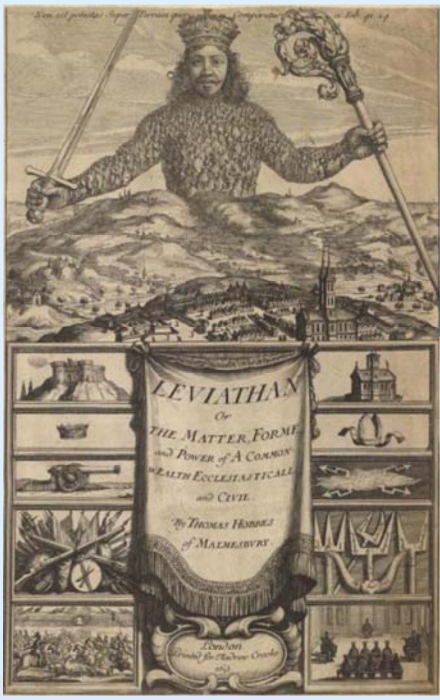
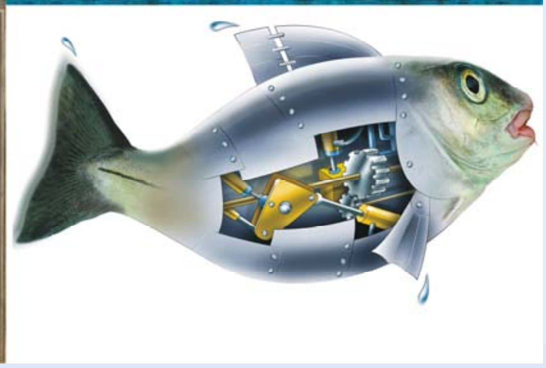
Science|Politics:

Boundary construction in mandated science

The case of ICES' advice on fisheries management

Kåre Nolde Nielsen





A dissertation for the degree of philosophiae doctor

UNIVERSITY OF TROMSØ
Norwegian College of Fishery Science
Department of Social Science and Marketing

Autumn 2008

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Frontpage pictures:

I (left): From Thomas Hobbes, *Leviathan, or, The matter, forme, & power of a common-wealth ecclesiasticall and civill*. London : Printed for Andrew Crooke, 1651 (copyright expired). II (top right): R/V *Jan Mayen*, photograph by Bjørn Gulliksen. III (lower right): *The Cyborg Fish* by Bjarne Stenberg – with kind permission from Jahn-Petter Johnsen (see http://www.cyborg-fish.net/).

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Acknowledgements

If this work were to be a true Actor-Network Theory story, I would start by crediting all the anonymous *non-humans* that have helped me so much: my devoted and hardworking computer, the Institute's steadfast coffee-machine, and so on. Admitting my anthropocentric bias, however, I will here only express my appreciation to the human nodes of the networks that made this project possible.

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Kåre Nolde Nielsen, Tromsø, October 2008

Project Summary

What is the relationship between science and politics? What should it be? How are their respective roles conceived and acted out in practice? Should science and politics be clearly separated? How? Are there cases in which they can be usefully mixed? What cases are those, and are there some ways of mixing that are better than others?

This project explores such questions by examining relationships between ICES fisheries advice and decision-making in fisheries management. While traditional conceptions portray science as a rather autonomous entity, this case examines a context in which such conceptions are particularly prone to be challenged. *First*, scientific advice that forms direct inputs into policy-making is better characterized as 'research' or 'mode-2 science' than as autonomous science. *Second*, advisory science on fisheries management is 'post-normal' insofar as its knowledge claims are uncertain, values disputed and decisions urgent. Here, fact and values easily become entangled, which in turn challenges conceptions of autonomous science. How is the science-politics boundary constructed here?

Since ICES advice constitutes the formal and highly important link between science and politics in fisheries, it offers a concrete location for studying boundary dynamics. Although this project mainly mobilizes Science and Technology Studies theory (in particular Actor Network Theory) it not only seeks to contribute to this literature, but considers how insights generated from such perspectives may contribute to the ongoing discourses on fisheries science and management.

The thesis demonstrates that a comprehensive understanding of the construction, maintenance and transgression of the boundary between fisheries advisory science and management cannot be limited to studies of 'boundary work' as discursive practices; it also requires examinations of practices in scientific knowledge production, the material embodiment of this knowledge, its use in policy-making, and conditions on which its stability depends. Forms of uncertainty in ICES fisheries advice are explored and are demonstrated to challenge conceptions of a clear-cut science-politics boundary. The thesis proposes ways in which the science and politics of fisheries can be reconsidered by the development of a framework for enabling evaluations of fisheries management systems. This is expected to enhance communication across disciplines concerned with fisheries management, and to promote systemic learning.

List of papers

Paper 1: Nielsen, Kåre Nolde, and Petter Holm. 'The TAC Machine: On the Institutionalization of Sustainable Fisheries Resource Management'. Unpublished manuscript.

Paper 2: Nielsen, Kåre Nolde, and Petter Holm. 2007. 'A brief catalogue of failures: Framing evaluation and learning in fisheries resource management.' *Marine Policy* 31: 669-680.

Paper 3: Schwach, V., D. Bailly, A.-S. Christensen, A. Delaney, P. Degnbol, W. van Densen, P. Holm, H.A. McLay, K.N. Nielsen, M.A. Pastoors, S.A. Reeves, and D.C. Wilson. 2007. 'Policy and knowledge in fisheries management: a policy brief.' *ICES Journal of Marine Science* 64: 798-803.

Paper 4: Nielsen, Kåre Nolde. 2005. 'Risking Precaution: Framing Uncertainty in Fisheries Advice.' Unpublished manuscript.

Paper 5: Hauge, K.H., K.N. Nielsen, and K. Korsbrekke. 2007. 'Limits to transparency—exploring conceptual and operational aspects of the ICES framework for providing precautionary fisheries management advice.' *ICES Journal of Marine Science* 64: 738-743.

Paper 6: Holm, Petter, and Kåre Nielsen. 2007. 'Framing fish, making markets: the construction of Individual Transferable Quotas (ITQs).' in *Market Devices* edited by Michel Callon, Yuval Millo and Fabian Muniesa: Blackwell, Oxford.

> Instead of the stuffed scientists hanging on the wall of the armchair philosophers of science of the past, we have portrayed the lively characters, immersed in their laboratories, full of passion, loaded with instruments, steeped in know-how, closely connected to a larger and more vibrant milieu. […] Who loves the sciences, I asked myself, more than this tiny scientific tribe that has learned to open up facts, machines, and theories with all their roots, blood vessels, networks, rhizomes, and tendrils? Who believes more in the objectivity of science than those who claim that it can be turned into an object of enquiry? (Latour 1999: 2-3)

1. Introduction

What is the relationship between science and politics? What should it be? How are their respective roles conceived and acted out in practice? Are politics and science both at their best if they are separated from each other? How should this be done? Are there cases when political decision-making and scientific knowledge production usefully can be mixed together? What cases are those, and can we identify models for mixing that are better than others?

Such questions become increasingly pertinent as science and technology in so many forms proliferate in our societies and contribute to their transformation. Just think of climate sciences, research on GMOs and nanotechnology, green technologies, resource economics and the anthropology of indigenous cultures. In these examples science is very close to value-laden questions and to political decision-making. But isn't science about being objective, about providing solid and dry facts, about creating a disinterested gaze? Indeed we can identify this as an ideal that, among other things, goes under the label of 'pure science' (Daniels 1967). As these examples may suggest, however, the relationships between science and society are as intriguing as they are complex.

In this project, I explore questions such as those with which I began within the world of fisheries science and management. I do that by examining some of the complex relationships between the science of fish stock assessment and political decision-making in fisheries management (Alcock 2004; Finlayson 1994; Wilson and Degnbol 2002). Indeed we shall learn something about how fisheries assessment scientists, 'immersed in their laboratories, full of passion, loaded with instruments, steeped in know-how', are ‘closely connected to a larger and more vibrant milieu’, as Latour, with characteristic enthusiasm, puts it in the quote at the beginning of this chapter.

Fisheries assessment science offers a rich case for studying the processes of science-politics boundary construction because it represents a context in which a traditional conception of science, namely that it represents an epistemologically autonomous entity in society, is particularly prone to be challenged. An indication of this challenge is associated with the generally high level of controversy in fisheries, particularly in a crisis context, such as that of EU’s demersal fisheries (Delaney, McLay and van Densen 2007). Here fisheries assessment science is frequently disputed by various actors, notably by (representatives of) fishermen, who are directly affected by the management decisions, which in turn are informed by the scientific advices that are based on stock assessments.

A preliminary consideration of what characterizes traditional conceptions of science and how these, in a more general picture, may be challenged in our time will help with the illumination of the project’s theoretical interests. Traditional conceptions of science involve a clear ‘demarcation’ of science from other social activities. Within what is now textbook philosophy of science (e.g., Lakatos 1978; Popper 2003), this conception is based on normative methodologies for science, developed from epistemological reasoning. Similarly, within traditional sociology of science, this conception is derived from ideal-type descriptions of the social norms of science (Merton 1996). In these pictures, science is seen as a rather autonomous social activity, indeed a republic (Polanyi 1962). These pictures portray a clear division of labour in which science provides facts and informs political decision-making. A flow in the opposite direction, from politics to science, would be regarded as intrusive.

Two recent developments in science and its relationship with society at large pose a challenge to this picture of an autonomous science. The first development is what Latour (1998) characterized as a transition from ‘the world of science to the world of research’. Much in parallel, Michael Gibbons, Helga Nowotny and colleagues (Gibbons et al. 1994; Nowotny, Scott and Gibbons 2001) portray a shift from Mode-1 to Mode-2 science. Here, Mode-1/science identifies the autonomous pursuit of knowledge as captured by Michael Polanyi’s image of ‘the Republic of Science’ (Polanyi 1962). This is in contrast to Mode-2/research in which scientists, commercial interests, government representatives and clients/customers are in close interaction. The second development is addressed by the notion of the 'risk society' (Beck 1992) which demands that society must come to grips with scientific and technological risks. Knowledge about complex and semi-open systems such as our environment can only be limited. Environmental sciences are what Funtowics and Ravetzs (1993) term 'post-normal', which is when 'facts are uncertain, values in dispute, stakes high and decisions urgent'. The concept of post-normality, hence, identifies situations in which normative and epistemological issues are intrinsically entangled.

The case of fisheries is well suited for examining how these challenges to the traditional conception of a clear science-politics boundary are met in practice, and for examining how the boundary is constructed in practice in such a context, for several reasons. *First*, the assessments, and the advices they support, are more appropriately characterized in terms of 'research' or 'mode-2 science' than in terms of autonomous science since these form direct inputs into policy-making. *Second*, since the science of fish stock assessment is uncertain, fact and values easily become entangled. *Third*, since the annual advice on fisheries constitutes the formal and overly important link between science and politics in fisheries, the case offers a concrete location to study their boundary dynamics.

This project not only seeks to contribute to the academic literature on social studies of science, however, but actively considers how insights generated from such a perspective may contribute to the ongoing discourses on fisheries science and management. Hence, the project may in itself be lodged in the vibrant boundaries between abstract science and practical politics.

The aims of this introduction are: *first*, to characterize the project's identity; *second*, to position my work in a theoretical and a practical context; *third*, to develop a theoretical framework for my research; *fourth*, to present my contributions, and to discuss how they are related and how they respond to the overall research objectives; and *fifth*, to present the main findings of this work and to reflect on their significance. I will proceed as follows:

In *Chapter 2*, I characterize the project's identity in terms of its objectives, composition, methodology and materials. I briefly introduce the chosen case of fisheries science and management, and I position my work in relation to relevant works in the academic fisheries literature. I explain how I have addressed my exploration of science-politics interactions in this context, and how I have worked empirically.

Chapter 3 puts the issue of the science-politics boundary into a historical perspective. As others have done before me, I track the origin of the ideal of a clear-cut boundary of this sort to discussions in ancient Greece in which Plato invoked a distinction between knowledge and opinion. This demarcation, together with a certain technocratic disposition to which it was linked for Plato, was opposed by a range of 'sophists' who were arguing for a relativist's conception of knowledge and a pragmatic view of knowledge relevant to political decision-making. This is why, in Latour's (1999) account, this scene is a precursor of the modern 'science wars', except that modern science in the latter replaces philosophy as the foremost knowledge authority, which entails that the *laboratory* replaces the philosophical argument as the device that enables a separation of knowledge and opinion (Stengers 2000).

In *chapter 4* I present and discuss a selection of contemporary theoretical conceptions of science's boundaries that appear to constitute 'obligatory passage points' (Latour 1987) for a student of such boundaries. Inspired by Gieryn (2003), I divide these conceptions into: (a) 'essentialist' which sees the boundary as 'inert', and (b) 'constructivist' which regards it as a dynamic site of exchange. The essentialist position are divided further into; *first* those focused on epistemology, including Popper and Lakatos (philosophers of science), Kuhn (a historian of science), and Feyerabend ('anarchist of science'); and *second* Merton's sociology of science, focusing on scientists' social norms. The forward moving narrative in this succession of positions is generated by what I see as limitations for each of the approaches I discuss.

I continue with an introduction of Gieryn's useful notions of 'boundary work' and 'cultural repertoires', by which issues of epistemology, demarcation and science's social norms are recast as empirical questions. Accordingly, this can be seen as a shift from an interest in designing the ‘division of labour’ to an interest in examining ‘the labour of division’.[1]

Gieryn’s notions, however, do not take into account the laboratory’s role in enabling a practical separation of knowledge and opinion. Therefore I turn to the laboratory studies of Actor Network Theory (ANT), and in particular to a discussion of Latour’s (1993) interpretation of science’s boundaries. Combining insights from Gieryn and (Latourian) ANT, I develop a framework for (studying) ‘co-production’ (or co-evolution) of science and politics. This framework is reflective insofar it explicates the interpreter’s role.

In the short *chapter 5*, I expound the reasons for the further development of this ‘co-production’ framework by addressing two recent developments in the relationships between science and society at large and by which traditional conceptions of autonomous science are challenged. As mentioned, these are; *first*, what Latour (1998) described as the shift from ‘the world of science to the world of research’; and *second*, what Funtowics and Ravetzs (1992; 1993) characterize as the emergence of ‘post-normal’ science. While Latour (1998) argues that there as yet is no philosophy of research, the ‘political epistemology’ that Funtowics and Ravetzs initiate is still at its beginning. Following Mol (2003), I suggest that such philosophies could be labelled ‘empirical’ since their challenge would be to take contemporary relationships between research and society into account.

In *chapter 6*, I integrate the papers presented here (see the list above) into the theoretical framework outlined in the previous chapter and discuss how they have helped in the exploration of issues pertaining to the science-politics boundary in mandated science.

Chapter7 summarizes some of the main theoretical and practical insights generated by this project, and offers some reflections on their significance. The thesis demonstrates that a comprehensive understanding of the construction, maintenance and transgression of the boundary between fisheries assessment science and fisheries management cannot be limited to studies of ‘boundary work’ in terms of discursive

[1] Here I borrow Robert Cooper’s notion of ‘labour of division’ (Munro 1997) without committing myself to his specific use of it.

practices; it also requires that the specific practices of the scientific knowledge production, its material embodiment, and its use in policy-making are examined, including the conditions on which the stability of this knowledge depend. Forms of uncertainty prevalent in contemporary fisheries management are explored and are demonstrated to offer particular challenges to traditional conceptions of a clear-cut boundary between the science of assessing and forecasting fish stocks and the management of fisheries based on Total Allowable Catches (TACs). The thesis proposes ways in which the science and politics of fisheries can be reconsidered. One such way would be the development of a framework for enabling evaluations of fisheries management systems. Such a development would enhance communication across disciplines concerned with fisheries management, and would promote systemic learning.

2. A characterization of the project's identity

This project is theoretically and empirically complex, which I suspect is reflected in the length of this introduction. As mentioned above, I will not address the integration of the papers before I return from the theoretical expeditions of chapter 3 and 4. In order to help the reader to get a grip on what this project is all about before this integration, I here characterize its identity in terms of its objectives, composition, and underlying theory. I introduce my empirical case, I explain how I have worked with it, and I introduce the empirical resources on which I have relied in so doing. Moreover, I position my work in relation to a range of relevant works in the academic literature on fisheries management.

2.1 Thesis composition and theoretical framework

My thesis work is represented by the papers that are listed above (p 3) and this introduction, which integrates and discusses them in a common framework. Compared with a monograph, which is the alternative format for a PhD thesis in Norway, a collection of papers comes with some advantages, but also a range of drawbacks.[2] To a

[2] The sheer length of this introduction and of the unpublished papers may suggest that this work perhaps is more appropriately characterized as a collection of papers with monographical aspirations. In so far as it is such a hybrid, my work demonstrates that it is possible to obtain the worst of both worlds, while the possibility of having the best of them may remain unsettled (see also note 15).

novice researcher, experience in writing papers for peer-reviewed journals is valuable since this form of communicating research is dominant today. Working on this project, I have found that the process of writing and submitting papers has provided an invaluable opportunity to reach a scientific community and to test out ideas within it. It goes without saying, however, that the performance of a collection of papers, when viewed as a whole, is inferior to that of a monograph. The writer may often be forced to throw interesting observations overboard in the final slimming of a paper – observations that otherwise could fruitfully have been included, analysed and discussed in a monograph. Moreover, overlaps and redundancies are unavoidable in a collection of papers – indeed the lack of them could indicate a low degree of cohesion between papers.

