory, to which Latour is a main contributor (see Latour 2000b). For now, it suffices to note that Latour’s point about the materiality of scientific practice is easily conceded – including when we think about the social sciences (see Whatmore 2003).

The third and final point to note is the most important in terms of understanding Latour’s particular version of constructivism, and it has to do with the recursive nature of the loops in the model. The basic point, at the level of philosophy of science, is that scientific facts do not ‘correspond’ to some ‘external’ natural reality; instead, scientific facts are made to ‘flow’ inside specific infrastructures, constructed by means of social (human) as well as material (non-human) elements. In Latour’s own (provocative, but very pragmatist) metaphor, scientific facts are like railroads: they allow for transportation of certain knowledges along laid-out tracks, but only in very specific directions, using very specific vehicles, and only as long as the tracks have been meticulously constructed. During his career, Latour has used any number of vocabularies for establishing this basic point about science: ‘actor-network’ is one such term, ‘circulating reference’ another (see Latour 1999b). Regardless of terminology, however, the point remains the same.

Based on these premises, Latour is capable of saying *both* that ‘facts are fabricated’ (2000a), *and* still claim to be entirely realist about the world (Latour

22. Both of these entities are put in inverted commas, because I have not yet outlined Latour’s idiosyncratic theory of social actants. This is the job for the meta-theory section of this summary chapter.

1999b). This is why Latour prides himself of having gone *beyond* the realism-constructivism divide, replacing it with an empirical agenda of studying just how specific sciences meticulously lay out their railroad tracks of knowledge in the social world. This, in a nutshell, is the social scientific agenda build into ANT (see Callon & Latour 1981). Latourian constructivism is a matter of denying any ultimate distinctions between the social and the material, humans and non-humans, culture and nature, politics and science, in the first place (Schinkel 2007). Small wonder if this intellectual project should give rise to sceptical responses from 'social constructivists' and 'realists' alike!

While Latour may thus claims to have overcome the realism-constructivism divide at the level of science studies, if we enlarge our frame of sociological reference, comparing him for instance to the methodologies of John Goldthorpe and Andrew Abbott, Latour's standpoint still belongs to the more narrative-constructivist pole of sociology (see Savage 2009). Latour is thus best understood as having made a realist move – associated with a material turn – in an otherwise highly social constructivist context, that of the sociology of scientific knowledge (SSK) in the late 1970s. While this insight helps in bringing lofty philosophical arguments on science down to sociological ground, the fact remains that Latourian constructivism is still an inspiring novelty.

Environmental sociology: inter/disciplinary borderlands

As noted, this thesis attempts to position itself in the juxtaposition of two sub-fields of sociological inquiry: besides sociology of science, the second important context of reference is environmental sociology. As also pointed out, relations between these two sub-fields do exist; however, one of my main ambitions here is to help bring their respective discussions closer together. The main trajectory for this effort of bridge building throughout the thesis will be to explore in some detail the implications of Latourian constructivism – as just outlined – for the main analytical concerns of 'standard' environmental sociology. In this sense, my engagement with environmental sociology parallels the point expressed in the introductory quote from Latour: having already done some work in the sociology of science, I *revisit* environmental sociology in order to explore the implications.[23] Another way of saying this is that these discussions will be entirely selective, focussing on how the realism-constructivism debate has played itself out in this environmental context.

Parallel to the way in which sociology of science conceives itself in terms of science-society relations, environmental sociology focuses on relations between society and the natural environment. Probably for the very reason of being situated at the centre of Latour's (1993) 'Modern Constitution' – separating culture from nature – environmental sociology has seen a fair share of heated realism-constructivism debate. Beyond issues of social science methodology, what has made these discussions especially heated is the sense that where you land on the realism-constructivism continuum will have important political consequences beyond academia (see Lidskog 2001; Newton 2007).[24] Put provocatively, methodological debates are strongly entangled in an 'ontological politics' of just how *real* and serious environmental problems *are*. Given the normative leanings of most environmental sociologists, the debate is one of how to convince a sometimes-sceptical public of the *reality* of environmental crisis – especially if one adopts (what is perceived as) an 'anti-realist' constructivist position?

Starting from broadly realist assumptions, attempts have been made, mostly by American sociologists, to basically re-construct the discipline of sociology – scolded for having too long ignored environmental constraints – into a branch of human ecology, with humans cast as an integral part of bio-physical reality (see Catton & Dunlap 1978). Unsurprisingly, such attempts have met with criticisms of 'material reductionism' – mostly from European sociologists. Conversely, starting from broadly constructivist premises, European sociologists have gradually attempted to rethink the socio-natural (or 'hybrid')

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23. There is an autobiographical reference embedded in this statement: in becoming a sociologist, I have always had a special fondness for environmental sociology – the topic, amongst other things, for my master thesis (see Blok 2007). I have also worked for some years in an interdisciplinary social science environmental research setting, at the Danish National Institute for Environmental Research ("DMU" in Danish).

24. Needless to say, the same is true for constructivism debates in the sociology of science – after all, this is where 'the science wars' of the 1990s were instigated. However, whereas sociologists of science have by and large been able to ignore these heated reactions (with the exception, maybe, of Latour; Latour 1999b), environmental sociologists, no doubt for political identity reasons, have been addressing them openly.

character of ‘contested natures’ (Macnaughton & Urry 1998) and global environmental flows (Spaargaren, Mol & Bruyninckx 2006). These attempts, however, tend to meet with criticisms of ‘naivety’ from American sociologists (see Dunlap 2002) – presumably more concerned with environmental destruction!

This short caricature of what is in reality a much more variegated landscape of environmental sociology is only meant to underscore two analytical points, both important for the contextualization of this thesis.[25] First, following 20-30 years of controversy, a sizable handful of sociologists working on environmental issues seem to have reached the ‘pragmatic’ conclusion of wanting to put realism-constructivism debates to rest. The entire discussion is now routinely dismissed as ‘sterile’ and ‘tedious’, as authors attempt to settle on some or other compromise formula, capable of integrating discursive and extra-discursive features of body and environment (see Dickens 2001; Irwin 2001; Newton 2007). Some social constructivists, for instance, have attempted to make clear that they do not *deny* the reality of environmental problems, but merely remain *agnostic* about these conditions (Burningham & Cooper 1999). Environmental realists, however, typically respond by pointing to the normative shortcomings of agnosticism.

This somewhat unsatisfactory state of theoretical and normative discussion brings me to my second analytical point in this context, namely the claim that, in order to *actually* move beyond the realism-constructivism debate in environmental sociology, sociologists will need to learn from Latourian constructivism. In this respect, a number of sociologists and STS scholars working simultaneously on science and environmental problems – notably Sheila Jasanoff (1997; 2004a), Brian Wynne (1996; 2002), Alan Irwin (1995; 2001) and (to some extent) Steven Yearley (1996; 2005a)[26] – have started to

25. Irwin (2001) and Yearley (2005a) both provide excellent starting points for readers interested in learning more about contemporary environmental sociology. Not coincidentally, both of these authors have strong intellectual affiliations to the field of science and technology studies (STS).
26. I say ‘to some extent’ in relation to Yearley because, in the context of science studies (STS), Yearley ranks as an important ‘social constructivist’ critic of Latourian ANT (see Yearley 2005b). This may, however, be another instance of fractal distinctions: what in the context of STS looks like a disagreement between Latour and Yearley may well, in the context of environmental sociology, be more of an agreement!

elaborate a promising model of ‘co-constructivism’ very much congenial to Latourian constructivism. On this co-constructivist account, society and nature are seen as so closely intertwined and mutually constitutive so as to make it rather meaningless to speak of natures being ‘socially’, as opposed to socio-materially, constructed. As signalled by the title of my thesis, I find this language of co-constructivism analytically attractive.

In a related direction, closer still to my concerns in this thesis, dialogues between environmental sociology and Latourian ANT thinking have started to emerge in the last decade or so (see Murdoch 2001; Castree & MacMillan 2001; Lockie 2004; Hannigan 2006; Asplen 2006; Arnoldi 2009). While most of these analysts share theoretical sympathies with Latour and his ‘material turn’, so far there is little consensus as to the merits of his programme relative to competing paradigms of environmental sociology. Murdoch (2001) comes closest to endorsing the Latourian agenda, in arguing that actor-network theory is indeed a co-constructivist alternative to existing theories in the field, and one that allows for a productive ‘ecologising’ of sociology. These are issues that I deal with centrally in several of the articles of this thesis, particularly when it comes to interpreting the socially ‘agential’ status of non-human whales (articles 2 and 4).

What is important to note, then, is that a certain ‘borderland’ is opening up in-between environmental sociology and the ‘new’ sociology of science (STS), unified under the label of co-constructivism. Borderland refers here to a shared set of topical issues, concepts and methodologies, in this case unified around an analytical interest in the role of scientific knowledge in relations between society and the environment (see Zimmerer 2007 on ‘environmental borderlands’). I consider my thesis as situated within this exact borderland, participating in what we might call a shared ‘local research frontier’ (see Mjøset 2006). Research frontiers should not be understood in purely disciplinary terms; instead, they represent mixtures of sociological, interdisciplinary and public concerns, articulated within the parameters of a sociological settlement. Sharing what he calls a ‘pragmatist attitude’, I fully agree with Mjøset’s assessment that “social science knowledge mainly grows in such frontiers” (2006:756).

This conclusion has another set of implications, the opening of which would represent something like Pandora’s box: the local research frontier into which this thesis is articulated cannot really be confined to the disciplinary parameters of sociology. Instead, as is increasingly common across the social sciences, the borderland between environmental sociology and STS represents a highly porous zone of mobile interdisciplinary encounters (see Jacobs & Frickel 2009). In researching this thesis, I have constantly criss-crossed disciplinary boundaries at the level of the debates from which I have benefited, theoretically and empirically. To name only the most relevant zones, throughout this thesis, I engage with debates in anthropology on 'global assemblages' of nature (e.g. Ingold 1993; Bowker 2005); with debates in human geography on human-animal relations (e.g. Philo & Wilbert 2000; Whatmore 2002); and with debates in science studies (STS) on the co-production of science, society and nature (Jasanoff 2004b). Besides being reflected in the diversity of journals to which my articles have been submitted, I strongly believe that these interdisciplinary 'environmental borderland' journeys have contributed to my overall analytical quest, which remains nonetheless sociologically defined.

To end this exercise in self-contextualization, I want to turn now, briefly, to some well-known sociological concepts, themselves part of both interdisciplinary and public borderlands, asking to what extent my thesis can be understood in light of their respective contexts. In particular, I have in mind Beck's well-known diagnosis of the 'world risk society' – square quotes setting the tone for further discussion.

Ulrich Beck's 'world risk society': context or panorama?

Theoretical treatises in sociology rarely become popular best sellers. Ulrich Beck's *Risikogesellschaft*, however, is somewhat of an exception, allegedly selling an impressive 60.000 copies within five years of its 1986 German publication (Lash & Wynne 1992). For sociologists, the evocative concept of a risk society has proven good to think with: whether hailed for its theoretical breakthroughs (Mythen 2007) or dismissed as a mythological 'environmentalist manifesto' (Alexander & Smith 1996), Beck's theorizing engages the sociological imagination. Thinking of society *as* a risk society has become part of a professional *habitus*, extending beyond the confines of academic sociology, and taking on an emblematic character symbolic of urgent real-world problems (Jensen & Blok 2008). Meanwhile, the notion of 'risk' has constituted itself as one of the more exciting trading zones between the sub-field of environmental sociology and broader concerns of the discipline (Arnoldi 2009).

As noted several times, this thesis is not *about* Beck's risk society theorizing, but rather represents a sustained attempt to re-conceptualize, by way of Latour and others, some central analytical concerns found in Beck's risk sociology. This does not mean, however, that Beck-inspired notions – particularly in the shape of 'world risk society' – are irrelevant to my concerns. Indeed, given its widespread sociological currency, it is fair to surmount that Beck's risk society diagnosis necessarily forms one of the immediate interpretive contexts of this thesis anyway, regardless of my intentions. This implies that I need some means of *translating* Beck's theoretical concerns into a language more congenial to my own modality of sociological inquiry. Once again, Latourian constructivism provides such a language: in what follows, Beck's risk theory is interpreted through the lenses of the Latourian concepts of 'network', 'quasi-object', and 'panorama'. First, however, a few introductory remarks about world risk society and why it matters.

The overall sociological tenets of Beck's risk society argument are well known: as modern industrial society advances, its associated production of techno-scientific risks is gaining a new, epoch-defining significance. Gradually, the logic of wealth distribution in 'first' modernity is being replaced, or overlapped, by the logic of risk distribution in 'second', late, or reflexive modernity (see Jensen & Blok 2008). Risk, in this context, signifies the gulf between a world of quantifiable and controllable dangers, on the one hand, and a new world of non-quantifiable insecurities, on the other. Decisions about nuclear energy, nanotechnology or financial investments unleash unpredictable and uncontrollable consequences – human-made 'manufactured uncertainties' – that might ultimately endanger all life on planet Earth (see Beck 2002). Anthropogenic global warming has quickly become a favourite illustration of this gradual 'de-bounding' of uncontrollable risks – as has indeed, in a different context, the worldwide risks of over-harvesting posed to endangered species such as whales (see Franklin 1999). At first glance, then, my cases in this thesis are clearly 'world risk society' cases.

Beck's line of risk sociology simultaneously enjoys prima facie credibility, in drawing strength from widespread societal narratives (not least environmental ones); *and* makes grand theoretical claims on the nature of late modernity, the status of which is as uncertain as the risks posited by his theory. The central tools of Beck's diagnostics – risk knowledges, manufactured uncertainties, counter-expertise, reflexive science, and so on (see Beck 1992; 1999; 2002) – are as conceptually slippery as they are sociologically suggestive. This 'intelligently speculative' character of Beck's risk sociology has led into some criticism for being empirically unfounded (e.g. Seippel 1998), conceptually misguided (e.g. Wynne 1996), and eco-ideologically exaggerated (e.g. Mol & Spaargaren 1993). In his more cool-headed commentary, Latour (2003:40) confines himself to noting that Beck still writes as if he was "roaming freely through the 'whole' of society". For this reason, Latour suggests, we should be careful with the risk society label.

To expand on this point by way of paraphrasing a provocative Latourian trope from a different context (2000b): all would be well with 'world risk society', except for the term 'world', the term 'risk', and the term 'society'! To start with risk, Beck's theorizing is notoriously ambiguous on core issues here: are today's globalizing risks, such as climate change, 'real phenomena' or 'social constructions'? If such 'risks' are indeed incalculable and uncontrollable, would it not be better to speak of 'uncertainty', given that risk – even in Beck's thinking – is inextricably bound up with notions of control and calculability? (Arnoldi 2009:46ff). Further, what exactly do we gain analytically by thinking, as does Beck (2002), about such disjunctive phenomena as global warming, the Asian financial crisis and the September 11th terrorist attacks using basically the same limited set of conceptual tools? Do we really need such quasi-generalizations to make sense of scientific knowledges in public controversies over whale and climatic futures – to restate the ultimate analytical goal of this thesis?

As implied, Latour's notion of 'network' provides a convenient alternative tool here. Latour makes this connection himself by stating that risk and network are "two similar ways of exploring the new type of complexities typical of late-, second or non-modernity once the black boxes of science and technology, so typical of the first modernities […] have began to leak hopelessly" (2003:36).[27] A 'network', in this ANT sense, refers to the unravelling of a maze of

27. Latour writes 'modernities' in plural because in his thinking, modernizing narratives have always been post-hoc retrofitting devices applied to a world that, basically, has never been modern (Latour 2003).

unexpected relations between heterogeneous elements – elements that, sooner or later, is likely to bring one into contact with science and technology. For instance, you begin with a piece of whale *sushi* in a Tokyo restaurant, and before long, you will have visited population biologists in the Antarctic Ocean, diplomats in the International Whaling Commission, Greenpeace activists, and small whaling communities along the Japanese coast.[28] Within this network, the endangered whale has become a 'quasi-object' – a dishevelled and risky object, a 'matter of concern' not a 'matter of fact' (Latour 2004d). The important point about quasi-objects is that, unlike Beck's somewhat free-floating risks, they always deviate from the straight paths of scientific reason and control by way of *specific* detours and entanglements (Latour 2003).

Dwelling on the notion of risk through a Latourian vantage point already suggests certain issues associated with the other two terms in Beck's world risk society. First, the notion of 'world', like other often-used terms such as 'global' and 'globalization', suggests the idea that contemporary social life should be nested into one, and *only* one, overarching socio-spatial context, that of the planetary whole. Indeed, Beck (2002:41) goes so far as to suggest that ecological conflicts are "by their very essence global". This, it seems to me, is at best a half-truth, at worst positively misleading. First, a long list of strictly *local* ecological conflicts could easily be assembled – from landscape management to exhaust particles from urban cars. Second, even when confining ourselves to ecological issues labelled 'global' – such as, indeed, the whale protection and climate change cases in this thesis – the 'globality' of such assemblages can be constituted quite differently, as will be shown throughout my essays (see Yearley 1996; Bowker 2005). Rather than an analytical resource, the local-global distinction – including such designations as 'world society' – should really be taken as topic for empirical exploration.[29]

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28. As the reader may have guessed, this is not too far from an autobiographical sketch of my Japanese whaling studies. Articles 1 to 4 in this thesis are dedicated, in this broad sense, to unravelling the Japanese whaling network; and article 3 is a methodological reflection on this practice of mapping.
29. Implicitly, this invokes Garfinkel's well-known distinction between resource and topic: the ethnomethodological task, most often, is to turn what others take as analytical resources into topics for further exploration. In many ways, Latourian sociology is a direct descendant of Garfinkel (see Latour 2003).