Most of the papers that are presented here (paper 1, 2, 3, and 5) are intended as contributions to the body of literature that we can think of as the 'academic fisheries literature', and thus represent contributions to the project's practical objectives (see below), namely to contribute to the existing discourses on problems, dilemmas and challenges in modern fisheries management. Paper 4 and paper 6 are in turn directed towards more theoretically-minded audiences within the social studies of science and such.

The challenge of the paper collection format is to integrate the papers into a whole. Given that the presented papers have quite different ambitions and audiences, this task could at the outset appear doomed to failure. The reason I think this integration is possible is that my way of thinking and working from the beginning of this project has been inspired by Actor Network Theory (ANT)[3] (e.g., Callon 1986; Latour 1987; 1988; 1993; 2005). Instead of limiting the sociologist's enquiry to 'social dimensions' of a problem, ANT can, as we will see, help us to explore linkages between the science, technology and politics of fisheries management. In particular, ANT can help us to explore how specific knowledge technologies, here fish stock assessment methods, 'mediate' (Latour 2005) science-politics relationships. The term 'mediate' implies that these methods contribute both to sustain and to transform these relationships. As will be apparent, it has been especially helpful to think of modern fisheries management in terms

[3] While this thesis will not provide any extensive introduction to ANT, chapter 4 offers some discussion on it. The reader may turn to footnote 81 for a brief summary on the ANT version on which I rely here.

of ‘heterogeneous networks’ (Holm 2000) in which ICES assessment working groups form ‘centers of calculation’ (Latour 1987).

While references to the STS[4]/ANT literature in the papers that form contributions in the fisheries literature are sporadic, I find that this owes more to journal genres and editorial choices than to the subject matter of papers. Although the emphasis and framing of problems differ between these literatures, my take on the subject matter (i.e., the science-politics boundary) is not incongruent between the papers. Just as it is not possible to import technical concepts that are well known to readers familiar with the fisheries literature into the literature of social studies of science without proper explanations, it would not be possible to import ANT notions into the fisheries literature without extensive introductions. While I have benefitted implicitly from analytical concepts and insights of the ANT literature, I have generally not found it necessary to discuss these explicitly in the fisheries literature. In this introduction, I will nevertheless make up for this. Based on the ANT-inspired theoretical framework that I develop in chapter 4, in chapter 5 I will indicate how the presented papers can be translated into the language of STS.

2.2 Objectives

The theoretical objective of this project is to study processes of boundary construction between mandated science and politics in a context of post-normal science. Here, ‘mandated science’ identifies science that is used as a direct input to policy-making. Post-normal science is when ‘facts are uncertain, values in dispute, stakes high and decisions urgent’ (Funtowicz and Ravetz 1993), and is characterized by entanglements of facts and values. We examine if and how norms associated with conceptions that posit science as a rather autonomous entity in society are challenged in such a context, and how the science-politics boundary is constructed, maintained and/or transgressed in practice.

To examine all this, we deploy a case study of fisheries science and management, which brings us to the project’s practical objectives, which are to contribute to the existing discourses on problems, dilemmas and challenges in modern fisheries management. This will be pursued from the perspective of social studies of science, as

[4] STS stands for Science and Technology Studies.

explained with a focus on science-politics interactions. As we will see, however, this focus will open broader deliberations on contemporary of fisheries science and management.

Starting with the theoretical side, the research objectives, we will now address these objectives more closely in relation to the case study. Against the background of the high level of stakeholder dispute in regard to scientific assessment and the management decisions they inform, the production and maintenance of a boundary between science and politics in fisheries may at the outset appear impossible. Fish stock assessments are, for many obvious reasons, highly uncertain, and fisheries management decisions are often urgent, while the values that relate to them are in dispute. In combination we can expect this to render the science of fish stocks assessment vulnerable to dissent. In the extreme, we can even imagine a situation in which uncertainties and controversies proliferate, such that the advisor's authority is undermined to the extent that it ceases to be recognized as a *scientific* advisor. In contrast, we can also imagine a context in which the recommendations of science are followed to the letter in the absence of public controversy, political negotiation and intervention. This would represent a technocratic management form in which 'politics' is silent. Fisheries management can be placed somewhere on an imaginary axis connecting these extremes. Decision making in fisheries management is institutionalized as a responsibility of democratically elected representatives who work through centralized bureaucratic systems, and whose decisions are supported by recommendations from experts who are broadly recognized as scientists. While the level of controversy and dispute is high, fisheries hence presents a context in which centres of 'science' and 'politics' comprise recognizable topological features. The question I examine here is how the roles of science and politics in practice are linked and separated in fisheries management. How are their respective tasks divided? Which norms guide this division? Which devices, that is, technologies of representation and intervention, enable and constrain it? How does it work? Can we characterize its practical potentials and drawbacks?

The latter questions bring us back to the practical side of the project's objectives, namely, how the exploration of science-politics relationships may contribute to the existing academic discourses on fisheries management. Once the case study and my way of working with it have been introduced, I pursue this further when I position my work in this literature.

2.3 The case of ICES advice on fisheries management

This project (Holm and Nielsen 2003) was designed as a study of science-politics boundary construction processes in the context of 'mandated science', that is, contexts in which scientific knowledge production is used directly in policies. Previous studies of the science-politics boundary in mandated science have, for instance, addressed bio-chemical research involved in determining and evaluating health impacts of additives in food, herbicides and discharges from chemical industry production (Jasanoff 1990), climate science (Lövbrand 2007; Shackley and Wynne 1996), but also advisory science, produced as an input to fisheries resource management (Finlayson 1994; Wilson and Degnbol 2002).

The stage of science and politics in modern fisheries resource management

I cannot think of a better way to introduce the stage of modern fisheries management than to give the word to Poul Degnbol, who succinctly writes:

> Modern fisheries biology has developed in close association with a management system characterized by both centralized decision-making based on numerical control of input output parameters through top-down control structures and by an explicit emphasis on resource conservation. Contemporary fisheries biology provides the cognitive basis for this system through stock assessments, which are basically predictions of short and long-term effects on stocks and yields given by various scenarios based on statistics. (Degnbol 2003: 32).

This thesis addresses a significant instance of the co-evolution of science and management, namely the introduction of Total Allowable Catches (TACs) as the main management instrument in the North Atlantic, and the assessment methodology and data infrastructure that supports TAC decision-making. The annual TACs are decided on the basis of short-term predictions (catch forecasts), in which a range of optional TACs are associated with predicted effects on the stocks, conceptualized in terms of certain key parameters, namely fishing mortality (F) and Spawning Stock Biomass (SSB). The TACs are central to the form of fisheries management we characterize as 'modern', and which is intensive on both science and management. The characterization of this management system[5], which we term 'the TAC Machine' (paper 1), and to which we will return throughout this thesis, enables us to undertake closer studies of contemporary science-politics boundary construction processes.

The empirical focus in my work is on the International Council for the Exploration of the Sea (ICES), which is 'the organization that coordinates and promotes marine research in the North Atlantic'.[6] ICES provides scientific advice to national and international bodies that manage resources in the Northeast Atlantic, including the North-East Atlantic Fisheries Commission (NEACF)[7], the Joint Norwegian-Russian Fisheries Commission, and bodies within the EU. ICES's annual advice on fish stocks is the product of a long process of collecting and standardizing data from landings and scientific surveys, of using this data in stock assessment models, and of reviewing the assessment and formulating advice. The stock assessments are produced within a range of specific assessment Working Groups of ICES, and they are reviewed by ICES's Advisory Committee of Fisheries Management (ACFM). Based on a standard format, i.e., ICES's Form of Advice, the ACFM formulates the ICES advice. The advice is then handed over to 'management bodies' where it forms the basis of policy decisions. Within the EU, for instance, the ICES advice is reviewed again by STECF.[8] Then the Commission drafts a proposal for policy decisions that is either adopted or rejected by the Fisheries Council, which consist of the fisheries ministers of each member state. Finally, a proposal is adopted[9], and the TACs are divided among countries, and subsequently fleets and vessels, and enforced. Constrained by their quotas, fishermen land their catches. The registered catches in turn comprise a major input to the next year's assessments and catch forecasts.

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[5] The reader may turn to footnote 50 for a brief presentation of my use of the term 'system'.
[6] http://www.ices.dk/aboutus/aboutus.asp (visited 12.11.07)
[7] http://www.neafc.org/ (visited 12.11.07)
[8] Scientific, Technical and Economic Committee for Fisheries. (http://ec.europa.eu/fisheries/cfp/governance/stecf_en.htm - visited 13.11.07).
[9] This is a simplification that leaves out other political bodies and processes, such as the COREPER (http://www.dip-badajoz.es/eurolocal/entxt/eu/institut/coreper.htm - visited 11.03.08), The European Parliament, lobbying activities, and so on.

ICES advice constitutes the formal – and I think it is safe to add – the overly important link between science and politics in fisheries management. This does not mean that all fisheries science is represented in this report, nor that all fisheries politics is about making decisions based on it. Much science and politics is produced in various fora outside this frame. The transmission of ICES advisory reports to the various representatives of the Common Fisheries Policy (CFP) is therefore not the only but, so to say, the *organized* and overly important contact point between science and politics. The present institutional framework for fisheries resource management, hence, offers a concrete site to study boundary construction processes, namely as focused on the ICES advice on fisheries management.

2.4 Genealogies: A call for effective histories of modern fisheries management

As Degnbol shows, history can be mobilized in a critical and effective exploration of the present. Taking off from the developments of fisheries science around the beginning at the previous century, Degnbol provides a concise overview of the history of the close association of the developing fisheries science and management through which the paradigm of modern fisheries management – characterized by stock predictions, centralized decision-making and associated control measures – emerged. Degnbol draws attention to how a transformation in the dominant research perspective, characterized by internationalization and formalization of the research base, was linked to the development of management institutions. He argues that research in this process shifted from a focus on spatial resolution and diversity to a focus on measuring a set of key parameters in order to quantify fisheries resources in terms of populations (i.e., stocks) on a large scale – a form of knowledge that was suitable to centralized decision-making.

Degnbol's paper forcefully narrates the background for a particular dimension of the challenges we face in fisheries management today.[10] In Foucault's terms (Dean 1994; Foucault 1977b), I consider Degnbol's paper a 'critical and effective history'. It is a history of the present, a *genealogy*, which enables the mobilization of a critique (Foucault

[10] As Degnbol's title ('Science and the user perspective: the gap co-management must address') indicates, this perspective concerns the alienation of users (i.e., fishermen) from knowledge production and decision-making in modern resource management.

1977b). The ideal of such a history is not to be critical in the sense of presentism, i.e., rebuking the past in the light of present ideologies. Nor can a history be objective in a positivistic sense. The genealogy is in a double sense a history of the present; situated in the present, it seeks historicize features of the present. In another context, Foucault said that a critique 'is a matter of pointing out on what kinds of assumptions, what kinds of familiar, unchallenged, unconsidered modes of thought the practices that we accept rest' (Michel Foucault 1981, cited in: Rabinow and Rose 2003). The motivation for writing an effective history is to get a grip on the conditions that shape the present in order to enable its transformation.

In my view, much of the effectiveness of Degnbol's (2003) story stems from the fact that it considers the development of science and management together, with a sharp eye on the technologies of measurement and control that mediate and support them.[11] Unfortunately, I am concerned that Degnbol's 18 pages stand rather alone in this respect. While this statement is a bit crude and will be qualified below, I do find that the fisheries literature is in need of more accounts of the co-evolution of technologies of fisheries management and the politics of fisheries science.[12] How did fish and fisheries become 'manageable', and what can that tell us about the challenges we face in fisheries management today? Why did the scientists' ability to forecast short-term stock developments become so important? And, hence, why is assessment uncertainty such a pervasive and pertinent issue today?

A range of historians have worked on the histories of fisheries science. Tim Smith (1994) offered a knowledgeable history of 'the science of measuring the effects of fishing, 1855–1955'. Helen Rozwadowski (2002) wrote an excellent history of 'a century of marine science under ICES' (1902–2002). Vera Schwach wrote a fine history about the development of the Norwegian Institute of Marine Research (1860–2000). Christensen and Hallenstvedt (2005) wrote a first-rate history of the first 75 years of the Norwegian Fishermen's Association (1926–2001), which pays attention to changes in

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[11] As such it can be regarded model of my work (see below). In addition, Degnbol's paper exemplifies a point I made earlier. While his audience is the fisheries academia, his paper could, by a shift in language and emphasis, readily be translated into a paper for, say, Social Studies of Science. Once we are beyond the Science Wars (see below) there is nothing preventing a biologist from being a better sociologist than the average professional!

[12] Kristin Asdal's (2004) account of how air pollution emerged as a manageable object in Norway, and of the political technologies this involved, represents an example of this sort of work in a different context.

science and management insofar as they influence the roles and identity of the Association.[13] These are all sound works, and they have been of great use to me. It is not within their main scope, however, to explore the co-development of science, politics, and their instruments, which I think reduces their effectiveness as a means to understand the challenges of the present regime.

As mentioned, in this project we will propose that the modern fisheries management system in the Northeast Atlantic can be characterized as a 'TAC Machine'. We describe how the TACs and the year-class structured single stock assessment models jointly appeared and proliferated in the decade from 1965 to 1975, and we suggest that they mutually supported and promoted each other in this process. After the new Oceans Regime of the late 1970s, stock assessments and TACs were parts of the annual routine for managing most commercially important stocks in the Northeast Atlantic.

Because I will talk a lot more about the TAC Machine, it is important for me to note that the attention it receives does not imply that I have lost focus on science-politics boundary construction processes. To see why this is so, we have to take a preliminary look into issues that will be explored further in a theory chapter (i.e., chapter 4) that follows. The ingredients of the TAC Machine, e.g., the quotas as a control measure on fishing and the catch forecasts that support TAC decision-making may at the outset appear to be 'mere technical' instruments and hence uninteresting to a sociologist. At least we would expect such instruments to be ignored by Latour's (2005) caricature, the 'sociologist of the social', who shuns the examination of technological issues in order to focus on the 'social perspectives' of the issues in question. In contrast ANT teaches us how such instruments can be carriers of (and mediate) cognitive, moral and political precepts. For the 'sociologist of associations' (Latour 2005), technology is social; it is 'society made durable' (Latour 1991). The organization of fisheries management around the TACs and the stock assessment methods that sustain them entails a new way of representing fish and fishermen, for intervening in them, and for disciplining them. It comes with a particular natural-social order, including a particular distribution of tasks between science and management that respectively represent the nature and the social.

[13] Some of the changes in the Association in this context are discussed in paper 6.

The TAC Machine is at once a device for representing, organizing and managing science, politics, nature, and resource users. These features come together and are preferably studied together. The primary but much distilled conclusion of this project is that a more profound understanding of science-politics boundary construction and boundary dynamics requires that we do not limit ourselves to studying ‘organizational dimensions’ and discursive ‘boundary work’; we also need to take devices such as the one described here into account. To convince you that this is so, I need to unfold a long, but, I hope you will agree, also interesting story.

Given the pervasiveness of TACs and their accessories in modern fisheries management in the Northeast Atlantic and elsewhere, we would expect their role in the present regime to be well described, and the history of their introduction and proliferation to be well covered. To our surprise this is not so. Degnbol’s (2003) paper, which concerns more than a century of science and management, does not give much attention to the TAC instrument and the particular predictive models that support TAC decision-making, its associated control measures and so forth. Although Roswadowski (2002) appears to have a keen eye on how developments in ICES assessments work are linked through the TACs to developments in the fisheries management bureaucracy, the scope of her work does not permit her any extensive treatment of these matters. Regrettably, Smith’s (1994) history of fisheries assessment science ends just before things really start to heat up, namely when fisheries management intensifies in terms of science, politics and control of TACs. Further, Schwach’s (2000) history has its main emphasis on the same period as Smith’s; it is remarkably silent on the TAC regime as it emerges throughout the 1970s to be consolidated in the 1980s, and on the role of fisheries science (Norwegian or international) in this process. While Christensen and Hallenstvedt (2005) consider and discuss elements of what we term the TAC Machinery, it remains outside their main scope. The TACs and accessories are pervasive in contemporary fisheries regimes in the North Atlantic and elsewhere. In the cases where they appear to fail, they are subjected to intense public critiques from various stakeholders that are concerned with fisheries management, including a range of disciplinary orientations in the academic fisheries literature (paper 1 and paper 2). In other disciplinary orientations, it appears that

TACs have become somewhat naturalized.[14] And yet it is as if no one has really stopped to ask how the TACs came about and what their roles are in shaping the modern regime. Perhaps we can only understand this conundrum by appealing to the intriguing and almost, but not entirely, paradoxical logics of an 'invisible revolution' (Holm 2001)?

In order to study science-politics interactions, I felt compelled to take a few and feeble steps towards a genealogy of the TAC Machine in this project, to which I will return. Foucault (1977b: 139) writes that '[g]enealogy is gray, meticulous and patiently documentary'. I cannot claim that I have been sufficiently meticulous and patient. But my hope is that I have managed to bring into daylight some of the grey sources of literature (see below) that can be used in the production of more effective histories of fisheries science and management than I was able to offer within the confines of this project[15]. First let me explain how I have worked.

2.5 Materials and methods: Documents, document analysis and documentary realities

For stylistic and editorial reasons, the presented papers admittedly are relatively silent on methodological issues, and they do not present the materials on which they draw at length. The purpose of this section is to amend for these omissions.

Since the underlying method/theory of this work is ANT, the question of 'method' becomes pragmatic insofar as the parole of 'following science in action' leaves it open by what means this 'following' is performed, recorded and reported.[16] One can imagine many ways in which ANT can inspire studies of the practical construction, maintenance and transgression of the boundary between the science and politics of fisheries management. The approach we proposed for generating empirical material in our original project plan was centred on an ethnographic field-work method (Holm and Nielsen 2003). This plan was to study boundary processes within an annual cycle of

[14] In general, much of the ITQ discourse within resource economics appears to naturalize and build on a TAC Machinery (see paper 6).

[15] The TAC Machine is most fully explored in Paper 1. While I readily admit that this text is in need of much further editing, and that it is far from being ready as an academic paper, it serves the purpose of presenting subject matters of central importance to this thesis in much the same way as an empirical chapter in a monography would.

[16] This is also the case when ANT is rendered a sceptical programme (footnote 81).

science and management, focusing on the Northeast Arctic cod stock. The fieldwork included observation of assessment work in an ICES working group (the Arctic Fisheries Working Group), observation of the review process and the formulation of the advice in the ACFM, and observation of (a part of) the practical regulation procedure in the Norwegian 'Regulation Council' (Reguleringsrådet).

However, as this work progressed, my focus partly shifted from this annual fisheries management cycle to the historical process of framing the roles of science and politics within the fisheries management system in the Northeast Atlantic. We have already seen my motivation for doing so. As Degnbol showed us, we can perhaps only get an effective grip on the relationship of science and management by exploring the joint history of the two. Stated differently, I shifted from only aiming at 'mapping'[17] contemporary interactions between (fisheries) science and management to also devoting attention to how these have co-evolved historically in a process that led to the emergence of the framework of modern fisheries management. Instead of sticking to a single case study of a single year, I began, as we have already seen, to explore historical aspects of the institutionalization of the 'modern' fisheries regime.

A further motivation for the shift in emphasis from the annual science-politics routine to the co-evolution of this routine is that while the communication from science to politics could easily be examined within the ICES annual advice, the reverse flow from politics to science, is not (at least not to the same extent) a formalized process in the fisheries system. This flow, however, could be effectively approached in terms of the historical process of framing science-politics interactions within the emerging modern model of fisheries management.

What we here term the modern form of fisheries science and management is the regime that has emerged within the last four decades. We date the formation of this regime to the hectic period 1965–1975 (paper 1), following which the main 'fisheries system' (paper 2) was in place with generic roles ascribed for fisheries science and management (paper 1 and 4). This, however, should not be taken to imply that the whole model of practising fisheries science and management was built from scratch at the beginning of this period, i.e., that its *origin* can de defined and determined to this period.

[17] The specific meaning of 'mapping' here will be explained later, see Figure 2 (p. 74).

On the contrary, there is a long history of science and international cooperation on regulations that predates the emergence of the management regime that we describe. What we find to be characteristic for this modern regime, however, is that it propels both science and management into intensive systems with annual routines. In this latter sense, the modern regime represents a relatively new invention.

While it appears that fisheries science and management quickly settled into annual routines of VPA-based[18] assessments, catch forecasts and TACs throughout the latter half of the 1970s (paper 1), the boundary between the former appears to have remained delicate and to require further attention and development (paper 2, 4 and 5). Not least in crisis situations, which are not infrequent in fisheries management, it is reasonable to expect increased investments in boundary work in the attempt to stabilize this boundary. Controversies open up the field to the researcher, among other things because working routines and normative positions become explicated. Indeed, this is why Latour (2005) advises us to study controversies.

This change in emphasis described above implied a shift in research materials and methods towards documentary analysis. While the ethnographic field work provided me with an invaluable opportunity to become familiar with the contemporary framing of fisheries science and management, and to test out my interpretations of this field, the papers that are presented here altogether build on documentary materials[19].

From the theoretical starting point of relationalism[20], it goes without saying that the identity of an object depends on how it has been (re-)assembled (Latour 2005). Hence, it is pertinent to ask what kind of a science-politics object I have assembled here. In line with the above described empirical change of emphasis, my answer is twofold. First, I have (diachronically) explored the framing of science and politics, focusing on the

[18] VPA stands for Virtual Population Analysis. The reader may turn to (Hilborn and Walters 1992) for an introduction to VPA.

[19] As described above, this field consists of observation material collected at (long) meeting sessions and interview materials. This material includes meeting notes, drafts of assessments, drafts of advisory reports, and taped interviews. Why have I not made use of all this observation material? First, I reiterate that the observations have been invaluable in helping me to understand 'the field'. Second, the observation material became less important in terms of direct use as we redirected much of our empirical focus towards the general institutionalization of modern fisheries management in the Northeast Atlantic. Third, there is not to my knowledge any precedents for the use of such materials in the journals in which we have published. Fourth, I ran out of time while I was busy writing the presented papers! The material may prove more valuable for publication in STS journals, which I intend to aim for later.

[20] ANT's relationalism is characterized briefly in footnote 81. ANT will be discussed further in chapter 4.

development of the TAC Machine device, in which ICES advice is at the centre. Second, I have (synchronically) explored the interactions of science and politics in the current setting of fisheries management, namely in terms of the documentary reality (Atkinson and Coffey 2004) of ICES advice (papers 4 and 5). In the following I will explain how I worked in both regards, and I will present the types of documents I have relied on in the process.

A documentary reality of ICES advice

Since the production of advice, and of the scientific work that supports it, comprises ICES's main mandate, ICES, more than anything, is a document-producing organization. While ICES, as its webpage notes, 'coordinates and promotes marine research in the North Atlantic'[21], little new research is actually carried out in ICES. Instead we can think of ICES as a space where scientists meet to produce and discuss assessments, utilizing data that have been collected and processed elsewhere. Since ICES, hence, is a space where people meet to produce texts, its archives are a rich source of information about its past and present. The collection and analysis of documents such as the assessment reports, advisory reports and reports of dialogue meetings between advisory scientists and representatives of management bodies (in addition to published papers in the fisheries literature) provided me with an opportunity to examine some of the interactions between science and management over time.[22]

It is important to consider that I have not only used documents as evidence of non-textual events. Documents, too, enact a reality of their own, which can be explored. In her discussion of research on 'documents in action' in organizational settings, Lindsay Prior (2003: 60) revisits the philosopher Gilbert Ryle's example of a 'categorical mistake'. In this example, a visitor is guided around a university campus after which the

[21] http://www.ices.dk/indexnofla.asp (visited 07.07.08)

[22] I found that the documentary method appeared superior to an interview method in this respect. Regarding the historical examinations of the introduction of TACs and new assessment methods in the early 1970s, we interviewed some scientists (J. Pope, A. Hylen, and Ø. Ulltang) who participated in making assessments and preparing management advice for ICES and the International Commission for the Northwest Atlantic Fisheries (ICNAF) at that time. However, as a likely consequence of the remoteness of these events on the scale of human memory, these interviews, which we conducted under the PKFM project, did not tell us much more than what we could infer from the assessment and advice reports and so forth. The documents appeared stronger in terms of historical details. The interviews, however, provided us with a good opportunity to test out the general historical interpretations that we had derived from compiling and reading documents.

visitor asks where the *university* is. Prior responds that instead of, as Ryle did, dismissing the visitor's question as categorical mistake, a more appropriate answer to the question of the university's location can be sought by redirecting focus from the university's physical location to what Atkinson and Coffey (2004) term its 'documentary reality'. The university may indeed be found as the entity that is addressed in webpages, legal documents, administrative correspondence and so forth.

Although the question of ICES's physical location may not have upset Ryle as much as the question of the university's location[23], it makes sense to analyse ICES in terms of documentary reality. Crudely, such a reality is indicated by the fact that much of ICES's activity does not take place at ICES's headquarters. Importantly, the concept of document reality opens up an analysis of ICES as a particular character, indeed as an *author*. When I explore 'ICES's advice' (paper 4), this should be understood in a very literal sense, namely in terms of an author and its text. I propose that there is a sense in which the individual ACFM members did not write ICES's advice but that *ICES* did. ICES advice constructs both an author (i.e., ICES) and an ideal reader, and therefore it not only a good site to study processes of science-politics boundary construction in fisheries management; in a certain sense it represents *the* site to study it – at least in so far as we talk about the formalized boundary. It is in ICES's advice, and in particular the introduction to this advice, that ICES represents itself to politics and frames its reader (say, 'the manager'). The advisory text hence offers the primary site for exploring the co-production of science and politics as authored by advisory science on fisheries management in the Northeast Atlantic.

Following a paper trail: the co-production of science and politics by the TAC Machine

The notion of the TAC Machine plays an important role in almost all the presented papers.[24] If the documentary reality of ICES advice (as described above) can be

[23] It is common to understand the question of ICES's location in terms of the whereabouts of its headquarters building, which is located in Copenhagen.

[24] Our story of the institutionalization of modern resource management as by the TAC Machine is primarily developed in Paper 1 (and to some extent paper 6), while papers 2, 3, 4 and 6 mainly address some of its

characterized as an answer to the question of how science and politics act on the contemporary stage comprised by modern fisheries management, the TAC Machine is central to how we approach and examine aspects of a genealogy and some properties of that stage. The task of documenting this fully would be daunting. We are at its beginning. Our work in paper 1 is incomplete, and its performance as a scientific text is admittedly poor. In order to explore the issues I address in this thesis, however, I felt that there was no other choice but to risk the jump into this rather uncharted chapter of modern history.

Worse still, I have jumped into this unknown territory without the parachute that a strict conventional methodology could have provided. Instead, I have generally worked as follows. I started with the rather open-ended issue of interactions between science and politics in fisheries science and management. This prompted the question of what characterizes modern science-based fisheries management. This issue was rendered operational by focusing on the formalized system of science and management by way of ICES advice and TAC management, which, as we explore, emerged in the early 1970s. How and why was this system developed? How did science and politics become organized within this system? Why did it happen in this way?

While such general research questions provided some focus, a series of more specific *ad hoc* questions helped me to structure my work with collecting and reading documents. These were questions such as the following: When and for which stocks did ICES first provide TAC advice? Which assessment methods did they use? Do the assessment scientist and the scientific advisors relate to management issues? If so, how do they relate to them? Are the emerging management instruments and the assessment methods linked? If so, how are such links expressed? In summary, my way of working can be recapitulated as an *ad hoc* process of asking research questions and gathering information that leads on to new sources and new research questions. These *ad hoc* research questions were guided by the overall leitmotif of science-politics interactions, and constrained by the form and availability of materials. The metaphor of jumping into the unknown, hence, is actually inappropriate; in fact I have lowered myself down little by little, securing my hold by all available means.

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properties and implications. The TAC Machine notion is not explored or used in paper 5, at least not explicitly.

By this way of working, I gradually became aware of which sources of documentary materials were available and relevant.[25] As hinted at above, the amount of documentary information stored in ICES's archives is overwhelming. I have spent weeks in the dark and dusty basement below the ICES offices, frantically skimming through and copying reports of ICES advice, assessment reports, ACFM meeting minutes, and reports of dialogue meetings between ACFM and representatives of fisheries management bodies. In general, I collected all sources that I believed could hold new information on the topics I explored. The sheer amount of pages that piled up in my office as a result came with the drawback that I had to give up systematically reading through all the materials. Instead, I read selectively, focusing on the type of documents, and the chapters within them, that I learned were most likely to hold information on the issues addressed by the research questions.

The TAC Machine device is most extensively explored in paper 1. Paper 1 is based on; *first*, unpublished historical materials that I have collected, and *second*, papers published in the 'fisheries literature'. The other presented papers also depend on these materials in so far as they build on the notion of the TAC Machine. The first group of documents, which will be described below, is chiefly comprised by assessment reports of ICES Working Groups and the reports containing ICES's advice to its clients). We have used peer-reviewed papers in the 'fisheries literature' for providing information and for positioning our contributions in this literature.[26]

In more concrete terms, I have collected a range of ICES materials which include, but are not limited to, the following:

- All stock assessment reports and advisory reports of ICES that address the cod stocks of the North Sea and the Barents Sea from the early 1960s to

[25] Recently, and too late to be taken into consideration in this work, I learned that NEAFC *may* hold documents in its archives (located in its headquarters in London) that could prove fruitful with respect to further research into the historical framing of modern fisheries science and management, i.e., to the further exploration of the issues we primarily address in paper 1.
[26] A different but particular use of peer-reviewed material is represented by the statements of resource economists that we cite in paper 6, which in our work are recast as empirical evidence in relation to the issue of performativity of economic theory.

2004. Further, I have collected most of the assessment reports and advices for other commercially important demersal stocks (haddock saithe and whiting) from these areas (primarily from the early 1970s to the late 1980s). In addition, I have collected advices and assessments on the North Sea flatfishes, the Greenlandic and Icelandic cod stocks, and some major herring stocks on which ICES provides advice (also primarily from the 1970s and the 1980s).

- All reports on dialogue meetings between representatives of fisheries management and ICES.
- A range of other ICES reports, for instance, some that concern development of assessment methodology and data issues.
- A range of reports and working documents that concern the development of ICES's Precautionary Approach.

While the most recent advice from ICES is available on its webpage (www.ices.dk), its previous advices can be obtained in the form of printed reports. ICES advisory reports are public documents that can be obtained from (European) libraries that are well-resourced with regard to literature in marine biology and resource management. Alternatively they can be obtained from ICES library.

ICES stock assessment reports and the reports of other ICES working groups are of a considerably more greyish nature than the advisory reports. In ICES, these documents are filed as Council Meeting (CM) documents. While CM documents from the year 2000 and beyond are available on ICES webpages[27], older documents are only filed in printed form in ICES's archives[28].

In addition to the development of fisheries management in the ICES area (i.e., the Northeast Atlantic) our story on the TAC Machine examines some of the history of the fisheries science and management in the Northwest Atlantic before, during and after the time of the introduction of TACs in the early 1970s, within the International Commission for the Northwest Atlantic Fisheries (ICNAF). Apart from the few published materials

[27] http://www.ices.dk/products/cmdocsindex.asp (visited 11.08.08).
[28] Here I wish to express my gratitude to ICES's kind and helpful librarians, Michala Ovens and Solveig Lund Vestergaard.

that I was able to gather on these issues, I have here relied on (a) the ICNAF 'Red Book Series', which contains (I) proceedings of the Standing Committee on Research and Statistics, (II) research reports of ICNAF's member countries, and (III) selected scientific papers,[29] and (b) two manuscripts that appear in an ICES dialogue meeting report.[30]

As mentioned, almost all the papers depend on the TAC Machine notion in so far as it offers a conceptualization of modern fisheries management. In addition, each paper depends on materials that enable the examination of a particular perspective. Some of these materials deserve some introduction:

Paper 1 deploys a narrative of path dependence. The notion of path dependence is briefly presented and discussed through references to a few original and central works in the academic literature on path dependence. Further, evaluations of the TAC Machinery are introduced and discussed. These are partly public reports (e.g., EC Commission documents) and partly works published in the 'fisheries literature'.

Paper 2 presents and discusses some published works on evaluation and organizational learning. It also presents and discusses changes in forms of ICES advice, which is based on reports of ICES advice and reports of dialogue meetings.

As concerns my contribution to *Paper 3*, it primarily depends on the materials listed for paper 1 above.

Paper 4 examines how the discourse on precaution became translated into ICES advice. This is based on a range of ICES documents, on published papers that address issues relevant to the development of the discourse of precaution, and on official documents (in particular the UN's straddling fish stock agreement) that guide or specify the implementation of the precautionary approach.

Paper 5 explores ICES's implementation of the precautionary approach by way of reference points. In doing so, the paper primarily depends on a range of technical ICES reports.

[29] I am indebted to the librarians at the shared library for the Directorate of Fisheries and the Institute of Marine Research in Bergen who offered me an (almost complete) spare Red Book series (1958–1979).
[30] These are Anthony and Garrod (1985) and Anthony and Murawski (1985).

Paper 6 presents and discusses elements in the published literature on Individual Tradable (fish) Quotas (ITQs). It uses public Norwegian policy documents to track a story that leads to an ITQ market within the Norwegian coastal fishery.

The author's confessions and challenges to sceptical readers

The informal method I have described above may prompt concerns about validity and representativeness of the texts that it has helped to bring about. The documentary material presented above is substantial, although it cannot be regarded complete in any sense. It goes without saying that only a small fraction of the material I have collected is referred to in the papers. Together with the fieldwork I have conducted, this collection of documents has nevertheless provided a background for the interpretations that are expressed in the individual papers. The document collection hence provides a documentary 'hinterland' of my papers, which is difficult, although not entirely impossible, to open up for a critical examination. How have I selected within this material? How did I decide which sources and issues to give weight to and which to leave out? How did I develop my interpretations?

While such concerns obviously come in different shapes within a qualitative and non-formalized approach, they remain important. As a consequence of my approach, as described above, I cannot claim to have a single and final way to meet such concerns. However, this is, I think, not uncommon in qualitative research in social sciences.[31]

As will be discussed further in the theoretical chapter that follows, I intend each of my texts to be more than just 'a story'. ANT texts are experiments that can succeed or fail in 'reassembling' a phenomenon (Latour 2005: 127) such as the TAC Machine. It has been my aim to let the papers justify themselves by the references they contain. The papers suggest and seek to justify certain interpretations (those that have been published in the fisheries literature have been found to do so acceptably in this context). These interpretations are open to be challenged, whether such challenges are mobilized from

[31] How can we, for instance, know that Foucault's representation of patient journals (Foucault 1972) or law cases against criminals (Foucault 1977a) are accurate and representative of the dispositives and epistemes he characterizes? Did he really read all those medical journals and law cases? Note that I do not pretend that the quality of my work can be compared with that of Foucault. But I think the possibilities that are available for dissenting readers can be. A reader determined to dissent from a text by Foucault would, unless s/he discovers problems that are obvious from the text itself (e.g., inconsistencies in the text's arguments), ultimately need to read and interpret sources similar to those Foucault read and interpreted.

similar or different materials. This is not different from when Latour explores how texts in natural science are written to withstand critique of real or imaginary dissenters: 'The dissenters cannot do less than the authors. They have to gather more resources in order to untie what attaches the spokesmen and their claims' (Latour 1987: 79).