Similar problems confront us with the very notion of 'society', arguably the stock-in-trade of the disciplinary sociological tradition. Exploring this issue in any depth will take us into the core of ANT, the topic of the next section. Suffice to say that any sociological notion of society suffers, from a Latourian perspective, from two deficiencies.[30] First, it is hampered by its strong association, build up over 150 years, with a particular historical form of political organisation, that of the modern nation-state, to the point of becoming almost co-synonymous – at least until 'world society' came along. In any case, the concept of society suggests a too static socio-spatial demarcation of mobile social life (Urry 2000a). Second, at least in sociology, society is usually assumed to consist of humans only – or, alternatively, of the products of human activity (as in Luhmann's communicative paradigm). Latourian networks are always conceived as heterogeneous, criss-crossing boundaries of human and non-human, social and material (Schinkel 2007). Instead of society, Latour simply speaks of 'collectives', designating this as the new object of sociology (2005; see Kneer 2008).

By way of summing up this movement between Beck and Latour, strictly speaking we might thus be inclined to transform the term 'world risk society' into something like 'local-global quasi-object collectives' – to better reflect the modality of sociological inquiry represented in this thesis. As we shall see, however, 'local-global quasi-object collectives' would simply be an ugly conceptual substitute for the 'actor-networks' of ANT – better, then, to stick with the original! At the same time, it is important to highlight why, for all their theoretical differences, Latour is nevertheless highly sympathetic towards Beck's risk sociology, to the extent of designating it "one of the most lively, creative and politically relevant forms of sociology developed in recent years" (2003:35). To be sure, there are clear thematic overlaps between the two bodies of theorizing, both of which cast techno-scientific risks and ecological entanglements as constitutive of our current societal predicament. As already noted, however, any number of sociologists has done so during the past decades

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30. The qualification of society as a *sociological* concept stems from the observation that, as Gabriel Tarde put it one hundred years ago, 'everything is a society' (see Latour 2002a). Hence, we may speak of 'plant societies', 'whale societies', 'societies of stars' and so on. This point has largely been ignored in sociology.

(e.g. Luhmann 1989; Eder 1996; Giddens 1990), without receiving the same Latourian praise. So, what constitutes the Latour-Beck alliance?

To fully understand Latour's appreciation of Beck's work, we need to embrace the idea that social theories are as much performative as they are descriptive. Put differently, and as Latour notes (2003:46), Beck's theory of risk "might not describe what has already happened, but it can offer a powerful lever to make new things happen". World risk society, on this account, is a powerful proposition, because it shifts attention away from the official face of modernisation to its unwanted, unpredictable, and uncontrollable material and symbolic side effects – many of which, Beck and Latour agrees, are ecological in character. In this move, as Beck has consistently stressed (1992), risk society simultaneously emerges as a 'knowledge society', marked by deep influences from, and controversies surrounding, knowledges and expertise in the realm of techno-science. Again, the parallels to Latour should be obvious – even if, I argue, Latour simply has a more elaborated and empirically sensitive sociology of science than Beck. Indeed, this is largely what I mean by pursuing a 'post-risk society' theoretical agenda for sociology.[31]

Latour (2005) has recently come up with a nice metaphor for talking about the performative aspects of social theories: like other grand narratives, social theories act as 'panoramas', providing a self-enclosed, 360-degrees, visually attractive, all-encompassing view of the totality of social life. Panoramas can be exciting or boring, inspiring or disabling, original or routine, politically progressive or reactionary. What they *cannot* be, however, are truthful depictions of 'the whole of society' – simply because there is no such thing as 'the whole of society', only socially situated summing-ups (panoramas). World risk society, I agree with Latour, is an exciting, original, and politically progressive sociological panorama, the viewing of which enables any number of inspiring sociological agendas. Once viewed, however, the sociologist needs to get out of the panorama – and into the actor-networks in which the action of risky quasi-objects unfold in dynamic time-space. This explains why the majority of this thesis takes place inside the hybrid actor-networks of endangered whales and contested carbon markets.

31. As will emerge later when discussing his notion of 'methodological cosmopolitanism', I believe the more recent work of Beck himself to be integral to such a theoretical agenda.

Meta-theory: actor-networking the global environment

In the preceding section, I have attempted to outline some meta-analytical underpinnings of this thesis, in terms of establishing (partial) connections between certain relevant contexts of contemporary sociology. Drawing on Abbott's methodological manifold, focus has primarily been on ways in which so-called realism-constructivism debates have played themselves out across two sub-fields of sociology, the 'new' sociology of science (or STS) and environmental sociology. This leads me into a critically appraising discussion on Ulrich Beck's 'world risk society' diagnosis, in terms of how Latourian co-constructivism reassembles Beck's inspiring sociological thinking under new conceptual forms. The purpose of this section on meta-theory, then, is to start unfolding in some more detail what these conceptual forms are, unpacking the label of 'actor-network theory' (ANT).

In reflecting on 20 years of ANT, Latour (1999a) employs the same rhetorical trope that I just attached to Beck: all is well in actor-network theory, except the word 'actor', the word 'network', the word 'theory' – and the hyphen! If by social theory we understand some set of substantive, positive statements as to what constitutes a society, a group, an actor, a social field, a power relation and so on, then ANT is hardly a social theory at all. More precisely, in ANT parlance, these questions are not really questions of social theory, but rather questions constitutive of *the social itself* – in other words, questions whose gradual unfolding is the job of social actors, not sociologists or social theorists. Another way of saying this is that ANT is primarily a method for tracing associations between non-social elements, human and non-human, in terms of how actors themselves gradually solve socio-ontological issues (Latour 1999a; 2003; 2005). 'Follow the actor' remains the Latourian slogan par excellence. If ANT is still to be called a 'theory', Latour insists that it is so only in a negative sense: providing a set of minimal conceptual devices – actants, quasi-objects, networks, panoramas etc. – which allows the social analyst to *avoid* confusing his or her own poor vocabulary with the rich one of social actors.

Saying this much is already to open a can of worms designated by terms such as 'empiricism' and 'descriptivism', much ridiculed amongst sociologists at least until recently (see Adkins & Lury 2009 – is a sea change under way?). Indeed, Latour is probably unique in the contemporary world of social theory in openly embracing these terms – albeit, for reasons of his theory of science, he speaks of a 'second empiricism', an empiricism of risky quasi-objects rather than solid matters-of-facts. More will be said about Latourian descriptivism later, when I engage in a small exercise of sympathetic criticism of his sociology. As of now, suffice to say that it makes little sense to adopt ANT as a methodological starting point if one is not committed to the idea, formulated most succinctly in sociology by Garfinkel's ethnomethodology (1967), that it is social *theorists* who must learn from social *actors* the meaning of their practices. ANT is a contemporary version of practice theory (Schatzki 2001); or, in other words, a version of French neo-pragmatism (Vandenberghe 2006).[32] Latour's project, in a nutshell, is to draw as many meta-theoretical implications as possible from his pragmatist starting point – to the extent of calling his genre of sociology 'empirical philosophy' (see Latour 2005).

Moving on to the 'actor' of actor-network theory, the problem with this term has already been suggested: according to Latour, most sociological theories operate on much too constrained assumptions as to what constitutes a social actor. Not only is sociology haunted by futile debates between rational and norm-oriented actor models (see Boltanski & Thévenot 2006). More importantly, Latour argues (1996b), sociologists routinely exclude the vast majority of active entities from their disciplinary consideration by affirming that only *humans* exert social agency. In ANT, an actor is whatever *affects* the course of social action, be it human or non-human: Greenpeace, Japan, digital cameras, population cetologists, intelligent whales, carbon dioxide molecules – these can all be social actors (and indeed they are in my empirical studies). Inspired by the semiotics of Algirdas J. Greimas, Latourian actors are really 'actants', relational semiotic agencies co-inhabiting some larger narrative of the unfolding of social events.

This notion of non-human actants is what has made Latour notoriously famous: his is a social theory where 'material objects have agency too'. Together with developments in the fields of mobility studies, human geography, material culture studies, philosophy of technology, feminist science

32. Alongside Latour, arguably the main exponents of this emerging category are Luc Boltanski and Laurent Thévenot, co-authors of a major recent work in social theory, *On Justification* (2006 [1991]). Latour makes ample references to this book throughout his writings since the early 1990s.

studies etc. – all experiencing a need to address 'late modernity' issues of nature, the body, foodstuffs, auto-mobility, spatial planning and new media technologies – this notion of non-human actants has even inspired commentators to talk of a 'material turn' in recent social theory (see Pels, Hetherington & Vandenberghe 2002; Urry 2000b; Alaimo & Hekman 2008). In my view, it is debatable just how much of a 'turn' these scattered developments amount to, at least in so far as sociology is concerned (the announcement of 'turns' – linguistic, affective, and so on – easily risk becoming an empty rhetorical gesture). Nevertheless, to the extent that materiality is back in sociology, clearly most of the honour is befallen on Latour and his non-human actants.

Now, admitting that 'materiality matters' to social life is one thing. Overcoming deep-seated dichotomies of sociality and materiality, society and technology, subject and object, culture and nature, for the purposes of social inquiry demands, however, conceptual work of quite a different order of complexity. In a certain sense, all of Bruno Latour's work may be interpreted as a sustained attempt to escape the confines of having to oscillate incessantly (or 'dialectically') between such modernist bipolarities.[33] In-depth consideration of this anti-dualism would take us into, amongst other things, Leibnizian monadology and Whiteheadian process metaphysics – that is, beyond the confines of this thesis (see Harman 2009). Suffice to say that, rather than trying to 'relate humans to non-humans' as if they were indeed divided, any number of Latourian concepts – hybrids, quasi-objects, actants, networks – is really meant as tools for *bypassing* this nature-culture bipolarity altogether (Latour 2005:63ff). To the well known 'inter-subjectivity' of sociology, Latour adds 'inter-objectivity' (1996b). With ANT, we go from problems of 'social order' to problems of 'socio-material ordering' (see Law 1994).

This provides a nice bridge to the next problematic concept in line, that of the 'network'. The trouble here is straightforward: since ANT founders Bruno Latour, Michel Callon and John Law adopted the term in the early 1980s, networks have become so ubiquitous, so commonsensical, and so domesticated in everyday language as to ruin any social theoretical shock value the term might once have enjoyed. Any would-be sociologist these days know that we

33. I draw here on an interpretation of Latour's work that I have developed in more depth, together with Torben Elgaard Jensen, in a different context (Blok & Elgaard Jensen 2009).

live in ‘network society’ (Castells 1996), that Internet is the model ‘global network’ (e.g. Cavanagh 2007), and that you need weak ties in your ‘social network’ to get a job (Granovetter 1973). The issue here is not intellectual snobbishness, but simply the confusion generated by the fact that ANT is not about these phenomena. Instead, in ANT parlance, a network denotes *any* set of dynamic relations between heterogeneous actants – regardless of whether the resulting pattern *looks* like a star-shaped technological network, a nation-state, an individual, a whale, an atom, or whatever. Networks, in this line of thinking, are the stuff of variable social ontologies, much like the rhizomes of Gilles Deleuze (Deleuze & Guattari 1987). Indeed, Latour (1999a) agrees with his science studies colleague, ethnomethodologist Mike Lynch, that ‘actor-network theory’ really should have been named ‘actant-rhizome ontology’.[34]

At first glance, the implications of this strategy are likely to seem absurd: if we are to be this liberal in our network conceptualization – including atoms and whales alongside individuals and nation-states – then how on earth are we to demarcate anything like ‘the social’, presumably the domain of sociologists (like me)? This, indeed, gets us to the crux of Latourian sociology, and the reason he distinguishes his own ‘sociology of associations’ (or networks) from what he terms ‘sociology of the social’ (Latour 2005). Sociology of the social, Latour wants us to realize, takes the social for granted as always-already established – as families, groups, nation-states, capitalism, social fields, autopoietic sub-systems, Empire etc., depending on ones social theory. In his sociology of associations, by contrast, the social is what emerges when *new associations* are established between heterogeneous actants that, until then, remain non-social. Whales by themselves are hardly ‘social’ in any sociologically relevant sense; connect them up to Japanese biologists, Greenpeace activists, and international science-politics conflicts, however, and it quickly becomes artificial to deny their participation in global social life.[35]

In the example just provided, exactly who or what is doing the connecting, the associating of different actants within a network of relations – in this case,

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34. Which is also why, six years later (Latour 2005), Latour is now ready to defend each and every term in his actor-network theory – including the hyphen. Naming, of course, is not the real issue here.
35. This, as the reader has no doubt guessed, is the theoretical plot of essay number 2 in this thesis.

that of ‘scientific whaling’? This is a tricky question, and it speaks to Latour’s reasons for being self-critical about the hyphen in his ‘actor-network’. The hyphen, as he remarks (1999a), invites sociologists to think that ANT is yet another go at the agency-structure debate, as if network would be the ‘structure’ constraining (and/or enabling – it hardly matters here[36]), the ‘agency’ of actors ‘within it’. This, however, would be a misunderstanding of ANT social ontology (see Elder-Vass 2008). Instead, with Latour, *any* social entity relevant to an inquiry is a completely specific actant, with specific effects on other actants, regardless of size, figurative shape, material make-up etc. Actants are material-semiotic entities – human or non-human, small or big – reshaping, modifying, translating and mediating other actants. Actants act; if they do not, they are not actants.[37] ‘Follow the actor’ means to follow this dynamic assembling and reassembling of life together.

What then, is a network? A network is the (potentially unending) *trajectory* drawn up by the mutual transformations exerted between actants interacting in dynamic time-space; a simple conceptual placeholder for a radically relational, processual and materially situated view of the gradual unfolding of social life (see Schinkel 2007; Krarup & Blok 2009; Savage 2009). Once again, ANT is more methodology than theory: *before* doing the actual work of empirical inquiry into some specific set of social trajectories, there is really very little the sociologist can say interestingly about the social world – beyond well-established commonplaces, tropes and clichés, that is. A network is not a property of the world, but simply a tool for networking. Networks are what inquirers equipped with ANT may draw up in their analyses, provided they are capable of following and mapping traces between heterogeneous actants, continuously shifting scales of time, space and agency in order to keep up with

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36. I am glossing Giddens’ well-known definition of ‘structure’ as what constraints and enables social action – part of his structuration theory (1984). Giddens’ is one of a string of dialectical agency-structure bridge-building efforts in contemporary sociology; Bourdieu’s habitus-field double is another (1988).

37. This non-dualistic social ontology is where theorists inspired for instance by critical realism take issue with Latour, arguing that ANT looses sight of latent and/or invisible social structures and their enduring social power-effects (e.g. Elder-Vass 2008). What this criticism overlooks, however, is that *anything* can be a Latourian actant – including much of what is usually taken to make up ‘structures’ in sociology (say, a nation-state bureaucracy). What Latour demands, however, is for the sociologist to document concretely the social *effects* of the actants invoked in an explanation, in order to guard against conceptual reifications.

the actors' world-building efforts. To work with ANT is to actor-network the world; which means that this thesis is primarily about actor-networking the global environment and exploring the effects such work might have.

This notion of scale-shift, or scaling, is crucial: unlike standard sociology, which routinely distinguishes between 'micro' and 'macro' theories, ANT refuses to know in advance what the relevant spatial-temporal dimensions of inquiry will be (see Bruun Jensen 2007). Rather than treating micro and macro as self-evident scales of social life – so that, say, families are micro and nation-states macro – ANT contends that any scale is the dynamic product of on-going scale-making exercises, spatial and temporal. Scale, in other words, is what actors do when scaling, framing and contextualizing each other (Latour 2005). Hence, although Latour prefers localized ethnographic methods, and claims to draw theoretical inspiration from both Goffman and Garfinkel (Latour 1996b), he would deny the standard label of micro-sociology to ANT. Indeed, he would deny the very distinction between micro- and macro-sociology.[38]

Latour himself (2005) goes so far as to say that overcoming, or bypassing, the entire micro-macro problem is the central contribution of ANT to general social theory. As is often the case with Latourian sociology, his preferred means of bypassing seemingly entrenched theoretical dichotomies – agency/structure, micro/macro, culture/nature, subject/object – consists in denying their very status as philosophical resources, turning them instead into topics for empirical inquiry, or rather, for empirical philosophy. In a certain sense, we may thus say that Latour has generalized Garfinkel's ethnomethodological insights by applying them to what it means to have a 'global', 'total', or 'universal' entity – the most obvious example being the scientific fact (see Law & Mol 2001). Unlike the 'regional' notions often found in sociology – of communities, cities, nations, international relations etc. – networks, Latour stresses (1997), "are by nature neither local nor global". Localizing ('making small') and globalizing ('making total') are dynamic processes of social life, often intermingling rather chaotically. When talking about climate change, for instance, actors will routinely jump, from one instant to the next, between the fate of future humanity and the prospects of the next local elections.

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38. With Michel Callon (Callon & Latour 1983), Latour did so already in the early 1980s, in a volume dedicated to 'integrating' micro and macro in social theory.

One possible way of summarising this small introduction to actor-network theory, then, is to say that Latour partakes in a more encompassing topographical, or topological, turn in socio-cultural theory (see Bingham & Thrift 2000; Hastrup 2005; Murdoch 2006). The topographical turn, as it were, is what happens once linguistic-literary deconstruction stops being a convincing form of social analysis, and the search sets in for a renewed sense of *realistic navigation* in a complex, border-crossing and hybridizing world of movement, fluidity and flux (see Latour 1993; Urry 2007). Topography, in this context, designates a sustained interest in the specificities of the social landscape, including both human and material components, as it maintains or changes its socio-spatial shapes and features over time. ANT, we might conclude, is a topological meta-theory for sociology, allowing for the emergence of variable topologies of social life (see Mol & Law 1994). As we shall see, this reconfiguration of meta-theory also necessitates new concepts of sociological method, suited to a world conceived in actor-network terms.

First, let me illustrate more concretely what a Latourian approach to scientific knowledge in global environmental conflicts may entail, by way of the complex example of climate change. The following is not meant as a sociological 'analysis' of climate change – this is the task of essays 5 and 6 in this thesis – but simply as a way of specifying a few more key ANT concepts, by way of an illustration. Following this illustration, I engage in a sympathetic criticism of Latourian sociology more generally, focusing on issues of description, generalization and abstraction.