2.6 Positioning the project within the literature on science-politics relationships in modern fisheries management

Although we should not forget what we can learn from studying the history of devices, I now turn to more contemporary discussions of the challenges in fisheries management. Fisheries management offers complex, or indeed, 'wicked' problems (Jentoft, Chuenpagdee and Kooiman 2008; Rittel and Webber 1973). Wicked problems are, among other things, characterized by the absence of a definite formulation or a permanent solution. Problems in fisheries management are addressed by a range of academic disciplines, and the corresponding literature is accordingly as rich as it is diverse. How does my work fit into the ongoing discussions on the challenges of fisheries management? How do I seek to contribute to these discussions?

However pertinent they are, I cannot answer these questions fully here. The reason is that I simply cannot commit myself to the task of reviewing these discussions adequately. They are simply too rich and too diverse, and I am afraid that if I attempt to do so, this would involve more interpretative violence than I am willing to exert.[32] Instead, I will proceed here by focusing on positioning my work in relation to the much more limited segment of this literature that explicitly focuses on my topic, namely science-politics relationships in fisheries management. As will become increasingly clear, however, my take on this topic in turn opens the way for broader deliberations on the challenges of contemporary fisheries management practices.

[32] As a tentative and much less ambitious response I refer to our discussion of the discourses on fisheries resource management as we address them from the perspective of systemic evaluation and learning (paper 2). Here, we go along with Degnbol and colleagues (2005) who argue that ongoing discussions on fisheries management are characterized by competing disciplinary orientations that tend to offer 'technical fixes' to fisheries management, which are characterized by being too narrow to take its complexity adequately into account.

As I have indicated, modern fisheries management is science intensive. Although assessment science is notoriously uncertain, the modern form of fisheries management, as it is generally practised today in the Northeast Atlantic, creates a high demand for assessment accuracy, a demand which is often not met (papers 4 and 5). This is one of the main reasons why the science-politics boundary is important, and why I find that research that addresses this boundary has something to offer in relation to the discourses on fisheries management.

While the academic literature on fisheries management is rich, it does not appear to me that very much attention has been paid explicitly to the boundary between science and politics in this field. There is quite a literature on assessment science and on fisheries management, respectively. There is much less devoted to their relationships, and there are yet fewer attempts to explore these relationships from the perspective of sociology of science or related perspectives. However, I have come across a few works of the latter sort that are relevant to introduce, and in relation to which I can position my work.

From institutional dimensions to science-politics technologies in fisheries systems

In a paper entitled 'The institutional dimensions of fisheries stock assessments', Frank Alcock (2004) indeed takes on the task of examining 'the linkage between fisheries stock assessments and fisheries policy'. Alcock's main argument is that fish stock assessments that are 'embedded within policy making organizations' are more influential on policy-making than 'autonomous assessments', which in turn are more influential in regard to a broader range of stakeholders that are affected by fisheries policies.

Alcock's more detailed proposition is that:

> Scientific assessment processes that are structurally embedded within policymaking organizations enhance the perceived salience, credibility and legitimacy of assessments for actors within the larger organization. Conversely, embedded stock assessment processes can weaken the salience, credibility and/or legitimacy of these processes in the eyes of external stakeholders that question the organization's salience or challenge the organization's credibility or legitimacy. (Alcock 2004: 132).

From an examination of three empirical cases,[33] Alcock establishes a tradeoff between assessments that are highly 'embedded' in a policy organization and 'autonomous' assessments. In the former, the assessments have a strong influence on policy makers but a low influence on external audiences. In the latter, the relative strengths of these influences are inverted.

The tradeoff that Alcock proposes has intuitive appeal and it also has theoretical and practical implications since it draws attention to (science-politics) dilemmas in the institutional design of fisheries systems (Charles 2001). While Alcock's introduction of the concepts of salience, credibility and legitimacy appear useful for analysing stakeholders' perceptions of scientific assessments and advices, I find that there are considerable limitations to Alcock's approach. I will discuss these limitations as they illustrate how ANT/STS, and the conceptualization of fisheries management in terms of cybernetic systems, may contribute to the academic discourses on fisheries management.

As a part of the empirical justification of his tradeoff, Alcock (2004: 132) takes on the task of showing how the 'salience, credibility and/or legitimacy' of stock assessments that are embedded in a policy organization can weaken in the eyes of external stakeholders that are critical of that organization. Alcock proposes that this can happen because embedded assessments are vulnerable to 'political constraints' that can distort precision, downplay uncertainty and redirect assessment focus. The example he mobilizes to illustrate this is the now classical case of scientific hubris and flawed management in relation to the socioeconomically vital cod stocks of the coasts of Newfoundland and Labrador. As it is widely known, following a series of scientific over-estimations in the course of the late 1980s, in the early 1990s it was realized that these stocks had collapsed. This, in turn, had dramatic consequences for the local coastal communities.

Alcock briefly presents us with the institutional setting and the series of events that comprises his case. The Canadian Atlantic Fisheries Scientific Committee (CAFSAC), which was responsible for providing stock assessments, is introduced here as

[33] Alcock's cases are the following: Cod management in Newfoundland and Labrador in the 1980s (to which we return below) represents the example of 'embedded stocks assessment'; the New England Fisheries Management Council and the National Marine Fisheries Service represent 'autonomous stock assessments'; and the Australian Fisheries Management Authority, Advisory Committees and Fisheries Assessment Groups represents an intermediate case.

highly embedded in the policy organization, namely the Department of Fisheries and Oceans (DFO). During the 1980s, inshore fishermen increasingly criticized CAFSAC's assessments which suggested that the cod stocks were increasing. The picture of growing stocks was publicly disputed by the inshore-men who were experiencing low and declining catches. Having confidence in CAFSAC, the DFO downplayed the inshore fishermen's complaints. In time, CAFSAC and DFO lost credibility and legitimacy in the eyes of the inshore fishermen.

This all fits well with Alcock's model. The assessment science is deeply embedded in the policy organization, and while the latter trusts in its advices, at least one stakeholder group increasingly loses confidence in science and management[34]. Alcock, however, does not go into much detail on how the political pressures imposed precision and redirected the assessment focus.[35] Hence, while Alcock's tradeoff model seems to correspond well with the case he discusses, his explanations remain somewhat abstract, and I would add, also somewhat unsatisfactory.

I am not sure Alcock's scheme would work well for the ICES-EU case. But I will here abstain from the attempt to mobilize this case as a challenge to Alcock's tradeoff and the explanatory mechanisms he invokes in its support.[36] The mobilization of such a case would require a substantial amount work, and it would probably not be worth the

[34] The offshore fishermen continued to experience good catch rates throughout the 1980s and did not lose their trust in DFO and CAFSAC, which, according to Alcock, is because they were much more closely affiliated with the policy organization than the inshore fishermen.
[35] Alcock (2004) apparently limits himself to contending that (a) 'embedded assessment processes tend to be less transparent and more apt to misreport uncertainty' and (b) there exists 'a strong incentive to harmonize assessment conclusions with policy choices or recommendations' and (c) this was what happened in this case.
[36] I limit myself to the following remarks. Clearly, ICES must be regarded as 'autonomous' in Alcock's scheme (but see the discussion below of how ICES is not 'autonomous' in another important sense). Alcock's trade-off, hence, makes us expect that ICES has a relatively low influence on decision-making in the EU but also a relatively strong influence on the views of other audiences. While attempts to resolve such questions quickly become very complex, much apparently points to the opposite: While the EU Commission and Fisheries Council generally aims at following ICES advice, EU fishermen loudly discredit ICES's assessments and find that ICES advice determines fisheries management in the EU to an unacceptable extent (Delaney, McLay and van Densen 2007). Delaney and colleagues observe that: 'The public debate about the deficiencies of the science does not appear to have influenced the year-to-year decisions about TAC directly, at neither the advisory stage nor the stage when proposals are made. CEC managers largely follow scientific advice in formulating their proposals to the Council of Ministers. When the agreed TACs deviate from the proposals, the political reasons are usually not given, but they are likely to be economic or social rather than biological. Therefore, it is at the level of individual ministers that national responses to the public debate may influence the decision-making, rather than at an EU level' (Delaney et al. 2007: 809).

struggle in so far as it would be naive to think in terms of a ‘refutation’ (or corroboration) of a general interpretative framework such as Alcock’s.

Instead I will address what I see as a main limitation to Alcock’s approach, which is apparent from the following excerpt:

> Conceiving of scientific information as pure or unadulterated prior to transmission into a policy arena misconstrues the pre-existing relationships between the set of relevant actors in a given issue area. For fisheries policies, institutional relationships between fisheries scientists, fishery managers, and the fishing industry exist independently of stock assessment processes. (Alcock 2004: 131) .

While we, for reasons that are pursued elsewhere in this thesis, can agree that it is unhelpful to think of assessment science as ‘pure’ in a positivistic sense, the notion of institutional relationships that exist independently of stocks assessment processes precisely identifies how ANT may help. A focus on ‘institutional’, or rather, *organizational* dimensions of the relationships between science, management and fishermen should not make us ignore how assessments methods sustain and transform these relationships.[37] Indeed, ANT would advise us to pay close attention to how (assessment) technologies may mediate such relationships.[38] I will return to this below. But first I will let another author show us how we can get much further with the Northern Cod case.

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[37] Hence, this is not an argument against studies of institutions but an argument that such studies must not ignore the properties of science methods and technologies. While paper 1 concerns ‘the institutionalization of Sustainable Fisheries Resource Management’, the specific technologies of representing fish and intervening in fisheries are addressed as key elements in this institutionalization process. In general, Holm (2001: 84) finds that institutionalists tend to shy away from studying science and scientific knowledge ‘as subjects for detailed empirical analysis, as institutions worth studying in their own right’. Instead, ‘remarks on science are restricted to vague references within the culture of rationalization, as symbolizing the essence of Western-style cultural account’. While Alcock considers assessment science empirically, this may not be in sufficient detail, and I suspect that his general propositions only hold up when applied rather vaguely. This, in turn, is not to say that our study is of sufficient empirical detail.

[38] In parallel to what I discuss as the limitations to Alcock’s approach, McGuire (1997) found that the ‘political ecology’ approach would have been unable to explain the collapse of the Northern cod, indeed because it would not take the technologies of catching fish and measuring fish stocks into close account.

Fishing for social (arte)facts

The reader familiar with the fisheries literature will have been waiting for me to address Christopher Finlayson's (1994) notorious sociological analysis of the Northern cod stock assessments. This analysis has been highly important for opening assessment science to external enquiries and deliberations, and it represents an important source of inspiration for my work. It has indeed been a critical and effective history of the Northern cod stocks assessments from 1977 to 1990. The purpose here is not to go into the case but to look at how Finlayson positions his work theoretically and works with it in practice. Finlayson's social constructivism and dedication to study the interactions between micro- and macro-processes bear some resemblances to my work, which, as mentioned above, draws on ANT. As a warm-up before the development of my own theoretical perspective throughout the following chapters, I will nevertheless draw attention to some differences between ANT and Finlayson's approach.

As this work, Finlayson takes on the task of exploring the negotiable boundaries between science and politics in mandated fisheries science (Wilson and Degnbol 2002). Finlayson notes that the dynamics of the relationships between scientific rationality in the academic tradition and political/bureaucratic rationality forms the backbone of his work, and that we should see his work as an example of the conflicts that emerge from the uneasy marriage of these traditions (1994: 2). He mobilizes 'taken for granted and disputed negotiated boundaries between individuals, institutions and other elements of social structure as explanatory factors' but draws attention to the critical importance of 'the boundary between science and the political/bureaucratic structure of the state' (Finlayson 1994: 16).

Finlayson (1994: 1) characterizes his work as 'forensic sociology', dedicated to providing a plausible explanation of a controversial, and critically important, period in the Atlantic Canadian fishery. He explains that while he set out from the perspective of social construction of scientific knowledge, he later realized that 'this micro-social approach to the problem was insufficient to explain the empirical reality of fisheries science' (Finlayson 1994: 11).[39] Believing that the fisheries crisis 'can most usefully be

[39] I note that this parallels my experience, which as explained above, made me devote attention to the historical framing of the science and politics in fisheries management.

understood as a product of multi-leveled and interactive social forces and processes' (Finlayson 1994: 10), Finlayson intends to explore the social construction of Northern cod stocks assessment at progressively larger scales of social organization' (Finlayson 1994: 128).

From the perspective of social construction of scientific knowledge, Finlayson (1994: 13) sees it as his task to establish the 'social context that created contemporary and retrospective judgments of "right" and "wrong", both by the scientists themselves and by outside groups and individuals'. He opposes this to a 'traditional' view, which 'holds that the 'success' and/or 'failure' of stock assessment science is attributable solely to the ability or inability of scientist's to objectively and accurately understand, describe and predict the dynamics of external reality' (Finlayson 1994: 10).

The primary empirical resource on which Finlayson draws in order to establish these contexts is an impressive collection of interviews with assessment scientists and fisheries managers within the DFO. In addition, he analyses a range of relevant documentary materials such as CAFSAC's assessment reports and reports of external assessments. His intensive discussions with a particularly engaged and reflective assessment scientist are documented throughout the book. These discussions enrich Finlayson's book by offering interpretations of particular events that are alternative to the interpretations that are mobilized from Finlayson's social constructivism.

While some of his conclusions are much along the lines of Alcock's model of the problems of embedded assessments[40], Finlayson's analysis of the problematic Northern cod stock assessments is much more theoretically and empirically profound than Alcock's. It goes without saying that this comparison is unbalanced since Finlayson dedicates a whole book to a case that figures as one of three examples in Alcock's paper. My point, however, is that Finlayson's analysis gets us much further towards a comprehension of the background of the problematic assessments and management decisions, and of the relationships between those responsible for making them, because

[40] 'There is yet another very real problem in the relationship between the state and its sponsored science that is the source of profound ambivalence between the two institutions. This arises from the fact that the state's fisheries policy derives its legitimacy by appearing to be in close association with science, but science derives its credibility by appearing to be disassociated from the state. The Science branch can only function in the state's interest to the degree it is successful in preserving its scientific legitimacy. However, the state will only be willing to function in the interests of the Science Branch to the degree it finds its knowledge production of practical value in achieving its political objectives' (Finlayson 1994: 151).

he looks at how they were made in quite some detail[41]: He interviews the scientists who made the assessments, he compares their results over the years, he looks at what data entries or model assumptions have changed in between them, and so on. This enables him to unfold the enormous 'interpretative flexibility' of CAFSAC's Virtual Population Analysis (VPA)[42], and to observe that it requires 'considerable subjective judgment as to the choice and weighting of the variable inputs' (Finlayson 1994: 13). In ANT terms, it is because he 'follows science in action', and because he is brave enough to attempt to open up the VPA, previously a 'black box' (Latour 1987) to fisheries sociologists, that Finlayson gets us beyond, or at least almost beyond, the mere suspicion of political corruption of embedded assessment science. If a touch of a conspiracy sentiment yet sticks to Finlayson's account, I think this relates to the form of his explanations in which, once the interpretative flexibility is established, 'social forces' are invoked to do the explaining. Let me try to explain what I have in mind here.

When Finlayson finds that his case represents 'a compelling empirical example of the social construction of scientific knowledge and the concept of interpretative flexibility' it is because:

> Over a period of three years, the dominant and authoritative claim of stock status and trajectory changed profoundly although the data set on which these claims were based had not. Therefore the explanation for this change must lie in the changing social context within which the data were interpreted. (Finlayson 1994: 39)

This first social context, in which the over-optimistic assessments were produced, was characterized by:

> the commitment to the idea of a growing cod stock [which] was so powerful that it can be shown to have been read back into ambiguous data through analytical models built open hypothetical assumptions about population and ecosystem dynamics. (Finlayson 1994: 13)

[41] Similarly, Wilson and Degnbol's (2002) case study of a problematic and particularly uncertain assessment is highly interesting, not least because particular choices of models and their configuration are here considered in their interaction with management decision rules, by which assessment choices may predetermine management decisions (see also footnote 45).
[42] In a strict sense, the VPA pertains to a specific type of age-structured models (Megrey 1989). Informally, the 'VPA' often refers to other variants of age-structured models. (The VPA figures in paper 1, 4 and 6).

This commitment, hence, is what carried 'politics' into the sacred chambers in which the numerical rituals of the assessments were negotiated, agreed upon and performed. Reviewing the self-confident promises of an objective scientific knowledge production that was to inform rational management decisions, which were mobilized to promote Canada's takeover of the resources within the 200nm Exclusive Economical Zone that it successfully obtained in 1977

> …we discover the seeds of the current fisheries crisis and the foundation of the institutional structure and process that would later result in the penetration of powerful social forces deep into the heart of fisheries stock assessment science. (Finlayson 1994: 23-24)

Finlayson mobilizes a good deal of observations to the support of the line of thought I have indicated by these three quotations. But I think there is even more to it. First, a careful reading reveals that it is contestable that the dataset did not change in the three-year period Finlayson refers to in the first citation.[43] Second, such 'social forces' tend to dissolve as such once we look at them closely. While Finlayson struggles to show how 'social forces' pull the assessment in, as it turns out, an unfortunate direction, ANT would prefer his alternative concept of the 'social context' of these assessments since it is less prone to determine prematurely what sort of relations and effects we should look for. Taking one more step down ANT's line of reasoning, we see that 'context' would be even better since it would be an empty box to be filled *in situ*. Hence, if the strength of Finlayson's study is his empirical detail, the complexity, and the contingency of the issues he addresses, I find that this also raises conceptual challenges to his approach because 'social forces', at least when 'forces' carries connotations of its more universalistic use in physics, is inappropriate. ANT does not accept 'social forces' as the *explanans*; the social must itself be explained (Latour 2005).