The climate science-society topography: an ANT travel guide

In the opening paragraph of *We have never been modern* (1993), Latour uses his morning newspaper to trace the making of the Antarctic ozone hole, from corporate boardrooms to atmospheric chemistry – or, as he neatly summarises, from chemical reactions to political reactions. Like any other hybrid, ecological or otherwise, the ozone hole is simultaneously *real* (like nature), *narrative* (like discourse), and *collective* (like society). If Latour was to rewrite his magnum opus today, he would no doubt substitute climate change for the ozone hole – but the analytical point would be similar. Global warming, Latour notes (2003:39), has emerged as the ultimate learning experience of a really *explicit* mix of cosmos and society, an explicit mixing of the real, the narrative, and the collective. Like our pre-modern ancestors, we moderns once again worry that the sky may be falling on our heads! And while no revolutionary committed to destroying capitalist modes of production ever thought of redesigning the Earth's climate, this is what is already happening clandestinely – and will happen knowingly in the future (Latour 2007a).

In response to this changing environmental discourse, sociologists and others are now studying the heated risk definition battles played out over climate in-between worlds of science, politics, and mass media – with particular attention to so-called 'sceptics', counter-experts questioning the reality of anthropogenic warming particularly in the US context (see Weingart et al. 2008; Demeritt 2006; Lahsen 2005).[39] Beyond this new empirical domain, however, climate change may well be analytically subsumed as just another manufactured uncertainty in world risk society – with nothing much new for sociology, except perhaps the question of just how alarmed we should be (see Lever-Tracy 2008; Brechin 2008). Alternatively, some sociologists prefer to undertake a futuristic turn, by engaging in the building of future climate-society scenarios – raising the prospects of mass refugee movements, civil wars spurred by resource scarcities, and a panoptic encroachment on liberal Euro-American consumer rights (see Urry 2008).

From a Latourian vantage point, by contrast, our sociological journey into the climate world starts elsewhere: it starts with the simple observation that, were it not for a handful of complex scientific computer simulation of climatic variations over a hundred-year timeframe, we simply would not *know* that we currently experience *global* warming (see Taylor 1997; Yearley 2009).[40] Beck, undoubtedly, would agree with this observation on the science-dependency of climatic risks; he might quickly proceed, however, to provide an overview of its implications for 'everyday life' and 'all of world society' (see Beck 1992;

39. Outside of the US, climate scepticism has a particularly interesting recent history in Denmark, home country of internationally renowned climatic counter-experts. The case of Lomborg and his anti-environmentalism is already well analysed in environmental sociology (see, e.g., Jamison 2004).
40. This wording entails no denigration of the importance of non-scientific and/or experiential knowledges in mediating human-environment relations. It simply plays on the observation that, outside of techno-scientific mediation, we have no means of experiencing global environmental variations (see Ingold 1993).

2002). ANT, by contrast, moves much slower. As a science-society-nature hybrid, the way climate change spreads through and modifies social life is heavily dependent on the techno-scientific infrastructures gradually built up to support it. At the same time, these scientific infrastructures are densely interwoven with economic, political and cultural concerns. Hence, to paraphrase a quote already introduced, when doing environmental sociology of climate change you simply need to do the sociology of science.

Unlike widespread views of the solidified natural facts of laboratory science, climate change is most commonly perceived as a multifaceted, risky and fluid reality. In the vocabulary already introduced, it is more matter-of-concern than matter-of-fact – matter-of-concern being simply another word for hybrid or quasi-object. Nevertheless, it is not entirely impossible to apply standard ANT concepts from the sociology of natural science to understand the gradual emergence of this climate change hybrid. Hence, it is primarily produced in scientific communities of climatic modellers using highly complex computer simulations to predict future climatic variations at the global scale (see Miller & Edwards 2001). These communities and their models, we might say with ANT, serve as 'obligatory passage points' for negotiating the scientific credibility of climatic predictions. Since the late 1980s, these negotiations have been institutionalized in the Intergovernmental Panel on Climate Change (IPCC), acting as powerful node in the climatic network – or what Latour (2005) nowadays call an 'oligopticon'.[41]

In this process, the IPCC has emerged as a powerful 'hybrid organisation', mediating concerns of global-scale science and politics (Miller 2001; 2004; 2007). Scientists within the IPCC are engaged in what we might call 'ontological kind-making': in historical perspective, the emergence of the global climate system represents an important instance of the co-construction of science, society and nature (Jasanoff 2004). Prior to the 1970s, there were hardly any references to this quasi-object, at least outside of highly specialised circles of atmospheric chemistry. The term 'climate' used to refer simply to particular weather patterns – tropical, desert, mountain, tundra – of particular places; today, the UN Framework Convention of Climate Change (FCCC) defines the 'climate system' as "the totality of the atmosphere, hydrosphere,

41. In his earlier ethnographies of science, Latour would speak of a 'centre of calculation' (Latour 1987a).

biosphere, and geosphere and their interactions" (quoted in Miller & Edwards 2001:7). Entire new global sciences – climate change science, Earth system science, sustainability science – have emerged, all participating in the gradual entrenchment of the climate system as an important social and political concern (see Lövbrand, Stripple & Wiman 2009).

In ANT perspective, this process of ontological kind making relies first and foremost on a dispersed set of scientific practices, instruments and discourses, by which scientists attempt to mobilize public and political credibility for their claims. Latourian sociology of science is distinct in this respect, in that it focuses attention primarily on the material aspects of scientific practice: by way of temperature measuring devices, computer databases, model simulations, ice core drillings and so on, scientists have managed to gradually stabilize – or 'black-box' – certain core statements about anthropogenic climatic changes. The practices of black boxing relies on scientific 'inscription devices', from textual accounts to graphs, figures, numbers and projections, capable of travelling the world without too much deformation, as what Latour calls 'immutable mobiles' (1990). Climatic scientists all around the world coordinate their activities in reference to a limited number of such inscriptions – notably the Assessment Reports on Climate Change prepared and released every four-five years by the IPCC. By the 2007 Assessment Report, anthropogenic global warming had become something like a socially established scientific fact, and much talked about by politicians, journalists, business leaders, environmental NGOs, pop celebrities and families debating their next trip to Thailand.

Like other scientific facts, anthropogenic global warming has a very specific history of emergence, in the course of which it has gradually forced or provoked an ever-expanding range of redefinitions of what we usually mean, in everyday life, by terms such as society, nature, politics, responsibility, morality, the global, and so on. To mirror Latour's point about pasteurization, we seem to have witnessed the 'climatization of society', to the point of making every innocent conversation about local weather vulnerable to political concerns of global proportions! In the process, climate scientists have lost control of their hybrid creation: the nature and reality of climatic changes is nowadays routinely negotiated across a diversity of social territories, from mass media to science fiction movies, from art exhibitions to political talk-shows, and from corporate boardrooms to family dinner tables all around the world. Climate change has become a massively pervasive, shape shifting, fluid matter-of-concern – and a major part of what it might mean, these days, to think of oneself as a 'citizen of the world'.

What ANT invites us to do, then, is to trace the network trajectory of emerging quasi-objects, from the first esoteric scientific paper to the present state of high-level global politics. Depending on the specific trajectory under study, such tracing may involve more or less historical work, weaker or stronger levels of scientific controversy, shorter or longer extensions into everyday worlds of business, politics, mass media, and family houses. In the case of climate change, scientific mobilizations of the world have been hugely successful: anthropogenic global warming is nowadays a ubiquitous social reality, with major implications for political debate, moral understandings of relations to nature, and social identity-making more generally. Controversies continue, however, at all levels, between scientific communities, and between 'ethno-epistemic assemblages' of scientists, bureaucrats, environmental NGOs and citizens with different visions of socio-natural futures (see Irwin & Michael 2003). In the process, we might add, climate change continues to thoroughly by-pass established boundaries between science, society and nature, quickly turning co-constructivist insights into everyday common sense.

Towards a sympathetic criticism of ANT descriptivism

Enough has been said about Latourian actor-networks to suggest that this collective body of theorizing – developed in conversation with Michel Callon and John Law – is itself something of a hybrid sociology (see van Krieken 2002; Hall 1999:15). Incidentally, Latour is fond of summarizing his ANT as 'half Garfinkel and half Greimas' (2005:54f, note 54), pitting these twin influences as "two of the most interesting intellectual movements on both sides of the Atlantic".[42] Unpacking the hybrid, we note how Latour gets his key concept of 'actants' from Greimas' semiotics, allowing him to seriously expand

42. It should be noted that this way of recounting the genealogy of Latour's thinking applies mostly to his sociology, and less so to the rest of his intellectual oeuvre. If one were to establish a more 'philosophical' genealogy of Latourian metaphysics, it would include references to such characters as Thomas Hobbes, Alfred N. Whitehead, Michel Serres, Gilles Deleuze, Isabelle Stengers, and many others (see Harman 2009).

standard understandings of action in sociology, by conceptualizing social actors as *anything* exerting effects on other actants in a set of ordered events (see Latour 1996b). More generally, as John Law argues (1999; 2004), ANT may be understood as a 'ruthless application of semiotics', in which *any* entity take its shape and acquire its attributes as a result of shifting relations to other entities. ANT, then, may well be the first social theory build from 'material semiotics'. Material semiotics is another way of saying that ANT takes a material turn beyond the realism-constructivism conundrum, by dramatically expanding what is to be considered *real* in sociological analysis.

Moving on to Garfinkel, the other half of the Latourian sociological hybrid, things become more complicated. On one level, the entire methodological ANT slogan of 'following the actor' may legitimately be read in light of Garfinkel's close attention to the ethno-methods of social actors in practically assembling situated social orders. Like Garfinkel, Latour is interested in the relation between practices and accounts of those practices, with his sociology focusing on how things get done by members of particular settings using the material and semiotic resources available at hand (Laurier 2003). Latour is very explicit on the radical meta-theoretical implications of his strict adherence to ethnomethodological prescriptions, to the point of stating (2005:32) that "actors do the sociology for the sociologists and sociologists learn from the actors what makes up their set of associations". In the world of ANT, nothing is beyond practice; "as Garfinkel has taught us: it's practice all the way down" (Latour 2005:135).

Such a practical-material starting point is easy enough to formulate in the abstract, connoting well-respected sociological traditions of not just ethnomethodology, but also the symbolic interactionism of Goffman, Blumer and Becker (see Latour 1996b; Becker 2007). Much like his American sociological predecessors, Latour is interested in empirically studying the performed details and the constitutive requirements, as well as the habits and routines serving to embody and stabilize, a plurality of situated social practices (see Rawls 2009).[43] Unlike the more language-centric strands of European

43. From the perspective of the history and sociology of ideas, seeing Latour's pragmatist allegiances, it is important to note the impact of American pragmatist philosophy on generations of American sociologists, particularly in and around the University of Chicago

practice theory influenced by Wittgensteinian ordinary language philosophy (see Schatzki 2001), however, Latour's approach to scientific and other social practices stresses the importance of embodied and materialized activities. Latourian sociology, we might say, is in part a materialized symbolic interactionism (see Latour 1996b).

Just as, in Neil Gross' estimation (2007:219), "the vast majority of sociologists" continue to see "ethnomethodology as tangential to sociology's true concerns", Latour may well run the risk of condemning his ANT to a similarly sad fate, given that he aligns his methodology closely to Garfinkel's prescriptions (see also Rawls 2009). In particular, Latour proudly adopts one of the cornerstones of ethnomethodological thinking, namely the requirement for the analyst to be able to produce, for each given situational order at hand, what Garfinkel termed a 'uniquely adequate' account (Garfinkel 1967; Latour 2005). Put briefly, what this means is that an investigator should ideally immerse her- or himself in the situation under study, learning to become a competent practitioner of the everyday mundane activities of the particular field. In this way, the investigator should become capable of giving an account of these activities that does not differ *in kind* from the 'insider' knowledge provided by the setting's participants. In the language of Latour (2005:144), this entails being "attentive to the concrete state of affairs".

So far, the reader may be forgiven for missing some of the 'radical' implications of this Garfinkel-Latour meta-theoretical hybrid. Describing concrete states of affairs, after all, is a common goal in any qualitative and interpretive sociology. In methodological discussion on ethnographic sociology, ethnomethodology is thus sometimes aligned to, or rather conflated with, seemingly associated notions of 'grounded theory' (e.g. Glaser & Strauss 1967), 'analytic induction' (e.g. Becker 1963), 'sensitizing concepts' (e.g. Blumer 1954), and, most commonly, 'micro-sociology' (see Have 2004 for a critique of common misunderstandings). What these conflations miss, however, is that ethnomethodology, in its strict sense, entails a form of social science empiricism and descriptivism much closer, if this is indeed possible, to natural science practice (see Hilbert 1990). This is why Latour, as already noted, openly embraces both of these otherwise much-maligned terms: "no scholar

from the 1920s onwards, and how this has influenced studies of everyday practical knowledge, including Garfinkel's ethnomethodology (see Gross 2007).

should find humiliating the task of sticking to description. This is, on the contrary, the highest and rarest achievement" (Latour 2005:136f).

Like other contemporary sociologists, including Andrew Abbott, Latour is thus defending a distinctly descriptive approach to sociological method (see Savage 2009). According to Abbott (1998:173), "one central reason for sociology's disappearance from the public mind has been our contempt for description". Description, he suggests, has long been subordinated in sociology to explanatory efforts, particularly of a causal-statistical kind. While this tendency may have been less marked in Europe than in Abbott's native America, here as well description is, as Latour points out (2005:137), still usually considered a stepping stone to the *real* goal of explanation. More radically than Abbott, however, and reflecting his ethnomethodological inclinations, Latour explicitly *rejects* this entire distinction. Adding 'social explanations' to descriptions, Latour warns, makes sociology flow much too easily; instead, description should always simply be extended one step further. In short, 'get back to empiricism' and 'just go on describing' seems to be Latour's new master-rules of sociological method (Krarup & Blok 2009).

Against this background, it comes as little surprise that Latour prefers sociologies of 'small histories' to those of grand abstractions (see Latour 1996a); nor that his work on the transportation system *Aramis* adopts the stylistic genre of a detective story (see also Austrin & Farnsworth 2005). Narration, as Abbott points out (2004:33), is the syntax of common sense explanation, and hence an important shape in which to situate sociological descriptions in dynamic time-space. Familiar everyday contexts also always constituted the starting point of Garfinkel's ethnomethodology (see Bertilsson 2004:378); and Latour arguably inherits his ambition of carefully elucidating a plurality of inhabited worlds. There are differences between the two, however, first in their respective contexts of description – Latour devotes more attention to the workings of scientific laboratories than did Garfinkel.[44] More importantly, they differ in approach when it comes to making social codes visible: whereas Garfinkel used breaching experiments in order to elucidate otherwise hidden social conventions, the Latourian dictum to 'follow the actors'

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44. In the late 1970s, Garfinkel did in fact study natural scientific laboratory practices, together with Mike Lynch who later became an important figure in the constitution of science studies as an academic field.

suggests that social codes are there to be observed on the surface. In ANT, this surface is itself conceived as a topological multi-dimensional space (see Adkins & Lury 2009).

One important manifestation of this descriptivist inclination to stay on the surface is the frequency with which Latour, and other ANT scholars, invokes concepts of observing, tracing and mapping to pinpoint the goal of sociological inquiry (see Latour 2005; Callon 1998). Mapping, as Marilyn Strathern notes (2004:xvii), has traditionally been a powerful image for the analytical exercise, suggesting some kind of correspondence to, or representation of, different cultures and societies. Maps imply a certain consistency, in the existence of cartographic points or areas that remain identifiable whatever the perspective of the observer. Conceived as tools of spatial ordering (de Certeau 1984), maps imply generalizations in the shape of configuring localities, regions, societies, or even the world, into some limited symbolic system. At first sight, then, ANT invocations of mapping as an analytical goal may seem strangely old-fashioned and counter-productive, given this method's explicit interest in the shifting contours of social territories.

What this analysis misses, however, is the way in which ANT helps us rethink the entire relationship between maps, territories, and contemporary social navigation. Saying that ANT is about mapping dynamic social relations tells us nothing about the *shapes* that are to be drawn using ANT as a method – given that, indeed, the social no longer comes in the shape of 'societies' (Urry 2000b). Actor-networks, Latour makes clear, may come in *any* imaginable topographical shape, suggesting that his is really a project of *re-mapping*, or re-imagining, a plurality of social world-making projects (see Tsing 2008). The extent to which socio-material relations in dynamic time-space may be analytically stabilized and abstracted in particular topological shapes, we may note, is one of the major discussion points in recent 'post-ANT' theorizing (see Gad & Jensen 2009). More specifically, John Law and Annemarie Mol (1994; 2001) have suggested distinguishing between four main types of socio-topological space: regions, networks, fluids, and fires. We will return to these concepts, and what I take to be their productive implications for sociological inquiry into global social relations, at various points in my six essays.[45]

45. More specifically, these topological concepts will show up in essays 3, 4 and 6, where they will be used to different analytical effects. Article 3, it should be noted, is explicitly

Secondly, from an ANT point of view, maps must necessarily be understood as themselves part of the social territory, not as standing outside or above it. Sociological maps may be understood as socially performative panoramas, employing narrative and visual devices in providing tools for social actors to navigate a changing landscape of socio-material relations (see Law & Urry 2004). More precisely, maps are more-or-less stabilized inscriptions, capable of some level of compatibility, standardization and circulation (see Latour 1999b:71). As Latour points out in his essay on 'circulating reference' (1999b), scientific facts are constituted in chains of transformation, on the one hand gradually loosing particularity and multiplicity, while on the other allowing for increasing textual stabilization and amplification of detail. In principle, there is no reason why social scientific maps should not be able to achieve a similar amplification effect, provided we can trace the relevant connection between individual variations and collective aggregates, by way of citations in science, for instance (see Latour 2003; 2009). ANT is indeed fully compatible with quantification, as soon as we know *what* to quantify.[46]

At times, however, Latour seems to be suggesting that each new sociological map must be redrawn from scratch, invoking once again the ethnomethodological dictum of unique adequacy. For instance, he goes so far as to suggest that ANT must "be remade for each new case at hand" (2005:121); and that sociology must produce singular explanations for singular, unique cases (Latour 1996a:131). This highly particularizing stance also explains why Latour prefers to talk about ANT in terms of method rather than theory, and why, if pushed on this point, he talks about the empty 'negative' infra-language of ANT, by contrast to the 'positive' meta-language of other, stronger, and more generalizing social theories. ANT has no room for "all-terrain entities like Society, Capitalism, Empire, Norms, Individualism, Fields, and so on" (Latour 2005:137); terms that would potentially make the analyst loose touch with situated, embodied and material social orders. True to his empiricism, Latour

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related to the topic of mapping, suggesting an ANT-inspired approach to global ethnography that I term 'ethno-socio-cartography'.