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[43] In a dispute on the extent to which an external assessment paved the way for more pessimistic reassessments, the CAFSAC assessment scientist who has the important role as Finlayson's critical dialogue partner says: 'What changed was the judgment calls – the subjective decisions of what weight to give what (and the data!). The two data points available in 1989 and not available in 1987 account for about 75 percent of the change in the perception of the stock between the two assessments' [emphasis added by Finlayson removed] (Finlayson 1994: 58). The scientist is not denying interpretative flexibility and that the scientists' judgements are influenced by their environment, but he maintains that the new data played an important role.

We gain a better understanding of the points I made above if we take a brief look at how Finlayson's epistemology and ontology[44] differs from ANT. As a social constructivist, Finlayson views 'scientific knowledge primarily as a social artifact and a social accomplishment rather than an objective description of external natural reality' (Finlayson 1994: 12).

Either scientific knowledge is objective, as in the sense of a hopelessly outdated naive positivism, or it is really a social artifact. If so, ANT represents the middle that has been excluded here. While ANT agrees that scientific knowledge (as other knowledge forms) is constructed, it does not regard this construction as primarily social or natural, but always socio-natural. Indeed, ANT sees the production of a distinction between the natural and the social as a possible outcome of such a knowledge production (Latour 1987; 1993). There is no interesting external reality to speak of, and there is no *sui generis* social domain. Reality, although we never know for how long it is stabilized, is always mixed and mediated. Questioning whether stock sizes and the catch forecasts are independent of our ways to measure them makes no sense in ANT's view. Further, for ANT, a 'social context' does not determine assessments, unless in this context we include, among other things, the cod landings, the scientific survey information, the catch rates from the fishing fleets, and the assessment models. For Finlayson, nature is soft and must be explained; the social is hard and can do the explaining, an assumption common to the Sociology of Scientific Knowledge (Holm 2001: 212). For ANT, in turn, the natural and the social are produced together in heterogeneous networks, which can be more or less stable.

This does not entail that ANT does not agree with Finlayson that stock assessments are flooded with 'interpretative flexibility', and that to some extent they are based on

[44] The following excerpt makes me question the stability of Finlayson's position: 'Fisheries managers do not now and probably never will know enough about fish and their ecosystem to construct enough facts to support agreement and cooperation. Instead it will be acknowledged that fisheries management is fundamentally a social process and the essential problems are sociological problems' (Finlayson 1994: 154). The word 'probably' here seems to admit that the limits to scientific fact making are a question of resources, which seems to be at odds with the notion of facts as social artifacts. But then again, if knowledge is socially constructed, why shouldn't it be able to support agreement and cooperation? The quotation makes me think that Finlayson here seems to be in need of a concept of socio-natural networks of variable stability. Hence, once we probe below the cover of social constructivist rhetoric, Finlayson may be much closer to ANT than anticipated!

professional ('subjective') judgment. Taking this back to the science-politics boundary we can ask: insofar as they to some extent preempt management decision-making, are such stock-modelling choices in a broad sense 'political'?[45] Even if we imagine that scientists 'outsource' such choices to standardized methodological decision-rules, would this not imply that these methodologies would become politicized? Let me try to develop a framework in which this question can be more effectively addressed.

ANT and fisheries management as cybernetic systems

Like Finlayson, ANT does not take the relationships between micro- and macro-actors for granted; they are something that most be studied empirically. ANT conceptualizes these relationships by considering 'macro-actors' as 'micro-actors seated on top of many (leaky) black boxes' (Callon and Latour 1981: 286). A 'black box'

> contains that which no longer needs to be reconsidered, those things whose contents have become matter of indifference. The more elements one can place in black boxes – modes of thought, habits, forces and objects – the broader the construction one can raise (Callon and Latour 1981: 285).

The 'Fisheries Leviathan' (paper 6), our metaphor for a political body that is able to evaluate fish stocks and decide on appropriate interventions in the fisheries, is but a macro-actor that is on top of a pile of, indeed, leaky black boxes. As we show, the powers to decide and control of this Leviathan are tied in with the development of the fish counting laboratory. Shifting back to micro-actors, we observe that their agencies are constrained and enabled (but not determined!)[46] by what we term the TAC Machine: members of ICES assessment working groups make single stock catch forecasts;

[45] Wilson and Degnbol's (2002: 5-6) highly interesting analysis of a remark by a director in the National Marine Fisheries Service (USA) effectively illustrates of what I have in mind here. They characterize four features of this remark: 'The first is its mechanical conceptualization of management where a scientific evaluation is plugged in and a management decision pops out. […] The second is the complete separation of science and management. The third is that the real decisions about outcomes are actually made on the science side, while the choice between the preprepared management measures is then entirely contingent on the scientific findings'. An interesting paradox here is that while this 'mechanical' device (i.e., an implementation of the precautionary approach as a strict decision-rule) helps in the separation of science and management, it also renders assessment science (which in this case is highly uncertain) the site where *de facto* management decisions are made.

[46] While the heterogeneous network comprising fisheries management may be 'heavy', while the TAC Machine may be 'path dependent' (paper 1), micro-actors can and will change it over time.

members of the Fisheries Commission propose TAC regulations; fishermen fish, violate or trade their quotas, and so forth.

I have at several points above suggested that an improved understanding of science-politics relationships in fisheries management is conditioned on a consideration of the practical technologies of representing fish stocks and intervening in fisheries. Our take on this (paper 1 and 2) is to conceptualize fisheries resource management as a *cybernetic system:*

> Cybernetics is the science of communication and control. The applied aspects of this science relate to whatever field of study one cares to name: engineering, or biology, or physics, or sociology…[The thing that can be controlled] is a system: any cohesive collection of items that are dynamically related. More formally, the items may be regarded as points connected by a network of relationships. Instead of classifying these systems as whether they are animate or inanimate, whether they are made up of a flow of paper-work or ironmongery (Beer 1967[1959]: 7).

In Stafford Beer's dusty old textbook, *Cybernetics and Management*, hence, we find a succinct and general formulation of what Holm (2000) talks about, although (in accordance with the latest fashion in Paris) he deploys ANT terminology, when four decades later he describes fisheries management as a 'heterogeneous network'. Beer's focus on dynamic network relationships, be they human or non-human, clearly anticipates ANT. Taking a closer look at ANT, we discover that this resemblance is no coincidence (Pickering 2002). Indeed we notice that the concept of a 'black box' (Beer 1967: ch. 4) is borrowed from the old cyberneticians.

Now, let me try to explain how this may be useful for studying science and politics in fisheries. In the spirit of Finlayson, a starting point could be to ask why ICES's assessments of the North Sea cod stock assessments were significantly biased for a number of consecutive years (Nielsen 2003). Since ICES must be regarded as 'autonomous' in Alcock's scheme, we cannot explain this from 'institutional dimensions', at least not alone.[47] Instead, let us see how we can reconsider this question in terms of a cybernetic system. While we can always argue about the relative importance of economic, social and biological values and how they should be defined, the purpose of

[47] See footnote 37 for further clarification.

fisheries management is to control fisheries in order attain some combination of these values. In this sense, fisheries management is a *homeostat*: ‘a control device for holding some variable between desired limits’(Beer 1967: 22). In so far as the North Sea cod stock for several years now has been assessed to be ‘outside safe biological limits’, its management can be regarded as a cybernetic system that fails to be a homeostat. Attempting to understand why, we track and explore the ironmongery and paper-work, namely the (as we say in Paris) *devices* of fisheries management. This includes the assessment methods, data infrastructure, the advisory format and how it frames decision-making, the control procedures, and so forth. Following Pickering (2002), we can find inspiration in some of the works of the old cyberneticians when studying scientific knowledge production, not in terms of epistemology, but in terms of what he calls a *performative idiom*, which is more suitable for studying how this knowledge is ‘constitutively bound up with the *dance of human and nonhuman agency* [as he romantically labeled it] rather than as a self-contained topic for enquiry in itself” (Pickering 2002: 414).

Should the North Sea cod crisis be attributed to absurd assessments, brainless bureaucracy, or crooked catches?[48] Once we start working our way through the cybernetic system, we begin to see that these old buck-passing exercises, although they must be recognized to play a role in their own right, are inappropriate. The reason is that we cannot evaluate them independently (paper 2). Latour (1993: 119) noted that ‘[f]acts are like frozen fish; the chain that keeps them cold must not be interrupted, however briefly’. This also applies to the numerical representations of fish, these cyborg fishes[49] (Holm 2006), that circulate in the cybernetic machinery of fisheries management. Fisheries resource management breaks down as a homeostat when there is no identity between the numbers in the catch forecasts, the TAC negotiations, the catches, and the registered landings. As this point it is not only the black boxes of the stock assessments that leak cod; they may seep out from all the joints of the TAC Machine. And because the

[48] Needless to say this list overlooks the potential role of environmental changes, which underscores the problematic status of the linkages between catch predictions, TACs and landings, and stock health that are assumed in the TAC machinery.
[49] See paper 4.

knowledge production is internal to the cybernetic system, it becomes difficult to say where the chain has snapped and where it remains intact.

Now I hope it becomes clearer why this is both relevant to the discourse on the problems of fisheries management and to the issue of science-politics relationships. In a cybernetic system, politics and science are linked not only ‘institutionally’, but also through models, data and measures of monitoring and controlling fisheries regulations and so forth. Political decision-making about TAC sizes, their distribution, and the control of the fisheries feed back into the properties of the assessments (paper 1 and 2).

Returning to Alcock and Finlayson, this indeed illustrates mechanisms of how assessments are ‘embedded’ in politics (and *vice versa*). But it also clearly illustrates an important sense in which the assessment cannot be ‘autonomous’, at least not in a TAC Machine. Alcock’s tradeoff argument about ‘autonomous assessments’, hence, only works as long as we do not consider how the technologies of assessing stocks and intervening in the fisheries not only link but also transform and redistribute science and politics (in broad senses) throughout the fisheries system. The worse the state of the stocks, the more the uncertainty about them, and the more the management decisions are urgent and disputed. Facts and values become increasingly entangled, and fisheries science, and its relationships with management becomes post-normalized.

Thanks to the old cyberneticians, we now have, in the language of Rittel and Webber (1973), recognized fish-stock assessment to be a wicked problem embedded in the even more wicked problem of fisheries management. This is why the conceptualization of fisheries management as a cybernetic system is very different from ‘the classical systems-approach’ which Rittel and Webber (1973: 162) critically characterize as being ‘based on the assumption that a planning process can be organized into distinct phases’. The knowledge production and the policy-making are not distinct phases in this cybernetic system.[50] Or are they?

[50] Because ‘systems’ and ‘systems theory’ mean very different things in different literatures, I should perhaps also guard against another possible misunderstanding. When I talk about systems, I am not thinking in terms of Niklas Luhmann’s theory of social systems (although there could be similarities between our uses). I take the freedom to use the word ‘system’ rather atheoretically, namely to refer to phenonema in which objects are systematically related. Cybernetic systems, in turn, represent a specific kind of system, and a specific way of considering them.

2.7 *A dubious intermezzo (in which the author considers whether science and politics have been mixed up)*

Let me reveal up front that the question whether science and policy are distinct or not is quite tricky. If it wasn't, however, my entire project would be in trouble. For if the answer to this question was straightforward, why write a whole thesis about it? Besides, if I came up with an answer already, why would you want to gnaw your way through the remaining stack of pages? To you make want to you read on, I need to restore the hopes that a science-politics boundary really can be constructed and maintained – even in fisheries management.

We are already familiar with the device I will mobilize to rebuild the boundary. It is the TAC Machine. As argued in papers 1 and 4, one of the attractive institutional features of the TAC Machine is the conventional division of labour between science and politics it stipulates. Let the scientists do the assessments and the catch forecasts, and let management decide on the TACs and distribute them. When we consider this in combination with the above cybernetic analysis, we realize that the role of the TAC Machine in constructing boundaries between fisheries science and politics is really a double-edged sword. The TAC Machine creates workable boundaries when it runs well, and they break down when it doesn't. In either case, however, we need to take the role of this device into close account.

If this answer doesn't convince you, if you don't believe that a science-politics boundary can be constructed in fisheries management, or at least not enough to make you find it worthwhile to read on, then I would say that there is more to it. What if it was just me who got science and politics messed up, because I'm so keen on bringing my cybernetic approach onto the stage and squeezing it in front of the institutional approach of Alcock and the social constructivist approach of Finlayson? Or maybe I am just making things too French and too complicated? Just think of the fact that nobody, when it comes down to it, really has problems in identifying ICES personnel as those who do the science and those in 'Brussels', that is, in the Fisheries Commission and the Council of Ministers, as those who do the politics. Maybe it is just as simple as that? Maybe science and politics are just two different institutions or organizations that can be more or less embedded in each other?

Yes, maybe I did mess things up. But at least I think you owe me a fair chance to sort them out again. Fisheries science and politics are just so interesting; they heat up so quickly, and all of a sudden you almost lose track of which is which. To avoid this, we need to be much better theoretically equipped next time we venture out in the world of fisheries. Hopefully, we can then better consider the roles of devices, institutions and so forth in constructing boundaries.

In other words, there is no way around the good old academic tradition; we simply have to consult the theoretical literature on science's boundaries. Instead of jumping at fancy new concepts like 'political epistemology' we owe it to the old epistemologists to consider how they used to demarcate science from politics. Before that, however, as always when one has messed things up, it is a good idea to try to retrace one's steps in order to figure out where things went awry. Perhaps we should even try to go back to take a look at how things started.

3. Plato's legacy and its modern challenges

As with so many fundamental aspects of our culture, it makes sense to track the discussion of the science-politics boundary back to ancient Greece. As everyone knows, it was here the tradition of philosophy was born. It was also here that the tradition of political thinking started by, in Isabelle Stengers' words (2000: 60), the 'desacralization' that 'deprives the power of the power to justify itself'. From this event and onwards, power does not imply right, at least not without an argument. If these two traditions, politics and philosophy, were not born as siamese twins, it is at least certain that they were quickly brought into opposition. A significant instance of the latter is represented by Plato who, in the dialogue 'Gorgias', let Socrates invoke a separation of true knowledge (*epistèmè*) and conviction (*pistis*) (Plato 1979). As we know, Plato's ideal was that of a perfect technocracy in which philosophical knowledge is positioned as authoritative. In Plato's ideal republic, just government is in the hands of the philosopher-king, distinguished by his transcendent and invariable knowledge, and by being *disinterested* in worldly matters (Plato 1992).

The reason Plato's dialogue is of direct relevance to the ongoing debates on the relationships between science, politics and society is that his dialogue captures their inherent themes and tensions (see Kochan 2006; Latour 1999; Nowotny et al. 2001). In order to see that, however, we most first acknowledge an important difference, namely that science has taken over the role as the holder of authoritative knowledge from Plato's proposed philosopher-king. To get a glimpse of this (non-platonic) shift from ideas and philosophical reasoning towards empirical sciences, we could start with Stengers (2000), who attributes the invention of modern science to Galileo because he invented the scientific apparatus that could make the phenomenon speak while silencing rival knowledge claims. We can think of Boyle's air pump as such an apparatus that makes Nature – now the only authority of the scientist – reveal the existence of a vacuum. And this time, it is the philosopher, Hobbes, who if not silenced, is left on a historical sidetrack (Shapin and Schaffer 1985). The new separation of *epistèmè* and *pistis*, hence, to a large extent is an achievement of the laboratory and its apparatuses; it is the laboratory that enables a separation of science, which is about facts, from non-science, which is about opinion (Stengers 2000).

The pervasive role of research/science in modern society, however, does not necessarily imply images of a technocracy like that contained in Plato's Republic. On the one hand, Daniels (1967) sets up the ideal of 'pure-science' in opposition to that of democratic culture. On the other, the positivistic flank in 'the science wars' (see below), in direct contrast, promotes science's ideals as a precondition for democracy.[51] Others argue that the relationships between science, technology and society are not fixed, but are up for renegotiation through public engagement with science (Callon 1999; Elam and Bertilsson 2003; Feenberg 2003; Wynne 1992a). In brief, Plato portrays themes and tensions in the relationships between knowledge production and governing that are still being negotiated on several dimensions, of which I will here indicate the following two: (a) Plato's technocratic disposition, expressed by distrust of the roles of politicians and

[51] In his inaugural address, 'Science as an existential foundation', as professor at the Centre for the Philosophy of Nature and Science Studies in Copenhagen, Jens Morten Hansen proposed that 'totalitarian religious, political and other violations of the dignity of mankind to a very large extent also can be characterized as violations of the foundation of natural science and rationalism […] Science must therefore gather its authority on the globally accepted concepts of truth that natural science and rationalism offer, if we are to hope that science can contribute to a new world order' (Hansen 2001) [My translation]. (The speech is available at http://www.nbi.dk/~natphil/prs/hansen/ - visited 21.10.07).

the public, and (b) his unconditional, to use Popper's term, *demarcation* between 'knowledge' and 'opinion'.

The distrust of 'politics' is a main theme in Plato's dialogue. Socrates leaves Gorgias, the rhetorician, speechless by reducing his political craftsmanship to a knack on par with that of cookery. Rhetoric, we learn, is neither about knowledge, nor about skill: it is a kind of flattery. While Callicles, the aristocrat and would-be leader, initially turns the tables, exposing the philosopher's hopeless inexperience in political matters[52], we know the outcome in advance: he is silenced by a reference to *epistèmè,* exemplified by geometry; who can argue against geometry?