46. According to Latour (2009), digital technologies like the Internet will most likely revolutionize the idea of quantification in social science. Gradually, as more and more naturally occurring traces of opinions, affiliations and consumption patterns will be digitally available, the idea of sending thousands of questionnaires to isolated individuals in order to obtain a view of 'the whole society' will come to seem obsolete.

claims that there is science only in the particular; and that it is the very character of the social to be specific, to require more details.

At this juncture, it is difficult to follow the reasoning of Latour who, so it seems to me, is now treating the natural and the social sciences too asymmetrically. Whereas his sociology of science has no problem in allowing for amplification, re-contextualization and abstractions in the natural sciences (Latour 1990), Latour is curiously ambivalent when it comes to the social sciences. While admitting to the empirical traceability of social aggregates, Latour seems to lack a convincing self-reflexive vocabulary for talking about processes of abstraction and re-contextualization in sociological work. To Latour, all previous sociological theory seems to do is to perform the same boring work of 'horizontal' generalization, lumping heterogeneous social entities together in falsely homogenized registers. Sticking to their fixed frame of reference (of Individuals, Society, and so on), sociologists are 'pre-relativist'; when in fact they ought to be fully 'relativist', in deploying the many world-making activities of actors in just as many projection grids.[47]

This point, while cast in too high-strung Einsteinian rhetoric, seems to me perfectly warranted: sociology, alas, is a discipline of too many generalizations, usually performed in the name of theoretical free flowing rather than sustained observation. There is no better (or worse!) indication of this tendency than the never-ending flood of *Zeitdiagnosen* subsuming entire societal transformations under a single conceptual heading – risk society being one such term, albeit a productive one. Where Latour seems off the mark, I would argue, is in assuming that *all* conceptual work in sociology must entail horizontal generalization, thus failing to account for what we might term higher-level abstractions in social science (see Holbraad & Petersen 2009). Unlike his source of inspiration Gilles Deleuze, Latour thus fails to provide an adequate concept of the concept, including an account of the work done by concepts as the 'black-boxes' of social science (see Krarup & Blok 2009). Concepts are what allow for some compatibility in social science – like other symbols, they translate singular speech-acts into public talk (Bertilsson 2009).

If concepts are indeed social science learning devices, forcing us to confront in creative ways the difficulties of presenting empirical data with much clarity,

47. Latour defines 'relativism', we may note, in accordance with Gilles Deleuze: "relativism is not the relativity of truth, but the truth of relation" (Latour 2005:95, note 119).

then the task of concept creation must be one of the most important sociological skills (Gane 2009). In recent years, there seems to have been a striking lack of inventive new concepts forged from within sociology, with most new concepts drawn instead from the writings of philosophers, literary critics and so on (see Beck 2005). Bruno Latour, in this context, must surely be regarded a sociological innovator, his work brimming with conceptual reconstructions, from hybrids, actants and quasi-objects to oligoptica, panoramas and immutable mobiles. Paradoxically, however, whereas all of these Latourian concepts are clearly important for his new topological socio-ontology, his own Garfinkel-inspired meta-theory remains incapable of articulating this importance in adequate ways. The same seems true, we may note, of the topologies of region, network, fluid and fire set forth by Law and Mol, whose conceptual status remains underspecified (see Cooper 2009).

Latour's reluctance to elaborate a theory of social science concepts may be understandable when read in the context of prior attempts – from Weber's ideal types to Blumer's sensitizing concepts – articulated within a neo-Kantian scenography of poor subjects condemned to acts of reductionism in the face of an overwhelming objective reality. If anything, Latour wants to leave this scenography behind, moving sociology closer to ideals of 'thick description' in a symmetrical anthropology of the moderns (Latour 1993). Even so, he does not exhaust his available options. Latour might, for instance, take a second look at his French neo-pragmatist colleagues, Luc Boltanski and Laurent Thévenot, in terms of how they have reconstructed the grammars of justification in everyday social life (Boltanski & Thévenot 2006).[48] The important methodological point is that Boltanski and Thévenot turn the very work of abstraction – of moving in-between the particular and the general – into a key topic of empirical social inquiry, thus grounding, we might say, whatever relative transcendence actors may achieve in the immanence of social practice. Their theory, then, is a theory of ethno-abstractions – a topological theory of legitimate moral abstractions in contemporary life (see Krarup & Blok 2009).

48. I say *second* look because, already, Latour makes frequent reference to Boltanski and Thévenot, quite obviously holding their 'sociology of critical capacity' (Boltanski & Thévenot 1999) to be amongst the most interesting developments in social theory over the past 20-30 years (see Latour 1998).

The reason for pointing to the sociology of Boltanski and Thévenot in this context is simply to make the more general point that conceptual work, including the work of forgetting or amplifying details in moving towards relative generalities, is as integral to everyday social life as it is to the practice of social science.[49] Furthermore, this need not entail any commitment to deductive uses of social theory. Rather, social science inquiry, much like other social practices (including detective work) relies on a variety of creative *abductions*, in which we form situational judgments as to ‘what is the case’ and ‘why something is the way it is’ (see Bertilsson 2004). Abduction, then, is that pivotal moment of inquiry, everyday or social scientific, when concepts, observations and practices meet, forming new knowledges that may or may not assume roles in future social conversations. In this sense, Latour is a skilled practitioner of creative sociological abductions, calling on us to “practice sociology in such as way that the ingredients making up the collective are regularly refreshed” (2005:261). What he fails to make explicit, however, is that in order to do so we need to know roughly what those ingredients might be.

Much like grammariticians, pragmatist sociology thus ought to involve the careful documenting, articulating, amplifying and comparing of the constitutive grammars of everyday life, in as much analytical detail as possible, and across an expanding plurality of different cultural life-forms (see Rawls 2009). At this point in his career, however, and outside the realm of natural scientific practice, Latour is only gradually starting to articulate positively the various grammars of inter-subjective social life.[50] One practical consequence of this, at the level of my own thesis, is that at various points throughout my essays, it has been analytically enriching to supplement the vocabularies of (post)-ANT with what

49. This is the point made beautifully by Jorge Luis Borges in his short fictional story about *Funes, the Memorious* (1993 [1942]), a man capable of remembering every single minute detail of every single split-second sensorial experience he ever encounters, but sadly incapable of any type of forgetting, and thus also of any type of abstraction or generalization (a distinction, incidentally, also made by Borges!).
50. In recent years, Latour has thus taken his ethnographic gaze into, for instance, the realms of law making (2010), politics (2007b), and religion (2002b), considering each as historically contingent regimes of enunciation, existence and truth-production. It remains to be seen, however, just what kind of overall sociology of hybrid a-modernity this Latourian re-description is going to amount to. In his own words (2008b), Latour is currently finalizing this long-term project of his, making him, as he says, the first philosopher to have worked simultaneously on the first and the second version of himself!

I take to be congenial sociological concepts stemming from kindred intellectual traditions. Most importantly, in analyzing the empirical dynamics of conflicting collective identities around whales and climate change (in articles 1 and 5), I employ analytical languages stemming from traditions of symbolic interactionism, on the one hand, and theories of political engagement, on the other. Incidentally, as concerns the analytics of politics, Latour still gets the final word, in that article 4 re-interprets whaling conflicts in the Latourian language of 'cosmopolitics' (Latour 2004c).

One way of summarizing this discussion on Latourian descriptivism might be to say that his new empiricist sociology succeeds, in my estimation, at the level of its practice, despite its inadequate meta-theoretical self-conceptualization! What is lacking, I argue, is primarily a more elaborate theory of the kind of creative conceptual work in the social sciences that Latour himself exemplifies, including the way concepts may come to serve as the equivalents of stabilized inscriptions in the natural sciences. While I agree with Latour in his reluctance towards horizontal generalizations, we still need to scale instances of sociological knowledge between the particular and the general. Abstraction is what enables our knowledge of particulars to be gradually compared, allowing us to search for *patterns* of social relations across dynamic time-spaces. The pragmatist approach to social science abstraction is not whether we need it or not, but rather what *kind* of conceptual abstractions best furthers work of refreshing the ingredients of social life. Despite my methodological hesitations with ANT, the overall claim in this thesis, to reiterate, is that Latour's conceptual abstractions still do a good job of enabling such a productive renewal of sociology's understanding of the global environment.

Methods: global-scale ethnographic case studies

Leaving meta-theory aside for the moment, what then are the main implications of ANT for doing social inquiry, in terms of the specific methods of bounding, researching, analysing, and writing up reports about particular empirical problems? As noted several times, Latour himself prefers to talk about ANT in the language of methods rather than theory; unfortunately, however, this does not entail that discussions of method will be any easier than discussions of meta-theory. Method, in the sense of the word that ANT entails, is a *very* broad term, basically connoting, as in the Greek etymology of the word, a sense of 'where to travel' and 'what to see there' (see Latour 2005:17). As indicated in my sociological settlement (figure 2), methods – much like meta-theory – thus encompasses hard questions of social ontology, when leaning towards the methodology pole. Unlike meta-theory, however, on the epistemology axis, our methods simultaneously needs to tell us 'what to see', that is, what kind of data to 'gather' (or generate) about social realities. This section, then, marks the beginning of me speaking seriously about data.

As already noted, I conceive of the empirical endeavour of this thesis in the language of ethnographic case studies – of which, to reiterate, I have conducted two, one on conflicts surrounding Japanese whaling (and biodiversity), another on conflicts surrounding carbon markets (and climate change). This sentence already contains many clues as to the implications of ANT at the level of methods. First of all, my invocation of ethnography, broadly construed, can be said to mirror the trademark strategy of ANT scholars of using ethnographic research methods in the study of scientific conduct (Latour & Woolgar 1979). While ANT is fully compatible with certain forms of quantitative methods, its stressing a situational, relational and dynamic view of social life nevertheless favours less-formalized, more adaptable, more flexible research practices, associated with 'doing fieldwork' (see Whatmore 2003). At the same time, what it means to 'do fieldwork' must itself be reconfigured from an ANT point of view.

The first aspect of my methods that needs further specification in this respect has to do with the notion of case studies. Again, as with ethnography, my invocation of this particular strategy of sociological knowledge production is far from coincidental, and there are at least two relevant intellectual genealogies here. First, in the context of 'general' sociological methodology, case-oriented approaches, typically of a qualitative kind, may be contrasted to variable-oriented quantitative approaches, each with their attendant set of discussions, strengths and weaknesses (see Ragin 1992). Second, in the context of the 'new' sociology of science (STS), case studies have formed an important part of this epistemic landscape since the 1970s (see Beaulieu et al. 2007). The empirical work of Latour, for instance, consists mainly of a wide range of case studies.

In my estimation, although this varies between sub-fields, sociological interest in case study methods has generally been on the ascendancy over the past 20 or so years (see Abbott 1992; Yin 1994; George & Bennett 2005; Flyvbjerg 2006). If we provisionally define case study as "an empirical enquiry that investigates a contemporary phenomena within its real life context especially when the boundaries between phenomenon and context are not clearly evident" (Yin 1994:13), it will be clear that this notion is not without its own ambiguities, however. On the one hand, case studies imply particularity and real-life situated-ness; on the other, case studies invest the social setting under study with some sense of generality, of representing some abstract-able categories of the social world. How to move from the particular to the general – or from 'case' to 'context' – is subsequently *the* major question in case study research (see Ragin 1992; Burke 2002). In other words, the sociologist must keep asking 'what is this a case of'?

This question raises some very tricky issues of abstraction and comparison reminiscent of the ones discussed before under the label of unique adequacy. I will come back to these issues in more detail; for the moment, the main reason to bring it up is to point to a third particularity of ANT methods, complementing an adherence to ethnography and case studies. As should be clear, ANT scholars tend to take a particular interest in techno-scientific complexities, that is, in the unpredictable trajectories of nature-culture hybrids. Peculiar to this approach is the further methodological dictum that studying quasi-object trajectories must be done, so to speak, in real time – while uncertainties proliferate, groups and beliefs conflict, and no one has the answer. ANT, in short, tends to study on-going controversies, or what Boltanski and Thévenot (2006) call 'critical situations', because these situations force actors to render their social worlds and values explicit and observable.[51] In the most abstract sense, then, and similar to other work in STS, ANT case studies – including my own – are case studies in the dynamic co-evolution of science, society, and the environment (see Jasanoff 2004). This, indeed, is the main reason this thesis is entitled 'divided socio-natures'.

51. This commitment entails no 'presentism', since on-going controversies may well be studied historically, as long as the analyst endeavours not to tell the story from the point of view of the eventual winner (a version of 'whig history'). Latour's study of Pasteur (1988) exemplifies the historical application of ANT.

Whereas ANT thus provides important rules of thumb when it comes to selecting and framing particular phenomena for study, there is surprisingly little in the way of more specific prescriptions on the conduct of ethnography to be found in Latour's sociology.[52] What at first seems an ill-founded neglect on the part of ANT may however, on closer inspection, come to be viewed instead as a principled eclecticism of data and data-generating devices, subsumed under the heading of inquiry. Latour sums up this principled eclecticism of methods in his (fictional) conversation with a frustrated organization studies PhD at the London School of Economics: "Enquiries, survey, fieldwork, archives, polls, whatever – we go, we listen, we learn, we practice, we become competent, we change our views". As Latour himself points out, this view of empirical work is really very simple, and it ties into his descriptivist approach to sociology: good inquiry always produces lots of new descriptions of a specific phenomena. In this respect, questions of what *exact* kind of data to generate, and how *exactly* to go about it, recedes into the background, subsumed under the pragmatist label of inquiry, as the gradual finding out of some reasonably convincing answer to particular analytical problems (Hall 1999). From now on in sociology, Latour prophesizes, *everything is data*.

This statement is certainly not meant to belittle the importance of the many fine-grained discussions in sociology on particularities of method techniques, from how to do correspondence analysis on statistical data, over to specifics of how to interview powerful elite and expert respondents – a relevant concern to me, since my studies revolve to a large extent around elite interviews (see Kezar 2003; Kvale 2006). The point, rather, is that such discussion always sits somewhere in-between formalized rules-of-thumb and post-hoc reflection on past experiences, and as such bears at best a contingent relation to the actual research process. As Abbott puts this important point (2004:84), "data, methods, and theory will all be recast again and again throughout the course of

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52. In this respect, things are different in the writings of ANT co-founder John Law (2004). Given his commitment to a poststructuralist imaginary of sociology 'after method', however, even here methodical prescriptions are sparse. Like Latour, Law adheres to ideals of the ethnographic tradition, arguing that methods of participant observation are adequate to a social world conceived as messy, fluid, ambiguous and multiple. Beyond re-stating these commitments – which I sympathize with, even as I find them slightly too 'romantic' (see Gouldner 1973) – Law arguably says relatively little about the actual conduct of ethnography.

any research project".[53] Sociological research, quite simply, is a dynamic process, in which questions of method techniques gets embroiled in all sorts of real-world practicalities, on the one hand, and wider issues of the overall trajectory of inquiry, on the other. Focusing too narrowly on techniques involves a risk of loosing sense of this wider picture.

What this means, amongst other things, is that I am currently writing up a post-hoc justification of data-producing manoeuvres for this thesis that have, as is always the case, at times digressed widely from any straight path of enlightenment! Potentially, as anyone familiar with ethnomethodology should agree, there is no limit to the levels of fine-grained, real-time, on-the-spot details I might enter into, in order to account for the myriad choices, considerations, troubles, ambiguities and ethical quibbles encountered in the doing of my empirical research. This is not, however, what I intend to do here, for a couple of interrelated reasons. First of all, writing endlessly about my own empirical experiences would not necessarily make much contribution to methods, understood as sets of on-going conversation in sociology. More importantly, and in line with the pragmatism of Latour and Abbott, what counts is not really the actual research process – where, to re-use an infamous slogan and assuming a level of ethical reflection, indeed 'anything goes' (cf. Feyerabend 1975) – but rather the post-hoc justification itself. This is old-fashioned theory of science: even *after* Latour, Abbott, and others have demystified the practice of (social) science, this practice *still* has to rely on certain 'logics of justification' deemed persuasive in some collective of practitioners (see Strathern 2004).

In this thesis, most work of method justification is done at the level of the actual articles that, as self-enclosed argumentative entities, relies for their persuasiveness on a satisfactory account of methods, data, and empirical claims. Indeed, it is worth noting that, while all of my essays make more-or-less specific empirical claims, most of them contain relatively brief accounts of the more 'technical' methods employed. In my interpretation, what this shows is that social science practitioners have a well-developed sense of what it means to

53. Along similar lines, Bourdieu (1988) puts this point poetically in writing about his own empirical research: "when we act without entirely knowing what we are doing, we make it possible to discover in what we have done something of which we were previously unaware".

do ethnographic fieldwork, conduct qualitative interviews, undertake a discourse analysis, trace semantic networks on the Internet, and reconstruct archival sources of information on some topic. In my inquiries into Japanese whaling and carbon markets, I use combinations of these qualitative methods, with varying intensities and emphases depending on empirical research opportunities and analytical problems at hand. All of this, we might say, is qualitative sociological business-as-usual, apparently with no particular need for in-depth justification – assuming, that is, that the resulting argument manages to convince! Only if my argumentative chains break down would questions of technique conceivable come to the foreground.