Is it not a pity that Plato gets away with this rhetorical move? Would it not have been interesting to hear Socrates' reply if Gorgias had asked him what he thought about basing practical politics on *geometry?* Would it be shameless of me to draw attention to the failure of Plato's personal experience as philosopher-king in Syracuse? I had to turn to the helpful footnotes of the translator, Terence Erwin, to find Isocrates' contemporaneous reply to Plato, in which he ranks '*doxa*' (belief) about useful things above '*epistèmè*' about irrelevant things (Plato 1979: 118). Quite different from the pleasing, unscrupulous and power-greedy image of the rhetorician we get from Plato, the ideal of rhetoric to which Isocrates was committed comprised practical politics as concerns the means, and humanitarianism as concerns the ends (Rummel 1979). By letting the sophists talk back to Socrates here, hence, we arrive at a modern challenge to science produced for politics. It is not only the epistemological properties of knowledge that counts, but also, as it is put in today's literature, its properties in terms of 'saliency', 'legitimacy', 'accountability', 'social robustness' and so on.

Now, Latour (1999) uncovers a hitherto rather neglected elitist alliance between Socrates and Callicles: it is them against the *mob*. While disagreeing on whether the best form of governing is sanctioned in transcendent knowledge or some kind Nietzschean

[52] 'For even if someone has an altogether good nature but philosophizes beyond the right age, he is bound to end up inexperienced in all these things in which anyone who is to be a fine and good and respected man ought to have experience. For indeed they turn out inexperienced in the laws of the city, and in the speech they should use in meeting men in public and private transactions, and in human pleasures and desires; and altogether they turn out entirely ignorant of the ways of men. And so whenever they come to some private or political business, they prove themselves ridiculous, just as politicians, no doubt, whenever they in turn come to your discourses and discussions, are ridiculous' (Latour 1999: 238-239; Plato 1979: 484c-e).

‘might is right’ thinking, they at least agree that there is no use in consulting the *agora*. In other words, the layman of the *demos* is, in Plato’s dialogue, silently silenced. This antidemocratic disposition, explicated in Latour’s account of Plato[53], is challenged within modern discourses on ‘participatory governance’ and ‘democratization of science’.

Last, but not least, Plato’s strict separation of knowledge and opinion is challenged. This is why Latour (1999) read Plato’s dialogue as a contribution – ‘fresh as in 385 b.c.’ – to the modern ‘science wars’, raging between positivist and constructivist interpretations of science. The combination of positivism and technocracy identifies one extreme part in this war, which Latour exemplifies with the view of the Nobel laureate in physics, Steven Weinberg. This view is also found among contemporary interpreters of science.[54] In the other trench, we encounter through and through (social) constructivism combined with various anti-technocratic or anti-technological, dispositions. Since Isocrates and his college, Protagoras[55], in addition to the roles of being mere ‘sophists’ that history has awarded them, were prominent figures within the tradition of scepticism and relativism, it makes sense to put them in this trench. The science war, in other words, can be traced back to quarrels between Plato’s truth-seeking philosophers and the obscurantist and nit-picking sophists. To represent a modern specimen of the latter category, I propose the ‘anarchist of science’, Paul Feyerabend, who wanted to ‘defend society from science’. In Feyerabend’s view, science should be seen as a useful myth among other myths. But it should be formally separated from the state, just as it has historically helped in the process of separating religion from state (Feyerabend 1975b). Let me conclude here by noting that the two most opposed parts in this long war about science, hence, actually disagree on both a and b.

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[53] Kochan (2006) offers an interesting, although rather harsh, critique of Latour’s account by arguing that while Plato (the author of the Republic) was antidemocratic, Socrates was ‘a true-blue Athenian democrat’. He then establishes Socrates’ dialectic method (elenchus) as a precursor of falsification that can be used to examine public beliefs rationally, and thus support a ‘deliberative democracy’. In my view, however, Latour and Kochan agree on much more than they disagree on, since they jointly reject the technocratic dispositions of Plato. Hence, we have here two different *translations* of Plato yielding two different proposals on knowledge in a democracy, which once again points to the relevance of Plato’s dialogues for debating science’s relationship with society at large.

[54] See footnote 51.

[55] The statement (attributed to Protagoras) that ‘man is the measure of all things’ obviously lends itself to a constructivist and a relativist interpretation of knowledge. See Stengers (2000) for an interesting discussion of how this statement can be deployed towards a transformation of the opposition between ‘facts’ and ‘opinion’.

One way to sum things up so far would be to say that while the philosophical tradition, in Whitehead's famous words, can be regarded as 'footnotes to Plato'[56] my emphasis here will be on the contemporary translations, significance and relevance of those footnotes.[57] I started out with reflections on the contrast between *epistèmè* and *pistis* that Plato suggested. Now it is time to leave Plato and the (other) sophists behind and to look at more recent interpretations of the boundaries of science. We leap across more than two millennia. We have to skip the history of the further institutionalization of modern science that was initiated by leading actors such as Galileo and Boyle mentioned above. We also skip over the history of philosophy of science, including figures like Descartes, Kant, and Hume, who obviously have contributed to the history of thought relevant to modern science. While all this may be *relevant* to my project, my ambition is not to understand the development of science and its history of thought. My goal is to study the science-politics boundary in a specific modern context. This is why I jump to an array of twentieth-century interpretations of science's boundaries.

4. Theoretical perspectives on science's boundaries

How to think about and how to conceptualize the boundaries of modern science? In the wilderness of diverging interpretations, which perspectives should I consider? And among these, which to support and which to attack? Although I have just jumped across most of the history of the institutionalization of modern science, I am obviously forced to introduce new constraints. I must explain why I have selected the few perspectives I present and discuss here. My strategy has been to focus on a range of 'classical' positions within philosophy of science and sociology of science. These positions are 'classical' in the sense that they form overly important references in the discourses of which they are part. For instance, they tend to be included in the compendia pertaining to standard courses on the philosophy of science that are commonly mandatory for postgraduate

[56] 'The safest general characterization of the European philosophical tradition is that it consists of a series of footnotes to Plato' (Whitehead, 1929 in 'Process and reality: an essay in cosmology', New York: Harper & Brothers). (Quote taken from 'Brief excerpts from Alfred North Whitehead and Charles Hartshorne': http://www.websyte.com/alan/brief.htm, visited 19.01.08.).

[57] As will become evident, the particular footnotes, with which I here seek to contribute, do not reside within the epistemologically focused tradition of philosophy of science but rather belong to what Annemarie Mol termed 'empirical philosophy' (Mol 2003: 4-7).

students. In other words, the classical positions form ‘obligatory passage points’ (Latour 1987) for both scientists *and* interpreters of science. Accordingly, they serve and obligate me doubly. *First*, they are used to inform scientists about what science is, and hence they contribute to the shaping of science and its boundaries – an issue that pertains to what will be discussed below as the ‘performative’ role of theory (Callon 2007). *Second*, I must take these theories seriously in order to learn from them, and in order to position myself in relation to them. How should I otherwise be able to answer obvious questions such as: Why did you use STS theory? and worse: Why did you *have* to pick one of its most obscure French variants? How can we take your little story on the boundaries of science seriously if you don’t know *Popper?*

It is, therefore, a narrow, but not arbitrary, selection of interpretations of science’s boundaries I offer you below. Although my discussion of positions is largely in accordance with the chronological order of their origin, chronology is not the structuring principle for my account. I have attempted to establish a narrative in which the critique of one position leads to the next but that at the same time respects a thematic structure. I begin with Popper’s demarcation proposal, and I end up with the perspectives I have mainly drawn on in this work, which pertain to Science and Technology Studies (STS) in general, and Actor Network Theory (ANT) in particular.

The thematic structure that I develop is inspired by, but – as will be apparent – not limited to, Gieryn’s (2003) rewarding discussion of the various ways the boundaries of science have been interpreted. In Gieryn’s discussion, and I follow him here, the contrast between ‘essentialism’ and ‘constructivism’ forms a main axis. According to the essentialists, an absolute demarcation of science from other activities is both feasible and desirable. The constructivist, in turn, argues that no such proposed rigid criteria in practice enable a strict separation of science and non-science. But to the extent that such a separation is nevertheless achieved, this is a result of local and contingent negotiations. While the essentialist believes in, and strives for, a formal separation of science and society, this in the constructivists view requires continuous investments in ‘boundary work’ in the form of various mundane practices as well as explicit ideological negotiations – and yet it can never be fully achieved (Gieryn 1983; 1999; 2003).

I suggest that two different metaphorical images of the boundaries can be associated with these interpretations. To the essentialist, the science boundary is like a border between two countries; the country of science, and the country (or wilderness) of non-science. Such a border can be understood as 'natural' (e.g., separated by mountain ridges or rivers), it can be conventional (e.g., a treaty between two neighbouring states), or it can comprise elements of both. Once formed or accepted, however, such a cartographic boundary is rather inert (unless a new 'science war' is unleashed, perhaps). In turn, the constructivist's image of the boundary is a highly dynamic place. It is like a biological cell-membrane. As we know from basic cytology, the creation of gradients across the semi-permeable biological membrane is costly; it requires active work. 'Boundary work' is like the continuous expenditure of ATP in membrane complexes that enables the maintenance of molecular gradients between an intracellular and an extracellular environment.[58] The cell membrane not only identifies a difference; it *makes* a difference.

I will follow this up by offering an account of Latour's (1993) double interpretation of the boundary. On the one hand, Latour's notion of 'work of purification', deployed to separate science from society, bears close similarities to Gieryn's concept of boundary work. On the other hand, interpreting science in terms of heterogeneous networks, as ANT does (see below), could seem to undermine the notion of boundaries altogether. If Science has no 'outside'[59], how can we talk about its boundaries (Latour 1983; 1988)?

I propose to simplify Latour's (1993) scheme by understanding 'purification', some of which can be recognized as belonging to Gieryn's 'cultural repertoires', as identifying certain forms of translation among many others. Aiming to combine the insights of Gieryn and Latour/ANT, I develop a framework for studying the 'co-production' of science and politics. Since it takes the interpreter's role into account, this framework has the potential for being explicitly reflective.

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[58] Readers unfamiliar with modern cytology should refer to the following website: http://users.rcn.com/jkimball.ma.ultranet/BiologyPages/D/Diffusion.html (visited 10.02.08)

[59] '"Science" has no outside, but only narrow galleries which allow laboratories to extend and insinuate themselves into places that may be far away.' (Latour 1988: 226).

4.1 Geographical boundaries and essentialism

In the following, I present some essentialists' positions on the science boundary. Returning to our metaphor, this interpretation of the boundary is like a border that separates the country of science from the country of non-science. I will present different interpretations of what makes science unique. My starting point will be comprised by positions pertaining to standard philosophy of science, which focus on epistemological properties of scientific knowledge. I continue with interpretations that focus on 'social' properties of the scientific community. I intend this contrast between an 'epistemological' focus and a 'social' focus to refer to the contrast between 'internalist' and 'externalist' history of science – without committing myself to a specific interpretation of this contrast. This contrast is in turn related to the contrast between 'context of discovery' and 'context of justification'.

In addition to these contrasts, I find it helpful to the purposes of this discussion to introduce a contrast regarding whether the ambition of the interpreter of science is primarily 'normative' or 'descriptive'. Obviously, the distribution of interpreters of science along this dimension can hardly be performed without considerable interpretational violence. Moreover, there is no way that an interpreter of science can be only normative or only descriptive. In the first case, the interpreter would not be talking about science as we know it. The second case is not possible because there is no 'view from nowhere'. We can think of the descriptive and the normative as two alternative interpretative modes that cannot be entirely separated. As will become clear later on, I do not believe in a sharp separation of the antagonists in terms of the contrasts mentioned here. I introduce them only as a starting point for further discussion.

4.1.1 Epistemological essences

Falsification as the demarcation criterion

It is natural to begin with Karl Popper (2003) who introduced the term 'demarcation' to identify the (normative) separation of the empirical sciences from mathematics, logic and 'metaphysical systems', including, with a more pejorative term, 'pseudoscience'. However, while Popper shared the ambition to separate science from metaphysics with the contemporaneous programme of 'logical positivism', which in particular was associated with the early writings of Wittgenstein and the writings of Carnap, he did not share the latter's anti-metaphysical, say *Humean*,[60] attitude. Popper realized that this programme was subjected to the logical problem of induction (addressed by Hume) and his notorious falsification programme represented his proposal to solve a double problem. It not only represented his solution to the inductive problem but also his suggestion for solving the 'demarcation problem'. Any theory that does expose itself to falsification by empirical evidence (Popper's favourite examples were psychoanalysis and Marxism) is, by Popper's criterion, to be purged from the category of (empirical) sciences.

What kind of science boundary does falsificationism represent? Gieryn's (2003) answer is that it invokes an essentialist's boundary of an epistemological kind. It is useful, however, to let Popper qualify that himself. Popper takes care to position his project in opposition to 'a study the actual practices of scientists', i.e., what he called a *naturalistic* methodology. His project was explicitly methodological, critical and normative. Falsification was what he proposed as the new rules of the game of science. While taking logic as its starting point, the science boundary based on falsification in this sense is 'conventional' (Popper 2003: 29-34).

The doctrine of falsification is immensely influential. It is ubiquitous in the curriculum for any of the standard courses in the philosophy of science that are mandatory for postgraduate students. Accordingly, we must expect that the normative impact on scientists' practices is also significant[61]. There is no doubt that Popper's contributions have been very valuable for the development of methodologies in science. In my view, the central tenet of falsification, i.e., that a hypothesis must be *bold* to be scientifically useful, is sound. To put it bluntly, the hypothesis that 'it will rain tomorrow or it will not rain tomorrow', just doesn't have the guts it takes to make science. Science, is a risky enterprise because you risk being wrong, and you should risk that. Moreover,

[60] Popper (2003: 12) actually referred to the following passage of Hume: 'If we take in our hand any volume; of divinity, or school metaphysics, for instance; let us ask, does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames. For it can contain nothing but sophistry and illusion'. (Hume, cited in Lakatos 1978: 2).

[61] Nevertheless, I have only encountered two explicit references to the methodology of falsification within fisheries science (Corkett 2002; Ulltang 1998). This does not necessarily imply that Popper's normative impact in fisheries science is negligible, however. My interpretation is that some of the difficulties of falsificationism (i.e., the Quine-Duhem thesis – see below) evidently are particularly intractable within the science of rather open systems such as the fisheries.

since the addition of trivial confirmations of a statement provides little new knowledge, it makes sense to deliberately look for instances that seem to offer it a challenge.

While falsificationism generally has been well received within science, it has, been extensively criticized by philosophers as well as by members of other disciplines such as sociologists and historians of science. The main problem[62] of putting more dogmatic versions of falsificationism into practice seems to be captured by what is now referred to as the Duhem-Quine thesis.[63] Quine (1961) argued that modern empiricism is conditioned on two 'dogmas'. The first dogma is Kant's distinction between truths that are 'analytic' (independent of fact) and 'synthetic' (based on fact). The second dogma is 'reductionism', the belief that each meaningful statement is equivalent to some logical construct upon terms that refer to immediate experience. Basing his argument on logics and semantics, Quine forcefully demonstrated that the distinction between analytic and synthetic statements can only be grounded in elliptical arguments – it is, he concluded, but a 'metaphysical article of faith' (Quine 1961: 37). This point undermines the second and linked dogma, which Quine (1961: 41) rephrased as 'the supposition that each statement, taken in isolation from its fellows, can admit of confirmation or information [i.e. disconfirmation]'.

While this argument explicitly addresses the (logical empiricists') theory of Carnap, it clearly also has consequences for falsification theory. In Quine's view (1961: 42-43), a consequence of the argument is that '[t]he unit of empirical significance is the whole of science', including language terms, observations, theory and even logical inference rules. However, 'the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reevaluate in the light of any single contrary experience'. While this led Quine to adopt a rather pragmatic view of how science works, it also made him conclude that the death of the two dogmas implied 'a blurring of the supposed boundary between speculative metaphysics and natural science' (Quine 1961: 20).[64]

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[62] A second difficulty relates to the fact that most science is based on statistics, since probability estimates, strictly speaking are, not falsifiable (Popper 2003).

[63] See Lewthwaite (2003) for a discussion of the Duhem-Quine thesis. (The document is available at http://www.ecclectica.ca/issues/2003/1/lewthwaite.asp - visited 06.11.07).

[64] In some passages, Quine (1961: 44) approaches Feyerabend's (1975b) view that science is a myth among other myths: 'In point of epistemological footing the physical objects and the [Homerian] gods differ only

Sophisticated falsificationism and the methodology of research programmes

If, as suggested above, the main challenge to falsificationism is to locate what is falsified, this in turn leads to difficulties in enabling a strict demarcation based on epistemological criteria. Evidently, Popper was aware of these problems. However, instead of trying to find out whether the historical Popper considered these problems in sufficient depth, and whether he was able to respond adequately to them[65], this is where I let Popper's important follower, Lakatos, step in, dedicated to rescue not only Popper, but also the ideas of rationality and progress of science.

In a direct response to Quine, Lakatos (1978: 99) offered an illuminating metaphor of falsification: a scientific statement or theory is like a nut that can be tested by the hammer of an accepted statement on the anvil of uncontested background knowledge. Devising a 'crucial experiment' corresponds to the hardening of these tools, and - Bang! - our bold little nut is either corroborated or falsified into dust. Now, what Lakatos allowed to the Duhem-Quine thesis is that the decision of what to dismiss in the nut-tool constellation, once the nut is squashed, is arbitrary. Hence, *naïve* falsification works neither as a methodological principle, nor as a demarcation principle. To overcome this arbitrariness, Popper developed *sophisticated* falsificationism, which 'allows *any* part of the body of science to be replaced', *provided* that the replacement is 'progressive'. The replacement of a theory is progressive if it has led to the corroboration of 'excess empirical content', which means that it has led to the discovery of new facts while still being able to explain what its predecessor was able to explain (Lakatos 1978: 31-47).

Hence, instead of focusing on a single theory, sophisticated falsificationism takes a series of related theories as its object, and this became the starting point of Lakatos' 'methodology of scientific research programs'. Such research programmes are characterized by a hard theoretical core, which is sheltered from immediate falsification by emerging anomalies by 'a protective belt' of auxiliary hypotheses. The research programme comprises methodological rules about what to avoid ('negative heuristic',

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in degree not in kind'; the epistemological difference is rather that 'the myth of physical objects' has proved to be more effective in structuring experience.

[65] Lakatos (1978: 93-94) distinguished between the dogmatic falsificationist ($Popper_0$) whom he mainly considered a straw-Popper invented by critics; the naive falsificationist ($Popper_1$); and the sophisticated falsificationist ($Popper_2$). He continued noting that: 'The real Popper developed from dogmatic to a naïve version of methodological falsificationism in the twenties; he arrived at "*acceptance rules*" of sophisticated falsificationism in the fifties'.

e.g., confronting the core with anomalies), while other rules concern the direction in which to go ('positive heuristic', e.g. the suggestion and development of hypotheses). Lakatos' methodology of research programmes not only offers a rational reconstruction of the succession of theories within such series, but also of the succession of 'progressive programs' (in which new theories and facts are proliferating) over 'degenerative programs' (in which anomalies pile up while theory and fact production is slow) (Lakatos 1978).

The methodology of research programmes comprised Lakatos' proposal to distinguish science from pseudoscience, and scientific progress from intellectual decay (Lakatos 1978: 1-7). As I hope follows from the above presentation, we can characterize Popper's and Lakatos' demarcation attempts as normative projects, carried out with an epistemological focus.[66]

The paradigm as a boundary criterion and the role of history

The role of history of science is the issue that perhaps most clearly opposes the projects of Lakatos and Popper with Thomas Kuhn's project that will be discussed in the following. The theories of Lakatos and Popper not only devised a sharp demarcation between science and pseudo-science, but also an equally sharp demarcation of science's internal and external history (Lakatos 1978: ch. 2). In Lakatos' view, 'internal history is self-sufficient for the presentation of disembodied science', while '[e]xternal history explains why some people have false beliefs about scientific progress'[67] (Lakatos 1978: 117). In his very influential work on 'the structure of scientific revolutions', Kuhn (1970) aimed at developing a more profound role for history than that of a chronology of science's achievements and its anecdotes – which made him suspend the distinction of context of discovery versus context of justification.

[66] Lakatos, however, would not agree to the 'epistemological' characterization of the methodological version of Popper's falsification theory as well as of his own sophisticated version: 'Popper's demarcation criterion has nothing to do with epistemology. It says nothing about the epistemological value of the scientific game' (Lakatos 1978: 156). This may be because his concept of epistemology is committed to a form of correspondence theory of truth. My concept of 'epistemology', as used in this text, in turn refers to properties of knowledge in general.

[67] This position is opposed by the methodological 'principle of symmetry' in the strong programme of Sociology of Scientific Knowledge (SSK): SSK 'would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs' (Bloor, cited in: Law 2004).

Kuhn's approach was highly influential within the later traditions of social studies of science. He is often regarded as one of the founders of post-Mertonian sociology of science, characterized by a change from the sociology of scientists to the sociology of scientific knowledge (Law 2004). While we can think of Kuhn's project as primarily descriptive and being simultaneously focused on epistemological and 'social' properties of scientists, he is also occasionally explicitly normative.[68] However, it makes sense to interpret Kuhn as an 'internalist' since his histories of scientific developments portray the internal dynamics of a seemingly *autonomous* science community – autonomous in the sense that the links of this community to its societal environment remain undeveloped (Stengers 2000). Yet, this is quite another form of internalism from that of Lakatos and Popper, since the 'social' for Kuhn is intrinsically linked with the 'epistemological' (i.e., in the paradigm), and since he is more interested in describing science's dynamics than in conferring on it a superior methodology or a rational reconstruction.

Kuhn's notorious concept of the 'paradigm' was pivotal to his interpretation of science. In its broad sense[69], the paradigm denotes 'the entire constellation of beliefs, values techniques, and so on shared by the members of a given community'. In brief, Kuhn (1970) characterized the development of science – which in his view is not the same as its progress – as the transition between paradigms in which 'puzzle solving' activities or 'normal science' takes place. If a paradigm becomes bogged down in a growing number of 'anomalies', this is associated with a transition from 'normal science' to 'crisis', with a shift to a competing paradigm as an impending outcome. In Kuhn's view, such a shift cannot entirely be rationally justified, among other things because of 'incommensurability' between different paradigms. According to Kuhn, therefore, a paradigm shift represents a *gestalt*-switch; it allows for a new perspective while simultaneously precluding another (Kuhn 1970).

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[68] In his introduction, Kuhn (1970: 8) offers the following methodological reflections: 'History, we too often say, is a purely descriptive discipline. The theses suggested […] are, however, often interpretative and sometimes normative […] many of my generalizations are about the sociology or social psychology of scientists; yet at least a few of my conclusions belong traditionally to logic or epistemology.'

[69] In a postscript written in 1969, Kuhn explained two main uses of his concept. In its narrow sense, 'paradigm' identifies one important element of the paradigm in a broad sense, namely 'the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science' (Kuhn 1970: 175).

Although Kuhn did not pay much attention to the issue of the boundaries of science, it is clear that his understanding of this issue is tied in with his notion of the paradigm. For instance, this is obvious from the following passage from his 1969 postscript: ‘A paradigm is what the members of a scientific community share, and, conversely, a scientific community consists of men who share a paradigm’ (Kuhn 1970: 176). Kuhn’s discussion of pre-paradigmatic sciences as ‘immature’ (Kuhn 1970: ch. 2; 178-179) provides another illustration of how the boundary of a science is defined by its paradigm.

Since the paradigm involves some kind of a negotiated consensus it may be surprising that I have categorized Kuhn as an ‘essentialist’ as regards the science boundary. Here I follow Gieryn (2003) and briefly present his argument for doing so. As indicated, Kuhn is not very interested in the science boundary. In fact, he asks:

> Can very much depend upon a *definition* of science? Can a definition tell a man whether he is a scientist or not? If so, why do not natural scientists or artists worry much about the definition of them? (Kuhn 1970: 160)

To this Gieryn responds that ‘Kuhn is an essentialist not because he offers paradigmatic consensus as a demarcation principle but because he dismisses as unimportant, merely “semantic”, those questions that animate constructivist studies of boundary work’ (Gieryn 2003: 403). In other words, Kuhn’s interpretation of the science boundary is essentialistic because it is inert. Although in a different sense than Lakatos’, Kuhn’s interpretation of science is internalist since he does not take into account how science may interact with society (Hacking 1999: 43; Jasanoff 2006a: 276).

Defying boundaries: Anarchistic epistemology

Since I will argue below that some of Feyerabend’s works[70] can be read as an attempt to undermine the science boundary, it may be puzzling that I have categorized him as an ‘essentialist’. My argument for doing so is similar to Gieryn’s argument for positioning

[70] Note that here I only refer to the later views of Feyerabend (say, from about *Against Method* onwards). Feyerabend’s late works are in direct conflict with his early works, including his early positivism and his dedication to falsificationism in the first part of his philosophical career. No one can accuse (the later) Feyerabend for not knowing what he was going up against!

Kuhn as an essentialist. It is as if Feyerabend says: Either Popper and Lakatos are able to define a rational and objective science-boundary, or there is no boundary at all! In my scheme, this makes him an essentialist – albeit in a negative sense – because he does not consider the possibility of a pragmatic and practical interpretation of the boundary.[71]

In his book, *Against Method*, Paul Feyerabend asked whether it is 'possible to have both a science as we know it and the rules of a critical rationalism'. Evidently, the term 'critical rationalism' refers to the programmes of Popper and Lakatos. Based on a close examination of how major scientific achievements came about *in practice*, Feyerabend answered his own question with 'a firm and resounding *no*' (Feyerabend 1975a: 175). One of his principal empirical cases concerns how Galileo proceeds in promoting the shift from a geocentric to a heliocentric worldview. From a rather detailed examination of Galileo's work and his struggle to convince his opponents about its significance, Feyerabend concludes that had Galileo been committed to the methodology of critical rationalism, he would have abandoned his research programme before getting it off the ground. Fortunately, he did not do that. Instead Galileo made his theories plausible by deploying a clever mix of ad-hoc hypothesis, propaganda, and speculative experience, in addition to facts that were recast in a new observational language (Feyerabend 1975a). Only much later, implies Feyerabend, would Galileo's theory comprise excess empirical content etc., from which it would defeat a competing theory on 'rational' grounds.

Now, at the outset, this neither poses a challenge to Popper's sophisticated falsificationism nor to the methodology of Lakatos, who readily admitted that 'all theories […] are born refuted' (Lakatos 1978: 5). The question is whether there can be 'objective' – which Lakatos (1978: 69), with a direct reference to Kuhn, opposes to 'socio-psychological' – reasons or standards for deciding when to abandon a research programme or a paradigm. Popper and Lakatos said yes and aimed at specifying normative methodological criteria. Kuhn and Feyerabend said no, arguing from their rich and convoluted historical descriptions of the practices that lead to major scientific achievements. Feyerabend's thesis, hence, can be considered a *reductio ad absurdum* argument, launched against rigid 'rational' methodologies. When he mockingly dedicates

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[71] Gieryn (2003) does not discuss Feyerabend's work.

his book to Lakatos as a ‘fellow anarchist’, it is because he argues that such ‘objective reasons’ would either be so rigid that they would preclude a range of major historical scientific achievements or be so lax that the methodology of which they would be part in practical terms would become indistinguishable from Feyerabend’s ‘anything goes’ – the only methodological dogma that, in his view, will not inhibit scientific progress (Feyerabend 1975a).

Feyerabend does not object to the *possibility* of a distinction between a context of discovery and a context of justification. In contrast, he finds that these represent alternative, frequently conflicting but equally important modes in science. To let the mode of the context of justification overrule the mode of the context of justification, however, would be ‘disastrous’ (Feyerabend 1975a: 165-169).

While Feyerabend debunks rational epistemologies based on his meticulous descriptions of the messy practices of a number of scientific heroes, his objectives, including his encouragement of ‘epistemological anarchism’, are highly normative. Obviously, the residual methodology of ‘anything goes’ to promote scientific progress is in itself normative. Yet, in addition to this, Feyerabend wanted to promote such epistemological anarchism for *humanitarian* reasons.

Let us now reach our main concern here, namely Feyerabend’s interpretation of the science boundary. As we have seen, Feyerabend strongly objects to methodological demarcation criteria, which he finds are neither realistic nor desirable. What is science then, and what are its boundaries? This question does not seem to occupy Feyerabend as much as his dismantling of demarcation criteria and methodologies. We are left with rather open characterizations of science as an ‘ideology’[72], a ‘myth’ [73] or a ‘form of thought’.[74] Such characterizations obviously fit in with Feyerabend’s quest to protect society against science as a dominating ideology. But they leave us with a problem since we here are interested in the boundaries of science. While Feyerabend, at least to the extent that we can support his account, managed to trample down the epistemological fence of the ivory tower of science, he did not care to replace it with another way to understand what differentiates science from other social activities: we are left with a

[72] ‘Science is just one of the many ideologies that propel society’ (Feyerabend 1975b).
[73] ‘[S]cience is much closer to myth than scientific philosophy is ready to admit’ (Feyerabend 1975a: 295).
[74] Ibid.

science without boundaries. How disappointing! All these epistemological struggles of philosophers of science have been fruitless; science is really not different from dubious activities like astrology (Feyerabend 1975a) or voodoo (Feyerabend 1975b).

It is too early to give up on our quest to understand science boundaries, however. The reason for this is related to the reason why Stengers would have preferred that Feyerabend had announced a 'farewell to epistemology' rather than his 'farewell to reason' (Stengers 2000: 36).

4.1.2 'Social essences' of science

In his classical essay, 'The Ethos of Science', Robert Merton ([1942] 1996: 267) addressed the 'complex of values and norms which is held to be binding on the man of science'. Merton regarded this study of the 'cultural structure of science' as 'one aspect of science as an institution'. The starting point for Merton is the appropriate observation that the common use of the word 'science' is polyvalent. In Merton's view the common use 'science' respectively denotes a characteristic methodology for certified knowledge, knowledge produced from such methodology, the cultural values governing activities termed scientific, or some combination of the former. The fact that Merton saw these uses of 'science' as *distinct* (although interrelated) is important because it indicates what he saw as the appropriate subject of the discipline, the sociology of science, that he had such an important role in creating. This becomes clear in the following passage:

> To be sure, methodological canons are often both technical expedients and moral compulsives, but it is solely the latter which is our concern here. This is an essay in the sociology of science, not an excursion in methodology. Similarly we shall not deal with the substantive findings of sciences […], except these are pertinent to standardized social sentiments about science. This is not an adventure in polymathy. (Merton 1996: 267).

Mertonian sociology of science, hence, is about its 'cultural structure', its norms and its sentiments as opposed to its epistemology, methodologies and practices. Merton, hence, accepts the role-division between philosophy and sociology of science that builds upon the distinction between the context of discovery and the context of justification (Holm

2001). This could well be a main reason for Merton's relative popularity among ('analytical') philosophers of science (Richardson 2004). While this interpretation of Mertonian sociology of science may not be uncontestable[75] – at least I think it is fair to say that 'The Ethos of Science' does not protect itself from such a reading. It serves to illustrate a position that is different from, but not necessarily in opposition to, the epistemologically founded demarcation attempts discussed above.

The 'ethos of science' can, hence, be interpreted as the demarcation criterion of science as regards its cultural structure, relevant to the (Mertonian) sociology of science. The social border between science and non-science is comprised by the four sets of 'institutional imperatives' that Merton (1996) proposed to constitute this ethos of science, namely 'universalism', 'communism', 'disinterestedness', and 'organized skepticism'. In Gieryn's (2003: 398-400) view, this demarcation argument is 'just as essentialist as Popper's'. It parallels the strong normativity deployed by a Popperian interpreter of science (i.e., 'if you don't stick to The Ethos, you are not part of science'). I do not intend to imply that these imperatives are merely Mertonian inventions; they are actively played out in science. As I will discuss below, such imperatives are in fact used to define people in and out of science. The ethos, however, becomes essentialist when insufficient empirical attention is given to how it is interpreted, negotiated and deployed in different contexts (Gieryn 2003). Let this be our reason to move on from such essentialist conceptions of the science boundary[76].

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[75] This interpretation of Mertonian sociology of science matches Mulkay's characterization of what he saw as the 'dominant' perspective within sociology of science (Mulkay 1979). Latour excels in assembling somewhat strawman-ish versions of such sociologies of science to illustrate what he is up against (e.g., Latour 2005). Cole (2004) not only defended Merton against the accusation of positivism about scientific knowledge, but also against the accusation that such Mertonianism has ever been dominant. (From a perspective of sociology of knowledge, however conceived, I find this mutual accusation of each others dominance rather interesting.) Gieryn (1982) turned the tables against the programmes of constructivism and relativism within SSK, arguing that these, from a Mertonian standpoint, were redundant. See Collins (1982) and Knorr-Cetina (1982) for two among several replies.

[76] In my view there are other important reasons to leave Merton's boundary interpretation behind. Fundamentally, I find it implausible to narrow the scope of the interpreter of science to a 'social' perspective. This will be addressed in my interpretation of ANT's perspective of the science boundary.

4.2 The cell membrane metaphor and boundary work

Within STS, including the Sociology of Scientific Knowledge, Ethno-methodological Studies of Work and ANT, epistemology and the practical work of constructing and maintaining science's boundaries are recast as *empirical*, or to use Poppers term, naturalistic, problems[77]. If essentialism and the cartographical image of the science boundary was a wrong track as concerns my purposes (at least regarding the positions I have selected and discussed) it is because relatively little *work* is done at such a boundary.

The essentialists cannot offer us much in relation to studying how the science-politics boundary is constructed, maintained or transgressed in practice; they can only tell us how they (according to different criteria) ought to differ. This is where I propose the image of a cell membrane as a more fertile metaphor for empirical studies of boundary construction. A cell membrane is a highly complex and dynamic site of exchange. Some cellular products are actively transported through the membrane in an energy-demanding process. Other components diffuse slowly through the membrane's pores. The main point is that the membrane as a boundary – in contrast to, for instance, a convention – represents an active site that has immediate influences on the properties of both the internal and external environment.

Gieryn (1983) defined boundary work of scientists as:

> Their attribution of selected characteristics to the institution of science […] for purposes of constructing a social boundary that distinguishes some intellectual activities as 'non-science'. While sociologists and philosophers struggle about the demarcation problem in a selection of journals, boundary work is routinely accomplished in practical, everyday settings: education administrators set up curricula that include chemistry but exclude alchemy; the National Science Foundation adopts standards to assure that some physicists get funded but no psychics get funded; journal editors reject some manuscripts as unscientific (Gieryn 1983: 781).

[77] Wittgenstein has been credited as a main source of inspiration for this development, in particular through his concepts of 'family resemblance' and 'language games' (Lynch 1992). The notion of 'boundary work', viewed in contrast to an 'essentialist demarcation', could be interpreted as related to the pragmatic notion of 'meaning is use' of the later Wittgenstein (1967).