Again, saying this is not meant to imply that everything that goes on in this thesis at the level of methods is entirely standard, straightforward and self-explanatory. Indeed, wanting to do ethnographic case studies – rather than, say, causal statistical analysis or historical narration (see Abbott 2004) – is far from self-explanatory, which is why this section opened by justifying my choice in the specific context of analytical interest in the dynamic co-evolution of science, society, and the environment. Moving further, at various points in this thesis – and specifically in essay 3, organized as a sustained discussion of ANT methods in the global arena – my ambition is to engage with, and contribute to, more fine-grained discussion on qualitative methods in contemporary sociology. These discussions are currently conducted under different headings, across sociology and related fields; the most relevant for my own concerns being, respectively, 'mobile sociology' (Urry 2000a), 'multi-sited ethnography' (Marcus 1995), 'methodological cosmopolitanism' (Beck 2006), and 'object-centred sociality' (Knorr-Cetina 1997). All of these headings bear some relation to concerns expressed in ANT meta-theory, but take discussions further in the direction of new rules of sociological method.

To start by lumping the first three of these designations into one problematic, what is at stake, generally, is what Ulrich Beck (2006) has nicely summarised as social life overflowing the 'container' of the nation-state. As has often been remarked (e.g. Bauman 1987; Wagner 2001), sociology as a discipline has historically arisen in close relation to the powers of the modern European nations, not least in Scandinavian countries marked in the post-war era by expansions of a universalistic welfare state (see Kropp & Blok 2009). Now that societies everywhere are presumably undergoing new rounds of 'globalization', this historically dominant pattern of methodological nationalism in sociology is coming under renewed criticism, challenged not least by what Beck and others term methodological cosmopolitanism (e.g. Beck & Sznaider 2006). Indeed, the environmental risks studied in this thesis are routinely mentioned in relation to this development, both epitomizing and itself spurring 'globalizations' of various kinds (Yearley 1996). The notion that a post-national condition should call for innovations at the level of sociological method is thus hardly controversial, although the term methodological cosmopolitanism arguably tells us little about the specifics of such innovations.

In this respect, we stand to gain more, I would argue, from what John Urry and others dub the 'mobilities turn' in the social sciences (yet another turn, adding to the practice, material, and topographic ones – this list is getting too long). Urry's starting point in wanting to move 'sociology beyond societies' (2000) is simple: from asylum seekers to business people, prostitutes to sports stars, "all the world seems to be on the move" (Sheller & Urry 2006:207). Environmentalists, too, like the ones interviewed for this thesis, are fierce users of the world's many airports, travelling this infrastructure of consumerist 'non-places' in order to participate in research conferences, diplomatic meetings, and transnational protests (Augé 1995; Blok 2005).[54] More importantly, attending sociologically to the travelling of people, objects, images, and technologies also imply the need for more mobile research methods, including new forms of mobile ethnography. Indeed, in some sense, this entire thesis is an instantiation of global-scale mobile sociology: much of the whaling case has been researched from a physical position within Japan; and the carbon market case involves physical travel to India, together with the imaginative mobility afforded by Internet 'cyber-ethnography' (Molz 2006).

Within discussion on mobile sociology, a whole range of innovative methods are currently being developed, from the use of time-space diaries to co-walking interviews, mapping of travelling objects, and careful attention to the affective atmospheres of bodily movement between places (e.g. Büscher & Urry 2009).

54. At the time of writing, tens of thousands of diplomats, heads of states, journalists, researchers and activists are on the verge of arriving in Copenhagen for the COP15 UN climate summit, nicely illustrating this paradoxical point about environmentalists addicted to runways. As a Copenhagen-based researcher, the whole event illustrates what Ulrich Beck (2006) terms the 'cosmopolitization of the local' – cosmopolitanism, these days, is fast becoming an everyday social reality to still more people (see Szerszynski & Urry 2002).

In my own studies, the invocation of mobile sociology is more metaphorical, having to do, first, with my own ethnographic mobility between far-away places, and second, with a general ANT-inspired interest in empirically tracking the displacements of objects, symbols and discourses within trans-local networks. This second point is what I call 'ethno-socio-cartography' in essay 3, illustrating this new method by way of the global displacements of whale-related figurations. As for the first point, it ties into what anthropologist George Marcus (1995) calls 'multi-sited ethnography'. Put crudely, multi-sited ethnography is what I *do* in this thesis, without engaging in too much discussion on its specifics (see, e.g., Lapegna 2009 for an overview). It makes intuitive sense to conceptualize the field of inquiry in multi-sited terms when researching globally dispersed assemblages such as biodiversity and climate change (Collier & Ong 2005); indeed, it is hard to see how these issues could be studied meaningfully by sociology *without* a multi-sited imaginary.

This brings me to a final point of method innovation brought about by, or rather enabled in reference to, ANT, in conjunction with kindred developments summed-up previously as the material turn in social theory. In accordance with a meta-theoretical disregard of distinctions between the social and the material, this approach entails empirical interest in, and attention to, the multiple materialities of the social worlds under study, together indeed with an awareness of the materialities of social research itself (see Whatmore 2003). In the apt vocabulary of Karin Knorr Cetina (1997), ANT epitomized a renewed sociological interest in 'object-centred sociality' and 'post-social relations', whether this be the kind of screen sociality enabled in financial markets by world-spanning computer networks (Knorr Cetina & Bruegger 2002), or the dramatic expansion of affectively meaningful relations to companion animals in late modernity (see Franklin 1999; Haraway 2003). In a very basic sense, taking human relations to natural environments as a sociological object of study, as I do in this thesis with whales and carbon dioxide emissions, is to engage in the empirical study of object-centred sociality. Furthermore, with ANT moving beyond social constructivism, it would make little sense not to engage empirically with these materialities, considered as hybrid quasi-objects.

The question of how to employ sociological methods in ways that stay sensitive to the non-human materiality of entities like whales and climate change is complex, and in some sense beyond the scope of this thesis. As noted, the main ANT response to this predicament is to turn the very production of natural scientific knowledges – themselves vehicles of spokesman-ship for non-humans – into objects of careful sociological study. This is also the method followed in my thesis, which looks in considerable depth at the knowledge making practices by which endangered whales and climatic changes come to participate in global social conflicts. Contrary to 'socio-centric' sociological methods, then, ANT forces one to take seriously the chains of material-semiotic mediations linking humans and non-humans. At times, documenting such chains may be done via bodily immersion by the researcher, as when I participate as observer in whale watch tourism (in essay 2).[55] At other times, sociological tracing remains at a distance, as when I attempt to unravel the biological and political linkages constituting whale population figures (in essay 3). In each case, the general point remains that no matter what, the *exact* nature of human/non-human relations is bound to remain contestable; in this respect, however, the sociologist faces the same predicament *as everyone else* (see Latour 2000a).

Again, it is not my intention to deal with these tricky issues in any further depth here; instead, I attempt to show the value of ANT as a sociological method of material semiotics across my essays, and thus hope to demonstrate their usefulness in practice. The most elaborated discussion of these issues is to be found in article 2, which may be read as my attempt to position the methods and meta-theory of ANT material semiotics in the wider landscapes of environmental sociology and human-animal studies. It bears mentioning, however, that the expanded roles afforded to non-human agency in ANT methods continue to be a major source of contentious discussion, not only in more 'mainstream' approaches to sociology, but notably within science studies itself (see, e.g., Bloor 1999; Vandenberghe 2002; Yearley 2005b). My own position would be that these are indeed valuable and necessary discussions, but also that most 'humanistic' criticisms of ANT miss the mark, because they fail to see that ANT is fully compatible, in socio-ontological terms, with an endlessly varying landscape of differently 'agentialized' non-humans (Harman

55. There is plenty of scope for expanding this comment much further. At the 2008 BSA conference on 'Social Worlds, Natural Worlds', I heard a wonderful presentation by a Canadian sociologist on how to expand ones sociological imagination towards conducting 'interviews' with trees as non-human social actors!

2009). The point, then, is that non-human agency needs to be studied *contextually* – which is what I attempt to do in this thesis, with whales embodying a class of particularly agential charismatic non-humans (Lorimer 2007).

To summarise, what I have outlined in this section is a set of principled arguments or justifications for some particular choices of interrelated methods – conceived as mobile, multi-sited, and materially sensitive ethnographic case studies – employed in the production of empirical data for this thesis. As pointed out, much or indeed endlessly more could be said about particular aspects of these methods, not least at the level of on-the-spot details of various difficult practicalities encountered along the way in my studies. To mention just one such 'practicality': learning enough Japanese to do fieldwork on whaling in Japan has been a constant struggle, and one resolved only partly successfully on my part, meaning that I have had to rely on a combination of a gradually improving spoken Japanese, translation assistance, and English-language elite interviewing. There are potentially enough issues hidden in such practicalities to warrant a few articles of method reflection (see, e.g., Bestor et al. 2003). The fact that these reflections are not to be found in this thesis does not imply any denigration of their importance, but simply reflects my analytical priorities in this context. After all, I consider the textual *results* of my Japanese forays sufficiently interesting in sociological terms to legitimately background what could, in other contexts, indeed become the foreground.[56]

At this point, then, my sense is that a somewhat different issue needs handling: why is it that, while consisting of two analytically linked case studies, this thesis can only be called 'comparative' in a minimal sense of the word? This, in my estimation, is perhaps the most important point at which my thesis

56. This may well be the juncture at which I stand in risk of loosing whatever anthropological audience this thesis may otherwise acquire! As Marilyn Strathern recently observed (2009:4), discussing Andrew Abbott's notion of fractal distinctions: "What for the sociologist Abbott is of intermittent concern ('Every now and then… social scientists recall that their ideas are as contingent on themselves as on their objects of study [2001:15]'), for present day anthropologists is a constant issue". In my estimation, this is a highly concise summary of, or analogy for, present-day relations between two disciplines that, on all other accounts, ought to have much more in common than their practitioners seem to like to think. In this respect, I readily pledge guilty of 'sociologism' when it comes to my invocation of ethnographic methods.

could have been, or perhaps in the future can be, substantially improved. Having said that, I *also* think there are good reasons why the comparative dimension remains underdeveloped – and the following sub-section is meant to articulate some of these reasons at the level of method.

Notes on cases, contexts, and contrastive comparisons

Somewhere in his impressive oeuvre of macro-historical comparative studies of Japanese religion in the tradition of Max Weber, Robert N. Bellah comments to the effect that Japan is the ultimate gift to any comparative sociologist (cited in Eisenstadt 1996). What Bellah has in mind is the way in which Japanese experiences of modernity, religion, community, selfhood, and so on, lend themselves readily as mirrors on often quite different experiences in Euro-America (see Bellah 2003). As a sociologist doing ethnography from within Japan – but entertaining no particular *professional* commitment to concepts of Japanese 'culture' – this comment has come to take on special meanings for me. Brilliant as his studies may be, Bellah's comment epitomizes a ready-made, conventionalized answer to the problem of comparative social science: treating individual countries as comparable cases across some domain of social life (see Ragin 1992). Bellah's version of comparative sociology, in short, embodies strong but increasingly problematic conventions: a commitment to methodological nationalism, coupled with a seeming belief that 'domaining' social life into politics, economy, religion and so on is a straightforward manoeuvre requiring little analytical work (see Strathern 2004).

Beyond what has already been said about multi-sited approaches to ethnography, methodological problems of case studies, contexts, and comparisons run deep in the epistemology and ontology of contemporary social science. In fact, ours may well be an era of a 'crisis of context' (see Schlecker & Hirsch 2001). Some of these issues were already discussed, indirectly at least, under the rubrics of ethnomethodological unique adequacy and Latourian empiricist-descriptivist commitments to singular explanations developed in relation to unique cases. It is easy to see how such ANT commitments to single-case-studies, as interpreted through an abstract ontological vocabulary of hybrid actor-networks, may cast doubts on the very do-ability of comparative social science (see Gläser & Laudel 1999). As has often been noted by more ‘orthodox’ colleagues in the sociology of science, a Latourian view of science-society relations can seem strangely fragmented in-between ontological extremes, with little regard for enduring institutions, discourses, and social categories (Guggenheim & Nowotny 2003). ANT, on this view, not only makes nation-based comparison difficult; in extreme, it may be (mis-)read as rendering *all* social science comparison impossible.

My previous sympathetic criticism of ANT descriptivism was meant as a first intervention in this tricky debate, signalling, first, that there is indeed grounds for concern, but also, second, that these concerns can potentially be rectified from *within* Latourian socio-ontological premises. What I argue, in essence, is that Latour fails to adequately recognize the work of social scientific concepts as legitimate abstracting devices, allowing certain forms of de- and re-contextualization of knowledge, and hence ultimately enabling comparisons across time-spaces. In this section, I want to build further on this line of thinking, in arguing that, far from being an ‘anti-comparative’ stance, ANT methods simply serve to warn against particular *forms* of comparison, and particularly against knowing in advance of empirical inquiry *what needs comparing*. Hence, we need to ‘temporalize’ these issues of case comparisons, in order to get a clearer sense of our available options for a new mode of comparative sociology beyond societies.

There are good reasons for starting this temporalization by briefly suggesting that a current ‘crisis of context’ runs deep in social science – in part, admittedly, to justify why this thesis is perhaps less stringently comparative in approach than might be expected. First, a number of ‘post-modern’ deconstructivist ideas, from language philosophy notions of inter-community un-translatability to Kuhnian incommensurability thinking, have arguably served to question the persuasiveness of social scientific attempts at trans-context commensuration (see Steinmetz 2004).[57] More important from my perspective, however, are the ideas touched upon several times already, pertaining to the part-whole problematic and partial connections of Marilyn Strathern (2004), and associated notions of trying to cope with a seemingly boundless unfolding of analytical

57. Echoing Latour’s characterization of post-modernism as ‘disappointed modernism’ (1993), none of these critiques, I believe, get to the crux of a crisis of context. I mention them here only as symptoms of a wider transformation of social science reflection on issues of comparison over the past 30 or so years.

complexity (see Schlecker & Hirsch 2001). What has become increasingly obvious here, as Strathern notes (1992:73), is that "nothing is in fact ever simply part of a whole because another view, another perspective or domain, may redescribe it as 'part of something else'".

Hence, speaking of a 'crisis of context' not simply implies that knowledge, including natural and social scientific knowledge, is increasingly recognized as contextually situated – a growing recognition arguably spurred by complexity theory (Byrne 2005) and also importantly by developments in the field of science studies (STS) invoked in this thesis (Law & Mol 2001). Just as important is the recognition that "what counts as a context depends on what one wishes to explain", and that in this respect, "there seems no end to the number of possible contexts" (Burke 2002). Contexts, from now on, always come in plural – epitomized in sociology, for instance, by the fact that 'society' no longer forms a taken-for-granted context of sociological inquiry (Urry 2000b).[58]

These wider developments can be observed clearly in the case (!) of the 'new' sociology of science (STS), and particularly in the work of Bruno Latour (Schlecker & Hirsch 2001). What happened in STS in the 1970s, put briefly, was first of all a singling out of laboratories as the local cultural settings of scientific knowledge (see Knorr Cetina 1995), and secondly, an adoption of ethnographic research methods in the hope of encompassing a diversity of scientists' everyday practices. Before long, however, the entire field was engaged in protracted disagreements as to which contexts best constituted the contents of science: should one focus on material or epistemological aspects of scientific work; experiments or back-bench gossip over coffee; relations to other scientists or relations to funding bodies? The narrowing down of research scope to 'local' laboratories thus entailed a 'fractal effect' of bringing out still more relevant contexts of practice!

These problems of context exacerbated once STS scholars, since the 1990s, decided to dramatically widen their empirical research scope to encompass the historical, cultural and political relations between laboratories and societies (see, e.g., Sismondo 2008). One important consequence has been a widespread

58. In this respect, I would argue, sociology is belatedly catching up with the discipline of anthropology, where 'culture' seized to be a taken-for-granted context already in the 1980s. The famous epitome of this development is the collective volume on *Writing Culture* (e.g. Clifford 1986).

adoption of Marcus' multi-sited ethnography in studying global assemblages of techno-science (e.g. Collier & Ong 2005) – a genealogy that my own thesis shares. In this context (!), the Latourian dictum to 'follow the actor' may be read as his response to the predicaments of a crisis of context, in seeking to justify an ethnographic search for an ever more in-depth situating of knowledge beyond ideas of an integrated whole (Schlecker & Hirsch 2001:78). Indeed, Latour explicitly rejects all notions of analytical 'context' as incompatible with ANT (2005). The best one can hope to do with ANT, according to Latour, is to study how actors ethno-contextualize themselves and others when framing and scaling dynamic relations.

Before moving on to question this Latourian dogma of 'no context', let me re-state that I consider the crisis of context to be a serious challenge, and that I further consider the ANT response to this predicament a necessary step in the right direction. One way of showing the value of ANT methods is to follow Latour himself in further temporalizing social inquiry, this time by drawing a distinction between 'new' and 'well-known' topics. ANT, Latour insists, shows its real worth "in situations where innovations proliferate, where group boundaries are uncertain, [and] when the range of entities to be taken into account fluctuates" (2005:11). In these situations, actors cannot be limited to "the role of informers offering cases of some well-known types" (ibid.). This is a convincing general argument for social inquiry, and also one that serves well in framing my own two empirical case studies. As I show in my essays, controversies surrounding Japanese whaling and carbon markets are both situations in which innovations proliferate, group boundaries are uncertain, and the range of relevant entities fluctuates. Indeed, this starting point in large part explains why I find the cases sociologically exciting.

What this further temporalizing of ANT method might allow us to see is that the extent to which case studies can be rendered comparable is in large part an *empirical* question. Hence, Latour can legitimately be read here as allowing only for 'actualist comparisons' (Steinmetz 2004), that is, comparing series of events construed as empirically commensurable. Indeed, at this level, sociological comparisons will often blend into actors' own ethno-comparisons, given that comparative inferences are ubiquitous components of all thought and speech.[59] In this sense, my studies in this thesis are already comparative, although more so internally *within* each case then between the two cases (see Mjøset 2006). Establishing a context for interpreting current Japanese 'scientific' whaling, for instance, almost necessarily entails a need for implicit or explicit comparison with Norwegian whaling practices (see essay 1); and interpreting different types of NGO actions vis-à-vis carbon markets requires some implicitly comparative typology (see essay 5). The important point, however, is that actualist and ethno-comparisons does not get us very far in terms of methods for conceptual comparison *across* domains.

For such cross-domain comparisons to happen, some level of conceptual abstraction is called for – and the question, really, is how to think about this work of de- and re-contextualization? At various points in this introduction, I have invoked conceptual abstractions, by suggesting for instance that mine are case studies of 'world risk society' (although I questioned the value of this characterization), or alternatively ANT cases of the dynamic co-evolution of science, society, and the global environment (which I am more strongly committed to saying). Taken on their own, however, both of these acts of theoretical contextualization remain too abstract to allow for any meaningful exercise of comparison between my two cases. In this respect, we would quickly find ourselves in need of 'analytical dimensions' to compare; and, to reiterate, what these dimensions should ideally be is anything but clear, given the open-ended, in-the-making nature of the social relations constituting each case domain.

What these observations imply, I believe, is that we need to keep thinking of social inquiry in dynamic terms, as involving recursive acts of 'casing' issues, together with 'contexting', understood as the temporary foregrounding of some or other theoretical context of interpretation. This is really another way of framing Abbott's point about the need to recast, again and again, data, methods and theory within any one inquiry. In this respect, my own studies are no exception, and as in the case of method justification more generally, the main

59. This comment echoes Durkheim's suggestion, in his *Rules of sociological method* (1982 [1895]), that all sociology is comparative, because explanation depends on establishing if a phenomenon is typical or unique. What needs to be added, however, is that this is not a peculiarity of sociology, but rather a peculiarity of any type of explanatory effort, whether based on 'lay' or 'professional' sociologies.

work of ‘contexting’ in this thesis takes place at the level of individual articles. Hence, for instance, article 1 invokes the context of symbolic interactionist approaches to social movements; article 3 invokes the context of multi-sited ethnography methods; and article 6 invokes the context of post-ANT notions of social topology. Within each of these respective contexts – and, indeed, within a host of others, also of a more empirical kind – my studies rely on performing a scaling between the particular and the general, or in other words, on making abstractable claims. The point, however, is that ‘general’ means always ‘general to some particular context’; and that other contexts could always have been added, creating more partial connections.

Put differently, what this means is that the ability to de- and re-contextualize bits of data, method, and conceptual abstractions vis-à-vis each other in new patterns is a major part of what we mean by originality and creativity in social science (see Lamont 2009). On this view, case studies are indispensable building blocks for *all* of sociology, regardless if cases are used for creating grounded theories (e.g. Steinmetz 2004), for testing hypotheses (e.g. Flyvbjerg 2006), for adding new insights to local research frontiers (Mjøset 2006), or for learning something new about particular, unknown, or unusual social phenomena (e.g. Abbott 2007a). In a very general sense, all of these valid goals of social science knowledge build on some notion of implicit comparison across cases; we should not assume, however, that theory development is the sole aim of case studies (see, e.g. George & Bennett 2004). In my own understanding, this thesis primarily contributes new empirical, methodical, meta-theoretical and political-ontological understandings to the local research frontier defined in the borderland between STS and environmental sociology. To this effect, my case studies deal with two sociologically ‘novel’ phenomena, enabling me, hopefully, to abstract original insights of rather more general interest to the discipline.

What I have not done explicitly, however, and what might be a sensible further research step, is to use my cases as what Karin Knorr Cetina (1999:4) calls a ‘comparative optics’ for identifying patterned differences between the two domains. As Knorr Cetina argues in her work on molecular biology and high-energy physics, looking at one science through the lens of the other helps to ‘visibilize’ the otherwise invisible (ibid.). This comparative optics is meant to bring out important *differences* between two fields, with each case serving to map equivalent and conflicting patterns in the other. Such contrastive comparison suggests itself, I believe, as an adequate method to employ when working from an ANT starting point of complex, dynamic, and detailed case studies of global-scale assemblages. In the context of this thesis, contrastively comparing whaling and climate change as two trajectories of divided socio-natures with varying spatial, temporal and social dynamics would surely make for a worthwhile analytical exercise. While I make some remarks to this effect in the following sections, however, systematically pursuing a comparative optics is beyond the scope of this thesis.

In the spirit of a somewhat regretful justification, let me suggest that there are good practical reasons as to why I have not been able to pursue such a new comparative optics for a cosmopolitan, multi-sited sociology any further than I have in this thesis. As Knorr Cetina notes about her own comparative approach (1999:19), "the great complexity of the fields investigated implies that the study could not possibly have been done by one person". By analogy to my own situation, studying the roles of scientific knowledges in public controversies on whales and carbon markets is to study fields of such complexity that simultaneously reaching an in-depth understanding of case-specific dynamics *and* applying a meaningful comparative optics has simply not been possible in this single-person study. This, I believe, is in itself a noteworthy finding. Cultivating a new sense of comparative optics for a mobile sociology defined around complex trans-local fields of material-semiotic practices is a labour-intensive project, requiring massive investments of intellectual and economic capital, together with new forms of collaborative research teams hitherto seldom in the qualitative social sciences.[60]

Implicitly, in the framing of my cases, something like a comparative optics has in fact played an important role, in the sense of helping me delimit fields of social relations to study. To see how this works, we may note how, in a strangely objectivist tone, Latour invites comparative efforts by distinguishing longer from shorter actor-networks (Latour 1993). In researching controversies

60. The large-scale anthropological *Waterworlds* research project embedded at the university of Copenhagen, under whose supervision I had the chance to visit Greenland to discuss 'scales of sustainability' in September of 2009, seems to me an emblematic illustration of where such collaborative global-scale social sciences may currently be heading, in conjunction with processes of Europeanization of research policy.

on Japanese 'scientific' whaling, it occurred to me that this was indeed a relatively short network, or more correctly, a micro-cosmos of global-scale relations, consisting of fairly delimited sets of political, diplomatic, scientific and activist groups (see essay 3). The same could not be said about climate change: this is, as Michel Callon (2009) notes, at present an 'unqualifiable' issue, in the sense that no analytical frame can contain it in its entirety. The actor-network of climate is simply too long and with too many chaotic overflows to constitute a doable ethnographic multi-site. This is the main reason for focusing my empirical efforts more specifically on carbon markets, which – while still chaotically complex – at least delimits a sociologically manageable field of global-scale social relations. A comparative optics, in short, has helped me gain a sense of the 'sizes' of different global assemblages (Collier & Ong 2005).

In sum, for sociology to move beyond methodological nationalism, we need to deal with the attendant crisis of context by gradually cultivating a new sense of analytical proportions (see Strathern 2004).[61] Beyond a conventionalized sense of what constitute cases and their contexts – as tied up with the nation-state – we need to creatively assemble a conceptual vocabulary for carving out trans-local fields of networks and mobilities, which we may in turn employ as comparative optics on each other (Urry 2000a; Beck & Sznaider 2006). In undertaking this endeavour, we need to accept that, regardless of analytical and empirical levels, a sense of complexity is going to reproduce itself at all scales of detail, from the 'small' to the 'large'. If anything, we might suggest with Latour (2005) and Law (2003) that the 'global' is often going to seem *less* complex than the 'local'! What this implies, minimally, is that sociology is in great need of rethinking its entire micro-macro distinction – a project to which this thesis in its entirety attempts to contribute (see, in particular, essays 3 and 6; Bruun Jensen 2007).

Returning for a second to the model of my sociological settlement (figure 2), this diagram is meant to suggest possible directions along which we can think about the challenges just outlined. In the case of my own studies (see figure 3),

61. In the course of writing my thesis, I have been engaged in discussion with colleagues in STS and anthropology on this new sense of comparative methods. In particular, I presented preliminary thoughts on the issue at an international workshop held in Osaka, Japan, in July 2009, under the heading of 'travelling comparisons'.

the diagram serves to define doable analytical questions for a comparative optics beyond methodological nationalism. Hence, at the levels of social embedding and meta-theory, respectively, we should ask how well ANT copes with the highly varying time-space geographies, scientific knowledge claims, and collective identity conflicts manifested in different global assemblages of socio-natures? These, indeed, are questions that my essays can be said to already address, when read across empirical fields (1 and 5 for social embedding; 2 and 6 for ANT meta-theory). Similarly, at the level of methods, it would be worthwhile to ask what the notion of 'ethno-socio-cartography' (essay 3) implies for the study of carbon markets, climate change, and beyond? Indeed, what would an agonistic 'cosmopolitics', analogous to the one identified for whaling conflicts (essay 4), look like in the climate case, and in further cases of divided socio-natures?[62] Can we start identifying recurring patterns here, perhaps even some 'demi-regular' mechanisms, despite noting that in empirical terms these cases may seem incommensurable?

Again, these questions must unfortunately remain speculative in this context, as going beyond my manifest studies. Nonetheless, the last two sections of this introduction is dedicated to taking some first few steps toward providing answers, by looking at how my two cases can be made to contrastively interfere with each other at empirical and political-ontological levels. The usual proviso applies here as well: as always, whatever connections to be drawn are bound to remain strictly partial.

Social embedding: whales, climates, and global knowledges

As pointed out, sociological problems has the curious character of always-already being over-determined by historical, political and cultural commitments, long before the arrival of the sociologist onto the scene (Brown 2009). While this predicament has given rise to venerable traditions of ostensibly 'value-free' and 'critical' social theories, associated with the names of Weber and Marx respectively, what the value-laden nature of sociological objects entail in terms of actual research practices has received less attention

62. At the time of writing, I am in fact working (together with Prof. Margareta Bertilsson) on this exact question, invoking a Carl Schmitt-inspired notion of 'climatic exception' to understand certain agonistic political tendencies in this domain.

than it should (see Abbott 2007b). Working on high profile and on-going environmental risk controversies, as I do in the thesis, the need to engage this issue of over-determination becomes immediately acute, in terms of how to position a *sociological* approach to whaling and carbon markets vis-à-vis other existing ethno-methods. This, we should note, is a relational problem. Just as this thesis explores how to make sociology relevant to collective understandings of ecological crises, the converse issue also arises: how to make ecological issues relevant to problems defined *in sociological terms*? To paraphrase Deleuze, 'problems' are not delivered up to common sense perception, but must be continuously created and recreated by the exercise of creative thought (see Brown 2009:106).

Indeed, everything outlined so far in this introduction aims to achieve exactly this: the crafting of sociologically interesting problems from some rather simple observations on the social role of scientists in environmental controversies. What has arguably been left implicit so far, however, is the kind of 'socio-cognitive settlements' that predates the arrival of my sociological interest in whales, climates, and other contemporary instances of divided socio-natures. To be sure, some of this context has already been suggested, particularly in discussing Ulrich Beck's world risk society theory, and in providing an ANT travel guide to science-society relations in the climatic domain. What remains, however, is to provide a more detailed sense of the local research frontiers, and the existing socio-political settlements, with which my sociological case studies are obliged to enter into dialogue, but which must also be shown to be insufficient in particular ways. The mere fact that something is a 'socio-political problem' – as ecological destruction surely often is – does not in itself justify sustained sociological interest.

What *does* justify sustained interest, however, is the way in which sociological engagement with issues of science, society, and the environment may prove transformative not only of the objects studied but also of sociology itself (see Fraser 2009). The rise of environmental sociology testifies to how the discipline has been affected by modern environmentalism since the 1960s – although the exact scope of this transformation is debated (see Brechin 2008). In a wider sociology of knowledge perspective, environmental sociology forms part of an emerging socio-cognitive settlement, with modern disciplines taking on board ecological issues, each on its own terms. Hence, we now have subspecializations of environmental economics, politics, law, and so on, all forming part, as it were, of a new ecology of environmental knowledges, which remains heavily dominated by natural sciences old and new (see Hajer 1995; Stengers 2005). In this process, the cognitive domain of environmental sociology has often been prematurely limited to the 'human dimensions' of environmental change, eschewing a sustained interrogation of what this means in theoretical and practical terms.[63] This thesis attempts to carve out a different ecological space for sociology, by noting how environmental risks may also prompt a radical reconfiguration of core concepts of the discipline (Cooper 2009:8).

More specifically, the kinds of environmental issues studied in this thesis, that is, issues of whaling (biodiversity) and carbon markets (climate change), illustrate in striking ways the oft-noted role of 'reflexivity' in contemporary social life, in this case organized on a global scale. To elite actors professionally engaged with these issues, a kind of global reflexivity of knowledge is a taken-for-granted starting point, in the sense of new information looping back on action possibilities at high speeds (see Riles 2008). To be sure, knowledges are unevenly distributed across social terrains, in ways both reflective of and constitutive for shifting power relations and social identities (Stehr 2005).[64] Environmental NGOs, for instance, may fruitfully be understood in terms of their differentiated capacities to generate, re-frame and disseminate strategically selected knowledges in-between worlds of policy-making, sciences, industries, and public constituencies (see, e.g., Yearley 1992; Jasanoff 1997; Jamison 2001).

What this global reflexivity of knowledge means is that issues of environmental risk are always-already analyzed, in a variety of sophisticated idioms, *before* the arrival of the sociologist and her/his tools of analysis. This situation entails any number of ambiguities on the part of the sociologist, in that it raises the prospect of a collapse of epistemological distance between analyst

63. Already in the 1990s, international social science programmes were organized around the idea of human dimensions to climate change (see Jasanoff & Wynne 1998). In the Danish science policies of the 1990s, the 'human dimensions' of environmental issues came mainly to mean micro- and macroeconomics (Blok 2007).
64. The Foucaultian notion of power/knowledge is one of several fruitful possibilities for exploring overlaps between Latourian ANT and so-called governmentality studies (see, e.g., Kendall & Wickham 1999; Miller & Rose 2008; Hekman 2009).

and object (Riles 2000). To give just one simple illustration: the way current global controversies over whaling are configured, knowing how many (or rather: how few) Japanese citizens support Antarctic whaling, and how many (few) eat whale meat on a regular basis, is of strong strategic importance to anti-whaling movements. This means that such statistical figures are routinely produced, for instance by the Japanese office of Greenpeace, and circulated in global media networks. In other words, what might under different circumstances be taken as a 'sociological' problem, of mapping whale-eating practices in Japan, is pre-empted, so to speak, by ethno-practices of routine knowledge-generation within the field. In a 'knowledge society', the objects of sociology are themselves the products of distributed knowledges.

Another important corollary of this situation is that the amount of existing academic literature on the two empirical issues I pursue in this thesis – whaling and carbon markets – is mind-bogglingly huge. This is true even if we allow ourselves to narrow the focus to the social sciences, where issues of whaling, in particular, has been extensively researched from anthropological, legal, and political science perspectives over the past 25 or so years. This outpouring of academic attention reflects the way global conflicts over whales and whaling have become emblematic of wider environmentalist concerns since the early 1970s, when slogans of 'saving the whale' started to emerge in Euro-American public life. Social scientific attention tends to fall in characteristic patterns: anthropologists study 'whaling cultures' (e.g. Kalland 2004), legal scholars attend to international environmental law (e.g. Gillespie 2005), and political scientists consider whaling an 'environmental regime' of global governance (e.g. Andresen et al. 2000). Interestingly, however, beyond anecdotal mentioning, sociology has so far contributed little to this cognitive ecology of social science whaling knowledges (but see Franklin 2001).[65]

In the domain of climate change, a comparable situation of cognitive division of labour look set to emerge, although in this domain, economics has assumed a far more prominent position than any other social science (see Yearley 2009). Contributions from the non-economic social sciences, including sociology, have so far been scattered – a situation rapidly changing these years, however (see

65. Indeed, in essay 2, I use this observation on relative sociological absence as a springboard for positioning ANT as a valuable ecologized sociology of human-animal relations.

Lever-Tracy 2008). As epitomized by the rise to fame of Sir Nicolas Stern since the mid-2000s, economic thinking in the climate domain has almost come to rival the public visibility of natural science; and in the domain of carbon markets, economics has had discernable political effects. Against this backdrop, STS scholars have recently taken up carbon markets as an important case study in the large-scale construction of socio-material markets (e.g. MacKenzie 2009).[66]

The reason for me to briefly outline the ecologies of social science in the environmental domains of concern to this thesis is that my studies can reasonably be read as sociological *responses* to these different knowledge-political situations. On the one hand, my approach to whaling and carbon markets share some fundamental similarities, including, amongst other things, a theoretical inspiration from actor-network theory, together with an analytical interest in how professional knowledge practices help frame the contours of public action in these environmental domains. One important reason for focusing on these domains through the employment of the 'new' Latourian sociology of science (STS), then, is exactly the way environmental objects are *already* constituted as products of distributed knowledges, not least of a natural scientific and economic kind.

On the other hand, my brief summaries of these two domains of whaling and carbon markets is also meant to indicate that a sociological response need to be contextually sensitive, given divergent case histories of knowledge-political patterns. In the case of whaling, so much is already known from anthropological, legal, and political science perspectives – including amongst whaling policy actors themselves – that carving out some 'available' domain of sociological problems has been somewhat of a challenge for me. In this situation, ANT proved an indispensable tool, in allowing me to recast the sociological problem as one of dynamic relations between humans and non-humans, culture and nature. Once recast this way, any number of interesting sociological problems upon up, in terms of how and by whom whales come to be known and socialized into human affairs as particular kinds of actors, processes attended by dramatic conflicts (see essays 2, 3 and 4). It quickly

66. This is the context in which my essay 5 is articulated, following on from a workshop, organized by Donald MacKenzie and Michel Callon in Durham in 2007, on social science responses to carbon markets.

becomes apparent, for instance, that biological sciences are only one part of much wider 'ethno-epistemic assemblages' of scientific, legal, cultural, and affectively imbued knowledge claims, mobilized on either side of the current controversy between anti- and pro-whaling proponents (see Irwin & Michael 2003).

In more fine-grained detail, the politics of Japanese 'scientific' whaling is a topic of great interest, particularly in recent political science where it is framed along the lines of 'why Japan will not give up whaling' (e.g., Danaher 2002; Ishii & Okubo 2007). These debates are mostly conducted in specialized journals on international wildlife law and policy, and often contain some reference to the fact that the Japanese government is acting against an (almost) 'international norm' of whale protectionism (Miyaoka 2004). In conducting interviews with pro-whaling protagonists in Japan, I decided to do my own version of this social science genre. Rather than political science vocabularies, however, my interpretation of elite Japanese allegiances to whaling is cast in terms of symbolic interactionist social movement theory, highlighting the moral construction of a collective pro-whaling identity (see essay 1). This interpretation, then, responds to a rather specialized research frontier, on the politics of Japanese whaling. In this respect, I believe symbolic interactionist ideas of collective identity-formation do a better job of theoretical contextualization than would ANT, even if Latour is basically in accordance with interactionist tenets when it comes to the dynamics of group formation (see Latour 2005).

Contrastively compared via the optics of the whaling studies, my sociological response to the carbon market case has been significantly different on a number of key accounts. First of all, as suggested, the knowledge-political situation is radically different in this domain, in terms of how the object of inquiry is currently constituted in spatial, temporal, and socio-cognitive dimensions. To name one of the most obvious contrasts, whereas global whaling controversies have consolidated themselves around a few key geographical zones of engagement, with Japanese Antarctic whaling the centre of worldwide political attention, the same cannot be said for carbon market controversies, which tend to happen in shifting patterns of intensity all over the developing world. In this respect, carbon market controversies mirror the wider knowledge-politics of climate change, which likewise tends to get articulated in a bewildering array of shifting loci of global attention, both physically and on the Internet (see Rogers & Marres 2000; 2008).

This situation, together with the relative dominance of economic knowledge-claims in the domain of climate change more generally, has prompted me to re-frame the tools of ANT analysis from the whaling to the carbon market case. As in the whaling case, my analytical interest is in mapping the ethno-epistemic assemblages forming around carbon market concerns; but since these concerns are simultaneously *more* spatially dispersed and *less* heterogeneous in knowledge-political terms, the exact creation of a sociological problem-space differs in mainly two ways. First, I decided to tackle issues of economics, market-creation and environmental overflows directly, by invoking that branch of ANT developed by Latour's long-term partner Michel Callon under the rubric of the 'performativity of economics' (Callon 1998). Second, in response to the spatial dispersion of environmental NGO activity vis-à-vis carbon markets, I employ web-based forms of cyber-ethnography more extensively – combined, later, with a gradual narrowing down of empirical interest to groups of globally visible Indian environmentalists (see essay 5). The resulting analysis is primarily an attempt to re-articulate a situated knowledge-political space around economic and ecological climate concerns, a particularly important cleavage in the history of modern environmentalism (Latour 2004b).

In the language of Callon (2009), carbon marketization is currently one dominant form of climate change 'problematization', by which he understands a process of gradual fragmentation of an 'un-containable' issue into more confinable problems. Instead of talking about global warming, professional actors from science, politics, industry and NGOs increasingly talk about market efficiencies, global carbon taxes, the rights of developing countries, technological innovations, and so on and so forth. This downstream movement to more specifiable and treatable problems, Callon makes clear (2009:543), is always in itself complex and conflictual, with some actors refusing the proposed divisions. Hence, radical NGOs and analysts of a Marxist bend, for instance, insist on viewing global warming as one aspect of a more general problem of economic growth and its discontents (see, e.g., Zizek 2008). What this means is that carbon marketization is a highly contested development in the political economy of climate change (see, e.g., Lohmann 2005). My own studies can be read as an ANT-inspired sociological response to this situation, in terms of embedding economics in a wider political ecology.

While Callon's suggestion that climate change is at present an uncontainable issue is thus partly echoed in my own approach to carbon markets, it remains an analytical provocation when read against other strands of ANT. Generally speaking, Callon may well be correct in so far as we stick to a 'classic' Latourian picture of how the sciences stabilize and disseminate black-boxed facts as immutable mobiles. This, indeed, is *not* how global warming behaves as a socio-ontological entity. Nevertheless, by building on the more flexible approach to varying social topologies found in the post-ANT work of John Law and Annemarie Mol (e.g. 1994; 2001), I believe we may speak with more precision than Callon allows for on the issue of how climate change knowledge move between and transform social worlds. In my meta-theoretical analysis of the socio-spatial relations of climate change and carbon markets (essay 6), I give special attention to the notions of fluid and fire topologies, positioning these as particularly helpful metaphors in terms of analyzing the co-construction of science, society, and the global environment.

Needless to say, my two essays on climate change can only start to scratch the surface of what the discipline of sociology could potentially be doing in this highly contested domain. In this respect, there are marked differences in temporality between the two cases: whereas my re-interpretations of whaling controversies benefits from a certain stabilization of knowledge-political patterns over the past 35 years, any interpretation of climate change is bound to remain speculative at this point, given that important problematizations, in Callon's terms, are still novel. Sociological attention to climate change is only now starting to pick up, and available theoretical approaches spread out predictably, from neo-Marxism (Zizek 2008) via mainstream policy analysis (Giddens 2009) to socio-political scenario building (Urry 2008). In STS, where attention to climate change has a longer if surprisingly limited history, approaches likewise vary, from issues of scientific computer modelling (Jasanoff & Wynne 1998) to the adoption of new green technologies in domestic settings (Marres 2009).[67] It is fair to say that a 'sustainable'

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67. In the process of writing this thesis, I helped co-convene, with Prof. Margareta Bertilsson, a two-day international workshop on *Science Studies Meet Climate Change*, with the aim of fostering a sense of important STS and wider social scientific research agendas in

sociological agenda for climate change engagement is still only dimly visible (but see Yearley 2009); and my claim in this regard is that the kind of STS, ANT, and post-ANT approaches that I practice in this thesis will be crucial partners in this conversation.

As I have laboured to show in this section, while both whaling and carbon market controversies can usefully be understood as the products of distributed knowledges in power-inflicted relations, the quite different spatial, temporal and knowledge-political set-up of the cases has necessitated important re-framings of my sociological approaches. Symmetrically, this also implies that the kinds of empirical, methodical, meta-theoretical and political-ontological insights of more general sociological interest that I have been capable of abstracting from these cases varies, in ways already suggested in the preceding sections of this chapter. As noted, sociological 'problems' must be continuously created and recreated from imaginative thinking; taken on their own, social science objects always risk veering towards a kind of arbitrariness, articulated as they are into a historically contingent set of political settlements (Brown 2009). In brief, it is my hope that this thesis succeeds in being simultaneously transformative of the objects under study and of sociological study itself (Fraser 2009) – in however limited and partial ways.

What remains to be further specified, however, is the meaning of wanting to talk, in this context, about a possible transformation of the object of study stemming from sociological inquiry. In following Latour (2005) and the notion of performative social sciences (Law & Urry 2004), contrary to what is assumed in so-called 'critical' sociology, sociological relevance to the object under study must be seen as a precarious, rare and risky event! The final section of this introduction is dedicated to a few notes on how my own studies play themselves out at this level of ontological politics.

Ontological politics: towards a-critical (but hopeful) sociology?

Ever since its inception in the 1970s, the roles, responsibilities and potentials of sociology in analyzing, criticizing, publicly debating and perhaps even ameliorating various forms of ecological destruction has been intensively and

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the climatic domain. A summary report of this event is available from: http://stsclimate.soc.ku.dk/.

reflexively debated in the sub-field of environmental sociology. In this respect, environmental sociology may serve as an interesting 'laboratory' for observing wider shifts in sociological understandings of critique. Hence, from extensive Marxist discussions on the 'ecological contradictions of capitalism' in the 1970s, the 1990s onwards has witnessed a widespread endorsement of the normatively more modest Habermasian notion of 'deliberative democracy' (see, e.g., Szerszynski 1996; Dryzek 1997). At a less highbrow level, however, it seems safe to venture that most international environmental sociologists would continue to consider themselves allied – ideologically, practically, or both – to some version of the new environmental movements of the 1960s. In environmental sociological debates, issues of normative engagement are seldom far beneath the surface, if not explicitly on the table.[68]

What is far from self-evident, in environmental sociology as in other branches of sociology, is just how such normative engagement is to be conceptualized and practiced? In this respect, it would still seem commonplace in sociology to hark back on some notion of 'critical theory'; but from the vantage point of a pragmatist-inspired sociology, it is at best debatable what such a notion implies in analytical and normative terms? The concept of ideology, for instance, would seem highly problematic from an interpretive-pragmatic vantage point: if we take the critique of ideology to depend on a strict separation of perspectives, between the talking critic and the agent(s) talked about, this whole 'hermeneutics of suspicion' begins to look itself rather suspect, because it implies the existence of a transcendent standpoint available only to the critic (see Celikates 2006). Add to this the difficulties of finding a 'natural' vantage point from which to criticise ecological risks – which, as noted, signifies the implosion of the very nature/culture binary – and one begins to understand why Bruno Latour (2004d), in the context of climate change controversy, claims that the notion of critique has "run out of steam".

Such a critique of critique does not express some flimsy thought on the part of a French maverick; instead, building on Garfinkel's ethnomethodology and the philosophies of Deleuze (1994) and Serres (1995), together with Boltanski and Thévenot's sociology of justifications (2006), Latour (2005) consistently

68. Empirically, these remarks of mine are purely anecdotal, based on personal experiences of participating in environmental sociology workshops at both European (ESA) and international (ISA) levels.

rejects the idea that sociology should take a critically debunking stance with respect to the understandings of the people studied (see Cooper 2009). Indeed, exploring what it might mean for sociology to entertain an 'a-critical' stance, beyond the confines of the modern critical matrix, is arguably an integral part of this version of new French pragmatist sociology (Celikates 2006; Vandenberghe 2006). This does *not* imply, however, that this vision of sociology entails a stance of value-neutrality. Rather, it implies that sociology is in need of rethinking its own ethics and politics, in terms of finding new ways of engaging the social world *beyond* critique. The question is what these new terms of engagement might be; and, more specifically, what they might imply for my concerns in this thesis?

One fruitful way of opening up this discussion, already signalled in relation to the rendition of my sociological settlement (figure 2), is to think about the implications of arguments developed in STS under the rubric of 'ontological politics' (Mol 1999). The basic version of this argument is straightforward: science, in this line of thinking, is not simply about observing, describing or explaining phenomena, but equally serves to bring new phenomena into being, that is, to create new socio-natural realities. This is why I claimed that climate science is engaged in ontological kind-making; just as, in my articles on whaling, I will similarly claim that different knowledge practices bring about different ontological kinds of whales (see essays 2 and 4). In this context, the important point is that sociology is *also* engaged in ontological politics; indeed, whereas the argument may prove controversial for the natural sciences, this insight is in some sense old news to sociologists (Law & Urry 2004). Still, it is worth exploring what it entails at the level of the normative and political engagements of sociology and its methods.

If we allow ourselves to move freely through the history of sociology for a moment, a characteristic set of polarised responses to sociology's political engagements emerge, in internal debates and external responses (see Latour 2005; Law & Urry 2004). On the one hand, certain branches of the discipline, typically associated with Marxist radicalism, has tended to entertain hyperbolic ideas as to its own subversive potentials – and occasionally been credited such very potentials from actors on the political right critical of 'over-socialized' individuals.[69] On the other hand, laments to the effect that sociology merely discovers the already obvious; uses an impenetrable jargon of neologisms; and/or remains infinitely inferior in terms of power to the world of economics, seems to accompany a good deal of sociological soul-searching. In short, and using the provocative language of Latour (2005), sociological self-conceptions vis-à-vis its own social relevance seem to swing wildly in-between megalomania and impotence! What seems to be lacking, then, is a more realistic sense of the performative capacities of sociological inquiry, in participating in the ontological unfolding of socio-material realities.

This assessment of ontological politics may lead to any number of implications, depending on how one, in the final analysis, positions the 'objects' of sociology – meaning here both its analytical prerogatives and the way we envisage the social purposes of the discipline. One way of positioning my arguments in this thesis at this fundamental level is to say that, building on valuable analyses by the likes of Ulrich Beck, John Urry, Bruno Latour, and many others, I am arguing that the objects of sociology are currently changing, and this for good and legitimate reasons. Indeed, this is why this summary chapter has already invoked too many 'turns' in the disciplinary fork road – a practice turn, a material turn, a topographical turn and a mobilities turn, to recount the specifics. Now, to make things worse, the term ontological politics suggests the need for yet another neologism, this time in the shape of an 'ontological turn'. The assumption here is that what is known (epistemology) is also being made more or less real (ontology) (see Adkins & Lury 2009). What this means, according to Law and Urry (2004), is that sociological knowledge production participates in the actual making of social worlds.

Somewhere within this phrasing lies the assumption that ontology is not about a single reality, but deals rather with a multiplicity of *realities*, of socio-natural worlds, all legitimate but unlike each other (Law & Urry 2004; Law 2004). The universe, in short, is better seen as a 'pluriverse' – to borrow this term from William James, dovetailing well with the radical pluralism of French

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69. The infamous neo-liberal statement from then UK Prime Minister Margaret Thatcher to the effect that 'there is no such thing as society' springs to mind here. In the self-conception of many Danish sociologists, something similar is no doubt suspected to have been the case with the dismantling of sociology departments at the hand of a political right-of-centre Danish government in the 1980s (see Kropp & Blok 2009).

neo-pragmatist sociology (see Ferguson 2007; Latour 2005; Boltanski & Thévenot 2006). For Law and Urry (2004), extending arguments made by Nicolas Rose (1996) in the Foucaultian tradition, this line of thinking leads them to criticise the way sociology has historically been complicit in the enactment of a single, Euclidean, homogenized, and 'legislated' social territory, that of the nation-state (see also Bauman 1987). From now on, they suggest, we will "need to imagine a fluid and decentred social science, with fluid and decentred modes for knowing the world allegorically, indirectly, perhaps pictorially, sensuously, poetically, a social science of partial connections" (Law & Urry 2004:402). Again, while the language here is slightly too 'romantic' for my likings, this thesis is broadly intended along such lines – as indicated, indeed, in my adoption of Strathernian partial connections as a meta-analytical trope.

In his own version of ontological politics, which he terms 'cosmopolitics' from the work of Belgian philosopher Isabelle Stengers (2005), Latour (2005) adopts an analogous if slightly differently emphasised position on the objects of sociology. The Latourian test for political relevance is whether or not the sociologist is capable of regularly refreshing the ingredients making up the collective. This is why Latour, rightly I believe, is critical of critical theory: this branch of sociology is too drunk on 'powerful explanations', constantly reshuffling the same old limited set of ultra-causes – instrumental rationality, capitalist exploitation, ideological distortion, Empire, colonization of the life-world, and a few more. "Retracing the iron ties of necessity", Latour warns, "is not sufficient to explore what is possible" (2005:261). For sociology to participate in this cosmopolitical exploration of the frontiers of the possible, it needs to make sure "that the number, modes of existence, and recalcitrance of those that are thus assembled are not thwarted too early" (ibid.). Sociology, from now on, ceases to be branch of 'deconstruction' – defined by Latour (1993) as "destroying in slow motion" – and starts being engaged in the gradual composition of a good common world.

This thesis is an attempt to practice a-critical sociology along the lines of Latourian cosmopolitics, eschewing the drug of sociological necessities in favour of an open-ended exploration of socio-natural worlds-in-the-making. Nevertheless, exploring the *exact* implications of such a conception of sociology is bound to be itself an open-ended inquiry, towards which this thesis is only a preliminary contribution. My most explicit reflection on these issues take place in essay 4, where I interpret global whaling controversies through the lenses of Latourian cosmopolitics, in the process re-specifying this at the level of agonistic pluralism (Mouffe 2000). In other words, my invocation of cosmopolitics in the context of a real-world global conflict over socio-natures takes sociology in the direction of political philosophy – a direction, notably, in which Latour (2004b; 2007b), Boltanski and Thévenot (2006), and the wider field of science studies (e.g. Thorpe 2008) is also currently moving. Clearly, there is plenty of scope here for further exploration.[70]

These reflections are not meant to suggest, however, that the direction of empirical political philosophy should exhaust the multiple implications stemming from Latourian cosmopolitics for how we think about the conduct of sociology. On the contrary, these implications seem to me potentially to be so radically far-reaching as to take us in an exploding set of directions, and in any case well beyond the confines of this thesis. For one thing, echoing the philosophy of Michel Serres (see Latour 1987b), Latour intends his sociology of associations as participating to a wider intellectual movement beyond the parameters of modernity, defined in terms of individual autonomy, enlightenment, critique and progress. According to Latour's political philosophy of a-modernity (2004b; 2008b), we no longer live in the 'time of time' but in the 'time of space', implying that 'ecological' questions of entanglements, co-isolations, and peaceful co-existence will define our most important political passions well into the future.[71] For all his invocation of situated case histories, Latour thus does not shy away from providing a full-fledged panorama of where sociology might want to direct its own collective passions in the future.

The notion of cosmopolitics can also be said to entail far-reaching prescriptions at the level of practical inquiry, having to do with the implication

70. The entire sociology of critique developed by Boltanski and Thévenot (1999) can be read as 'empirical political philosophy', representing a promising agenda, in my view, for sociology. My thesis carries fewer traits of this thinking than I would retrospectively want, for the simple reason that I have only become aware of this work in the process of writing – another important 'practicality', as it were.

71. Underlying this statement is Latour's rather idiosyncratic vision of 'spiralling' time; he is not suggesting that time does not matter anymore, only that time does not move in the straight paths envisaged in the era of modernization (see Latour 1993).

that sociological attempts at refreshing the ingredients of collective life will often, perhaps routinely, *fail*. Sociological ‘experiments’, no less than natural scientific ones, stand at constant risk of loosing interest by reproducing some ready-made explanation. In fact, amongst contemporary philosophers of science referred to by Latour – notably Isabelle Stengers (2005) and Vinciane Despret (2004) – a bleak picture of social science emerges. Latour (2004a) summarizes this counterintuitive point by saying that, contrary to natural science objects that obstinately ‘objects’ to being studied, the discovery of most social science is that, “when impressed by white coats, humans transmit objectivation obediently”! This provocative point implies that, contrary to common wisdom, the road to good science, natural *and* social, is for the scientist to be passionately interested in the object of study, providing as many opportunities as possible for this object-subject to question the imposed categories of the researcher. As Latour acknowledges, in stark contrast to the way he is sometimes caricatured, this is a *very* demanding view of scientific practice – and one reinforcing, in this context, the importance of sociology taking his ‘new’ empiricism seriously.

The ‘experimental’ methods and protocols of sociology are of course quite unlike the ones employed in the natural sciences. Sociology is in the business of giving *textual* accounts of the social world, using a handful of fully artificial tools, from field notes to statistical databases (see Becker 2007). These accounts are *risky*, not only because they rely on long chains of fragile translations – say, from ethnographic observation to quickly scribbled field notes, on to an evolving argument invoking a few key concepts from some theory – but because the *specificity* of what is thus being translated can easily be lost along this trajectory. Social science objectivity, on this account, is a matter of the capacity to textually extend the ‘objectfullness’ of social events into the event of reading through the medium of the text (see Latour 2005:133). What this demanding view implies, at a minimum, is that the *way* in which we write our sociological accounts, far from being a ‘merely stylistic’ issue, in fact matters crucially to their validity, persuasiveness and possible effects. As always, the judgment as to how well my own texts manages to live up to such requirements is mostly in the hands of future readers.

Finally, it is worth reflecting briefly on what the notion of an a-critical sociology implies at the level of the affective and normative attitudes adopted by sociologists towards their objects of study. The notion of critical theory, we may note, has tended to prematurely pre-empt this question, since it suggest a default answer: the sociologist should adopt a *critical* attitude, which, according to most dictionaries, implies an inclination to criticise unfavourably. Critical theorists famously disagree amongst themselves as to the exact requirements of normative 'positivity' implied by their 'negative' judgments, but regardless of details, in analytical practice the attitude remains much the same. What the notion of a-critical sociology implies, then, is an invitation for sociologists to explore an entirely *different* register of normative-affective attitudes towards the social world. Critique, as it were, is brought down to size, and situated within a critical matrix of everyday grammars for how to build legitimate social bonds, gradually assembled in European contexts since the 17th century (see Boltanski & Thévenot 2006).

On this question as to what might supplement the repertoire of critique in sociology's way of imagining its normative-affective social relations, my sense is that the verdict is still out there. Feminist social theorists, including some science studies (STS) scholars, have valuably suggested that attitudes of empathy, care, love and solidarity are at least as crucial for the creation of valid knowledges as are attitudes of criticism (see, e.g., Mol & de Laet 2000; Haraway 1988, 2003; Despret 2004). I fully agree with this assessment, particularly as regards the notion of solidarity which, as American neo-pragmatist philosopher Richard Rorty has suggested (1991), should be seen as a crucial component of an anti-foundationalist epistemology, in terms of judging 'what is good for us to believe'. In line with Haraway's notion of situated knowledges (1988), this argument entails no 'relativism', but rather points to the way in which trans-local solidarity is an integral part of any scientific *and* non-scientific endeavour to challenge, defend and modify collective knowledge-claims.[72] In the absence of minimal solidarities, there can hardly be any trust in the knowledge of others; an important theme, incidentally, in my studies of how biological science *fails* in the arena of whaling conflicts (see essays 1 and 4).

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72. For a thought-provoking and somewhat more critical discussion of Haraway's notion of situated knowledges, including her rhetorical invocation of 'anti-relativism', see Herrnstein Smith 2006:23ff.

While I thus broadly sympathize with these feminist-inspired insights, I also think they come with limitations, given that sociology is obliged to deal with phenomena that it is hard to emphasize with, let alone love – symbolic and physical forms of violence, for instance (see Boltanski and Thévenot 1999). Such forms of violence, indeed, are implicitly and explicitly present in my case studies (see, in particular, essay 4). In such situations, we may be better off following the promising suggestion recently made by Japanese anthropologist Hirokazu Miyazaki (2004), of adopting *hope* as an important method in the social sciences. In a bold move, Miyazaki joins a strand of Marxist utopianism associated with the work of Ernst Bloch to the strong tradition of pragmatist meliorism, based on the belief that human action has the capacity to improve the world, in arguing for a temporal reorientation of knowledge towards an open-ended future (see also Green 2008). Just as Latour invites us to explore the frontiers of the possible, Miyazaki's invocation of hope helps put the object(s) of social science into new perspective.

By way of digression, this leads me back to the starting point of this section: how to position sociology vis-à-vis the kinds of ecological crises dealt with in this thesis, as well as in the wider field of environmental sociology? I do not think there will be one generally valid response to this question. Indeed, one implication of this discussion is that, just as we need to envisage a multiplicity of situated socio-natures rather than one all-encompassing Nature, discussions of ethics and politics in sociology from now on need to be conducted in more context-dependent and empirically engaged ways. The kind of ontological cosmopolitics envisaged by Latour and his science studies (STS) colleagues implies a situational and relational approach to the ethics of social science (Thrift 2003). Political relevance in sociology will need to rely on critical *proximity* rather than critical distance, as one amongst other contributions to the task of progressively composing better common worlds (Latour 2005). My own studies into divided socio-natures, ideally, should be evaluated along these lines of an ethically engaged proximity.

Having said this, the point about non-critical registers of affective-normative attitudes in sociological inquiry, particularly the notion of hope, also needs to be situated more carefully vis-à-vis the ecological crises highlighted in environmental sociology. Quite simply, when confronting an issue such as climate change known to have potentially catastrophic effects not least on vulnerable communities in the global South, speaking of hope as a sociological method may at first seem inappropriate. In this context, however, it is important to note that what needs countering is first of all a sense of *theoretical* hopelessness, or, in other words, what Latour calls the 'masochism' of critical theory in always envisaging a small handful of all-powerful structural enemies (Latour 2005:252; Miyazaki 2004). When engaging in some empirical proximity with specific socio-natural issues and their scientific, economic, political and cultural trajectories, as I do in this thesis with whaling and carbon markets, new layers of contingencies and future possibilities are likely to shape up, in respect both to sociological inquiry and political action. This, at least, has been an important guiding ambition throughout these studies.

At the same time, this ambition of wanting to explore some frontiers of the possible by way of a-critical sociology is fully compatible with an encompassing sense of limits, not just to any single piece of inquiry, but also in a more political sense. First, as concerns inquiry, public-political relevance should be viewed as a hard-won achievement, by no means as guaranteed.[73] This thesis provides useful illustrations of this point: whereas both of my empirical studies have indeed met with interest from a variety of public actors engaged in the respective assemblages, they have done so in unpredictable and sometimes troublesome ways. Hence, for instance, my studies into Japanese whaling has gradually brought me into contact with European anti-whaling NGOs, eager to claim this knowledge as support of their already solidified standpoints, even as I have actively strived to complicate this process of identification.[74] As for my carbon market studies, to the extent that Danish journalists have shown interest in 'sociological perspectives' on climate change, it has proven difficult to frame carbon market issues in sufficiently non-technical terms to make it palatable to a broader media audience. Even this

73. In recent years, debates on how to once again make sociology matter to contemporary social developments have been conducted amongst some of the disciplines' most prominent spokespersons under the heading of 'public sociology' (e.g. Burawoy 2005; Beck 2005; Abbott 2007b). I consider my reflections here to be broadly in line with these discussions, albeit with a slightly different focus.
74. Specifically, I wrote a research dissemination piece on Japanese whaling in a well-known Danish environmental NGO magazine, which turned out to give rise to interesting conversations, including a high-level parliamentary commission debate on the whaling policies of the Danish government in March 2008.

small anecdotal experience speaks to the sheer level of technical complexities encountered in the climatic domain, with attendant and serious problems of public disengagement.

By way of engaging empirically with on-going controversies surrounding two socio-natural matters of great public-political concern world-wide, this thesis turns out to be as much about conflicts, uncertainties and deep-seated differences as it is about the search for a common world or cosmos. Taking my studies as indicative in this respect, our current state of involuntary ecological togetherness in the 'time of space' is surely one of multiple fractures and conflictual potentials – with apocalyptic anticipations of global warming serving as an omnipresent imagery in public life (see Urry 2008). In this respect, I find it appropriate to end this paragraph on the good common world by once again quoting Richard Rorty. In proposing to replace knowledge with hope, Rorty makes the following comment, which may well be equally valid as regards sociological *and* practical political chances of dealing with a phenomenon like climate change:

> A pragmatist sense of limits requires us only to think that there are some projects for which our tools are presently inadequate, and to hope that the future may be better than the past in this respect (Rorty 1999:51f).

Summing up: themes to keep in mind while reading the essays

One way of summing up what has admittedly become a rather 'rhizomatic' argument with many branches, is to return to a designation introduced in the beginning of this chapter, that of John Law's 'modest sociology' (1994:8ff). What Law has in mind with this concept of modesty in many ways expresses the ethos of my thesis: different descriptions and theories in sociology, Law notes, will not easily add up to an integrated whole. This is both an ontological and an epistemological claim: different theories (epistemology) will keep applying equally well to the same complex reality; and changing realities (ontology) will keep necessitating new theoretical tools. The best way of reacting to this predicament, Law suggests, is to adopt a stance of modesty: we need to consider our own knowledge as at best a decentred, indirect and partially coherent way of describing a dynamic, relational, and pluralistic social world. Throughout this summary chapter, like Law (2003; 2004), I have referred to this stance of non-holism in the language of partial connections, borrowed from anthropologist Marilyn Strathern (2004).

As should be clear, however, modesty is not really the right word for describing the new mode of sociological knowledge making, which I have attempted to co-articulate in this chapter by way of engaging, most prominently, with the sociology of Bruno Latour. In fact, the following observation made by Peter Wagner (1999:343) on Boltanski and Thévenot seems to apply equally well to ANT: what is characteristic of these French neo-pragmatist sociologies is a combination of radicality and modesty. Like Boltanski and Thévenot, albeit in different ways, Latour is radical in his questioning of basic presuppositions of the social sciences, including the meaning of these very terms, the social and science (see Latour 2000b). At the same time, however, Latour (2005) is fairly modest in terms of the advances he claims to have made towards a future reconstruction of social theory. His claim that ANT is merely 'half Garfinkel and half Greimas' needs to be taken seriously in this context; as does indeed his admonition that ANT is not really a theory, but rather "a very crude method to learn from the actors without imposing on them an *a priori* definition of their world-building capacities" (Latour 1999a:20).

Indeed, in the end, ANT is perhaps best interpreted as a meta-theoretical seize-fire operation: a way of bracketing age-old methodological preoccupations in favour of a renewed empirical interest in a variety of rapidly changing socio-natural realities at the turn of the century. In the course of this chapter, I have touched upon several such methodological preoccupations, by way of invoking what Andrew Abbott (2001) describes as the methodological manifold of sociology. Most importantly, following Latour (1999b; 2005), I have positioned ANT as a powerful tool for bypassing the entire realism-constructivism debate in sociology, by having outlined a new understanding of what it is for an entirely socialized scientific fact to co-construct its own materially equipped realities. This new model of co-constructivism, simultaneously elaborated by original work in science studies on the dynamic co-evolution of science, society and nature (e.g. Jasanoff 2004b; Wynne 2002; Irwin 2001), in many ways epitomizes the implications of ANT for environmental sociology and beyond. I have summarised this approach as a post-risk society theoretical agenda for sociology – an agenda that still remains highly indebted to Ulrich Beck's seminal theorizing.

As one way of ordering the different contributions made to on-going conversations in sociology by the six essays constituting the body of this thesis, I have attempted in this chapter to abstract a model of the complex topography of different sociological contexts of knowledge-making (see figure 2). The way I intend this sociological settlement to be interpreted, it follows the same fractal logic that, according to Abbott, structures much of what goes on in the discipline. What this means is that regardless of the scale of attention – talking, for instance, about my particular articles; about the sub-field of environmental sociology; or indeed about the history of 'general' sociology – the model is meant to provide conceptual tools for (re-)interpreting some deep-seated presuppositions of social inquiry. In writing this summary chapter, I have found the model particularly helpful in visualizing the trajectory followed, and the territories traversed, in researching, analyzing and writing my essays (see figure 3). Before drawing this section to a close and commencing on the essays themselves, it may prove helpful to briefly reiterate some main lessons drawn under each conceptual heading of this settlement.

First, at the level of meta-theory, I have positioned Latourian ANT as a comprehensive and analytically fruitful approach to social inquiry, as summarised by a number of 'turns' in the landscapes of contemporary social theory – the pragmatic, the material, the topographical, the mobile, and the ontological. What these concepts amount to is a renewed sense of socio-natural realism, conceptualized at the level of a networked and hybridizing world of trans-local movement, flow and fluidity. The kinds of divided socio-natures dealt with empirically in this thesis in many ways epitomize such socio-ontological claims, inviting us to rethink habits of sociological theorizing pertaining to science-society relations. Whales and carbon dioxide are quintessential quasi-objects, the kinds of hybrid matters-of-concern currently reshaping contemporary societies (Latour 1993). While I simultaneously express reservations toward aspects of ANT methodology, in an attempt to 'sociologize Latour', ANT is thus positioned as a promising avenue for 'ecologising' sociology, promising to make it relevant to a collective understanding of global ecological conflicts in science and public life.

Second, at the level of methods, I have reflected on the conduct of global-scale ethnographic case studies, as represented by my studies into Japanese whaling (biodiversity) and carbon markets (climate change). Rather than engage in much in-depth discussion of techniques, I have attempted to position my practices of qualitative inquiry in a number of on-going debates on social science methods. These include debates on how to move sociology beyond methodological nationalism, by embracing a more mobile, multi-sited, and cosmopolitan imagination of the fields and assemblages under study. Further, I have reflected on the far-reaching implications of these arguments for how to think about case studies and their comparison, given the attendant 'crisis of context' (Schlecker & Hirsch 2001). I end this discussion by concluding that ANT-inspired sociology should be fully compatible with what Karin Knorr Cetina (1999), in a different context, calls a comparative optics of making contrasts visible across such trans-local domains. In making this argument, I criticise ANT for failing to elaborate an adequate understanding of the abstractions performed by social scientific concepts. Further, in a self-critical manner, I discuss the practical limitations that help explain why such a comparative optics is not fully employed to my two cases in this thesis.

Third, in terms of what I call social embedding, I have attempted to show some important practical implications for my studies of a more general feature of sociology: the historical, political and cultural over-determination of sociological objects of study. Environmental risks such as biodiversity and climate change provide striking illustrations of global reflexivity, in the sense that high-speed circulation of information serve to shape unequally distributed possibilities of different actors within these domains. This situation, I argue, create ambiguities for sociological analysis, because it is far from self-evident how to uphold a sense of analytical distance between sociological and other ethno-methods in such knowledge-dense domains (Riles 2000). Throughout the process of researching this thesis, theory, methods and data have been continually recast in response to this predicament, in attempts to carve out 'do-able' and interesting problems for sociological analysis. ANT, in my estimation, has proven an indispensable tool in this respect, in that it has allowed a (re-)casting of the main sociological problems as ones of human/non-human, culture/nature, and politics/science relations. Reading across my six essays, this on-going work of framing and re-framing analytical problems will be evident.

Fourth and finally, invoking on-going discussions in the borderlands between STS and sociology (e.g. Law & Urry 2004), I ended this summary chapter by reflecting on the ontological politics of sociology. Issues of ethics and politics are never far from view in environmental sociology, and sensibly so, given the potentially catastrophic stakes for all of socio-natural life manifested in problems like climate change. Nevertheless, alongside Latour (2004d) and others (Boltanski & Thévenot 2006), I argue that 'critical theory', together with notions of deconstruction, has proven largely inadequate as an attractive ethical-political self-conception for contemporary sociology. The a-critical alternative hinges, first of all, on a new conception of the political relevance of sociology as a hard-won achievement, tied up with a demanding view of the specificity of knowledge. Second, I explore what might be won, at the level of affective and normative attitudes, by supplementing the notion of criticism with solidarity and hope, as being important methods of social science knowledge making and public engagement. In brief, critical proximity and not critical distance serves as the guiding ambition of this thesis.

To end on this note, it is my contention that what has just been summarised in terms of meta-theory, methods, social embedding and ontological politics, when added together, provide a fruitful answer to the problem posed in the opening section of this chapter: *how to device a new mode of sociological inquiry adequate for, and relevant to, an understanding of the many conflictual science-dominated ecological issues proliferating in current-day societies?* Any judgment as to how convincing this claim will be, however, hinges to a large extent on the articles that follows. To reiterate, each of these articles represent its own situated engagement with particular aspects of this wider problem, without claiming to exhaust the fields of possible knowledges. Only by reading across the essays, as I have done in this introduction, do we get a sense, however partial, of the wider pattern of how sociology could conceive its own future role as participant to co-constructions of science, society and the global environment. It is my hope that fellow sociologists – and others – will find enough of value and inspiration here to pick up the thread, weaving denser pictures of our state of precarious ecological togetherness. For all the valuable existing contributions to this task, to which my own studies owe a great deal, the larger part of this endeavour, I contend, remains ahead of us.

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