‘Boundary work’[78], hence, invites empirical research throughout a range of areas.

Although he credits some works of Mulkay for the term, Gieryn (1983: 2003) draws attention to the useful concept of ‘cultural repertoires’:[79]

> Scientists have a number of ‘cultural repertoires’ for constructing ideological self-descriptions, among them Merton’s norms, but also claims to the utility of science for advancing technology, winning wars, or deciding policy in an impartial way. (Gieryn 1983: 783).

Popper’s and Merton’s interpretations of science, for instance, can fruitfully be seen as simultaneously comprising interpretations of, and contributions to, ‘cultural repertoires’, available and relevant to scientists and others for constructing boundaries. Research on the deployment of such repertoires recasts essentialist epistemological and normative interpretations of science as empirical studies of scientific practices. It took quite some pages to get to this point. But it may comfort the reader to know that since this point figures among the central theoretical tenets of my work, the effort of reading of the previous pages has not been entirely in vain.

While the concepts of ‘repertoires’ and ‘boundary work’ discussed above are useful to understand discursive strategies in relation to scientific boundary construction, I will argue that Gieryn’s approach has a major weakness: there is no account of how the material and cognitive practices within science make a difference to what science is. Crudely, we could ask: Is the deployment of certain ‘cultural repertoires’ sufficient to make someone a scientist? Clearly this is not so. Recall that Stengers (2000) established the *laboratory* as the device that allows the separation of knowledge and opinion. How would ICES, for instance, provide scientific advice on fisheries management without models and data? Is it not obvious that the way ICES models fisheries resources contributes to the framing of decision-making about these resources? Who would deny Degnbol’s (2003) observation that the science of fisheries developed in close interaction with the emerging fisheries management institutions? In what sense can it be plausibly

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[78] See (Gieryn 2003) for a further development of the notion of ‘boundary work’ and for a presentation and discussion of a range of illustrative examples.

[79] The reader may also turn to Rip’s interesting and related notion of repertoires (Rip 1994). As an alternative (or complement) to the notion of repertoires, we could, following Latour (2005: 204-213), think of ‘plug-ins’ for the scientist.

held that the boundary between science and management has not developed too? Let this be the motivation for moving on to (Latourian) ANT, which was originally developed to study scientists in their laboratories (Latour 1987; Latour and Woolgar 1986).

4.2.1 ANT and the double interpretation of the science boundary

Is ANT's notion of heterogeneous networks inconsistent with the idea of boundaries? I will argue that Latour's (1993) 'we have never been modern' suggests that the answer to this question is yes *and* no. Using one of Latour's favourite cases, I illustrate his suggestion that science is dependent on transgression of traditional boundaries. This case will also serve to illustrate the abovementioned weakness of Gieryn's approach, as well as to point out how this weakness can be overcome by studying 'science in action' and by 'following scientists through society' (Latour 1987). Towards the end of this chapter, I propose a framework for studying the science-politics boundary that combines Gieryn's approach with Latour's. When I shift to the empirical use of this framework (section 5.4 below), I will introduce some additional STS 'boundary concepts' that usefully can be reconciled with this already eclectic approach.[80]

In Latour's (1993) 'we have never been modern', which occupies a quite central position in the ANT literature, the question of scientific boundaries is caught up in the twin concepts of translation and purification.[81] On the one hand, Latour interprets the

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[80] Is what follows my approach, Latour's approach or an eclectic blend of Gieryn's and Latour's approaches? Frankly, I cannot say for sure – it depends on how you prefer to look at it. However, I am less preoccupied with this question than with developing a theoretical and methodological approach that I find useful for my work here. I am not aware of occurrences of the concepts of 'science boundary' and 'boundary work' in Latour's works. Latour prefers to stick to his own concepts that are often just as open as they are inspiring. Likewise, it does not appear to me that Gieryn is very interested in ANT. This reciprocal lack of interest could be set off by the disagreements that I discuss here. It must be recognized, however, that my attempt to link some of Latour's ideas to some of Gieryn's is conditioned by my rendering of both of them.

[81] The concepts of 'translation', 'heterogeneous networks' and 'purification' (Callon 1986; Latour 1987; 1988; 1993; 2005) are crucial to ANT but also, it must be said, a bit difficult to comprehend for a reader not familiar with ANT. One reason for this is that they form part of what Latour calls an 'infra-language' (Latour 1996; 2005: 30), which is designed to be vague at the outset, allowing for the 'displacement from one frame of reference to the next', and to be explicated 'in situ'. The underlying methodological idea is that the ANT researcher seeks to carry out an empirical examination with the minimum of conceptual baggage that this 'infra-language' comprises an effort to avoid importing predefined concepts into the analysis, which would frame the outcome of the analysis in advance (Latour 2005).

Since I cannot undertake to present and analyse ANT in depth here, I have to take it for granted that the reader is somewhat familiar with it. Except from how I address (Latourian) ANT in the main text, underlying nature of science as translation and (heterogeneous) network construction processes in which boundary transgressions are crucial. On the other hand, he recognizes the tremendous work of purification deployed to separate ‘nature’ from ‘society’. While heterogeneous networks, for instance, ‘link in one continuous chain the chemistry of the upper atmosphere, scientific and industrial strategies, the preoccupations of heads of state, the anxieties of ecologists’; the work of purification divides this into ‘a natural world that has always been there’ and ‘a society with predictable and stable interests’(Latour 1993: 11). This ‘modern’ separation is authorized by what Latour terms ‘the Modern Constitution’ which ‘invents a separation between the scientific power

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however, I will make some remarks on my interpretation of ANT in this and in some of the following footnotes. To readers unfamiliar with ANT, I suggest that Latour’s recent introduction (Latour 2005), his brief paper ‘On actor-network theory – A few clarifications’ (Latour 1996), combined with some case studies (e.g. Latour 1987; 1988) could be a good start.

The following is an attempt to offer a sketch of ANT as it is rendered in *Reassembling the Social* (Latour 2005). Heterogeneous actor-networks result from the attachments of actors to things and concepts and other actors. ANT is hence *relational* as opposed to *essentialist*. In ANT’s view, there are no essences because identities (e.g., of actors) depend on what networks they are caught up in. Instead of picturing independent surfaces or substances in three dimensions, ANT pictures networks in which each node comprises as many dimensions as it has connections (Latour 1996). The form of such connections, be it material, semiotic, ‘social’, or of some hybrid nature, is not pre-given; indeed the way that new connections contribute to the repertoire of connections is in empirical focus (Latour 2005: 233). ANT is not a sociology but an associology; a sociology of associations: ‘There is no society, no social realm, and no social ties but, *but there exists translations between mediators that may generate traceable associations*’ (Latour 2005: 108). ‘Translation’ is the connection that transports transformations, and the ‘network’ is the traces of this process that a scholar is able to record (Latour 2005: 108). While an ‘intermediary’ ‘transports meaning or force without transformation’, ‘mediators’ ‘transform, translate, distort, and modify the meaning or the elements they are supposed to carry’(Latour 2005: 39). Because ANT sees this uncertainty of the ‘mediators’ as fundamental, it can be considered a sceptical programme that aims at taking this uncertainty into account. Understood as a sceptical programme, ANT considers five uncertainties that must be ‘piled up’ when doing ANT, only to be resolved ‘in situ’ (Latour 2005):

1) Uncertainty about existing groups. Instead of reification of classifications imported by the analyst, the advice is to study *group formation* (e.g., boundary work) (Latour 2005: 27-42).
2) Uncertainty about what acts. Hence, the operational definition of an actor as what is made act by many other actors (Latour 2005: 43-62).
3) Uncertainty about the nature of agency. The nature of agency in human/non-human assemblies is left open (Latour 2005: 63-86).
4) Uncertainty about the ‘social’ and the ‘natural’. Hence, ANT prefers to think in terms of ‘matters of concern’ instead of ‘matters of fact’ (Latour 2005: 87-120).
5) Uncertainty about how to write an account. The text is an experiment that may fail ‘to perform the social’ (Latour 2005: 121-140).

These five points render ANT a sceptical programme in a more general sense than is captured by ‘generalized symmetry’. Latour’s (2005) rendering of the ANT programme in *Reassembling the Social* takes it to an interesting extreme; it is an idealization of ANT as a relational ontology (see note below) and a sceptical methodology. It goes without saying that this sceptical rendering of ANT is extremely demanding, if not outright impossible to follow to the letter in practice. This does not, however, preclude it from being useful to stimulate the analyst’s ability to reflect, and to encourage the development of new possible forms of descriptions/explanations.

charged with representing things and the political power charged with representing subjects' (Latour 1993: 29). When Latour provocatively exclaimed, 'We have never been modern', it is because the heterogeneous actor-networks, which transgress categories like 'nature' and 'society', constitute ANTs privileged level of analysis – in fact, in a certain sense, its ontology.[82] Latour explicitly writes that this does not mean that modernity is an illusion. Rather, in his picture, the 'absolute dichotomy' between nature and society is possible because the 'moderns' never consider the work of purification and translation together; they are practised separately (Latour 1993: 40). The appropriate – I am not sure if I can say consistent – title of Latour's book would therefore be, 'We are but we have never been modern', depending on whether the focus is on (the product of) the work of purification or the work of translation.

[82] Since ANT occasionally talks about 'ontology' it is important to note that the meaning of ontology in ANT differs from its more traditional meanings in philosophy (if we can talk about anything 'traditional' as regards such a heavily debated concept). In order to see this, I find it illustrative to contrast ANT with a modern set of definitions of ontology and epistemology in analytical philosophy of science. Allow me to use here the definitions presented to me in a lecture that was given as part of a PhD course on the philosophy of natural science by Finn Collin, professor in philosophy at the University of Copenhagen. His handout reads: Strict ontological objectivity: 'It is possible to define a conception of the world as it is "in itself", independently of the contribution from any subject of cognition'. Strict epistemic objectivity: 'There are epistemic methods and procedures that actually make it possible to reach an ontologically objective picture of the world'. At most, ANT would be agnostic to these possibilities. While it is essential to Collin's definition that ontology and epistemology are conceptually separable, this is never the case in ANT. For ANT, 'ontology' and 'epistemology' are always entangled and situated; knowledge and its objects are relational entities, developed inside a network. It follows that there is no view from outside a network, that is, from nowhere. To understand a network one must connect to it. While there is no view outside the networks, it is possible to study the transformation of networks, and to triangulate between different network positions. Here, the cartographical metaphor is appropriate: ANT can be thought of as an exercise in the mapping of actor-networks, which should be thought of in terms of processes, rather than in terms of entities. An actor-network, says Latour, 'is the trace left behind by some moving agent' (Latour 2005: 132).

It is worth noting that just as Kant dismissed his '*ding an sich*' as uninteresting, there is nothing interesting for ANT behind or between the filaments that make up the networks. Latour (2005: 242) asks: 'If it's true, as ANT claims, that the social landscape possesses such a flat "networky" topography and that the ingredients making up society travel inside tiny conduits, what is between the meshes of such circuitry?' To this he answers: 'I call this plasma, namely that which is yet not formatted, not yet measured, not yet socialized, not yet engaged in metrological chains, and not yet covered, surveyed, mobilized or subjectified' (Latour 2005: 244). Hence, no unmediated representation is possible; it is rather as if ANT thinks of this as a contradiction. And if something is not somehow represented, it is boring, indifferent 'plasma'.

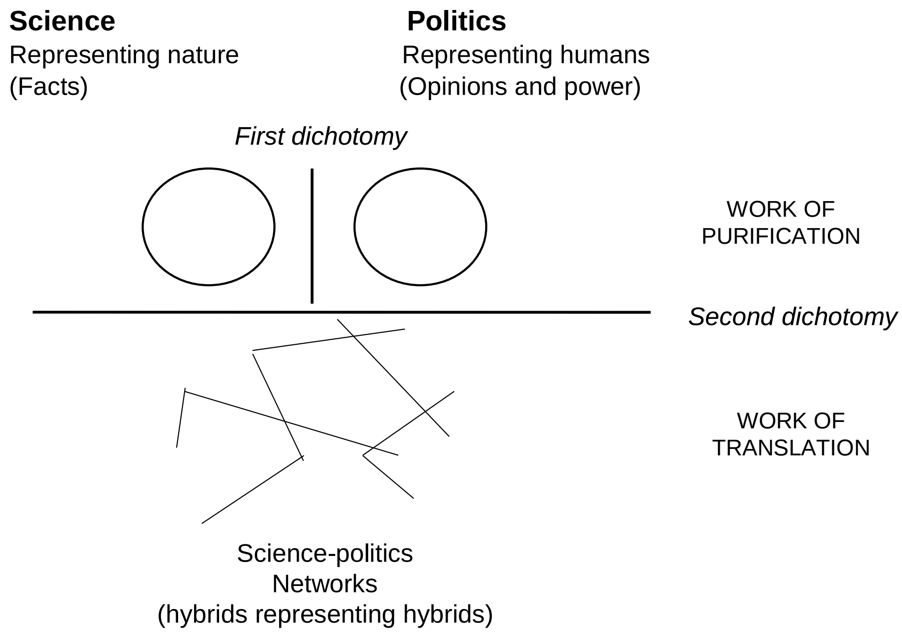

Fig. 1. Purification and translation of science and politics.
The figure is an 'inversed' version of Latour's (1993: 11) figure in the sense that the focus is shifted from what is *represented* ('nature/non-humans' and 'culture/humans') to what *represents* ('science' and 'politics'). See text for explanation.

Figure 1 recapitulates the simultaneous processes of translation and purification.[83] The lower part of the figure portrays actor-networks that link heterogeneous entities across traditional (conceptual) boundaries, producing and representing new hybrids (lower half of Figure 1). Sanctioned in the Modern Constitution, the work of purification is simultaneously performed to separate science and politics (upper half of Figure 1). I propose that in this scheme, 'the work of purification' corresponds to (but is not necessarily limited to) a group of cultural repertoires used for performing 'boundary work', as covered by Gieryn, in order to safeguard science's knowledge authority. This includes repertoires used for defining and policing boundaries, and for excluding 'non-

[83] Figure 1 is derived from Latour (1993: 11). Latour's original figure illustrates how 'nature' and 'culture' are separated by the first dichotomy. Since we are primarily interested here in relations between science and politics, I have inverted Latour's figure in the sense that the focus is shifted from what is represented (i.e. 'nature' and 'culture') to what represents ('science' and politics'). While I found it suitable to also 'invert' the labels appearing the lower part of the figure, the inverted networks, of course, remain without a clear geometrical shape.

scientific' elements.[84] As Gieryn mentioned, this includes Merton's imperatives. Other repertoires used in boundary work pertain to 'the work of translation'.[85] Here we can also turn to Gieryn's examples in the above citation; to make 'claims to the utility of science for advancing technology, winning wars, or deciding policy in an impartial way' (Gieryn 1983: 783). The effectiveness of the work of translation is here conditioned by the transgression or blurring of conventional, pre-defined boundaries, as they are enacted by the work of purification. To illustrate such work of translation in practice, I now turn to one of Latour's favourite cases, namely that of Pasteur's laboratory.

The laboratory as the lever that can move society

A laboratory, i.e., the place where the scientist works (Latour 1987: 64), comprises much more than the deployment of discursive positions such as the repertoires discussed above. Think, for instance, of microscopes (Hacking 1983), petri-dishes with specialized growth media (Latour 1988), seventeenth-century state-of-the-art air-pumps (Shapin and Schaffer 1985), or complex computer simulation models (e.g., Shackley and Wynne 1996). All these can be thought of as 'inscription devices' (Latour and Woolgar 1986) that allow 'nature' to speak up and be recorded as 'facts' in scientific texts. We can say that it is by way of the laboratory that:

> The equal world of citizens having opinions about things becomes an unequal world in which dissent or consent is not possible without a huge accumulation of resources which permits the collection of relevant inscriptions. What makes the difference between author and reader is not only the ability to utilize all the rhetorical resources studied in the last chapter, but also to gather the many devices, people and animals to produce a visual display in a text. (Latour 1987: 69-70).

[84] The case of the exclusion of J. Sudbø, who was caught having published a scientific paper based on faked data, represents a recent Norwegian example of the latter. In Denmark, the question whether Bjørn Lomborg's book, *The Sceptical Environmentalist* (Lomborg 2001), should be regarded (and hence be criticized) as science or not offers a much more intriguing example of the difficulties of delimiting science, since this was much less clear. The dedicated committee that was appointed the task of evaluating this case worked for a long time before arriving at rather vague conclusions.

[85] Not all forms of boundary work are deployed to the purification of science in order to safeguard its knowledge authority. For instance, Gieryn talks about 'expansion', which is when 'insiders seek to push out the frontiers of their cultural authority into spaces occupied by others' (Gieryn 2003: 429).

Now, contrast this with the prescription for STS with which Gieryn ends his chapter on science's boundaries:

> Get constructivism out of the lab to release its interpretative potency where the referent is not nature but culture. If science studies has now convinced everybody that scientific facts are only contingently credible and only as good as their local performance, the task remains to demonstrate the similarly constructed character of the cultural categories that people in society use to evaluate those facts and claims. […] Getting constructivism out of the lab moves science […] closer to places where matters of power, control, and authority are settled. (Gieryn 2003).

These quotes illustrate both the main similarities and differences between Gieryn's position and Actor-Network Theory. ANT[86] agrees that fact production is a local enterprise that involves local negotiations. This is what ANT found when it ventured into the laboratory to study science in action (Latour 1987; 1988; Latour and Woolgar 1986). But in terms of ANT, those negotiations are not confined to a 'social' world. Instead, ANT views the categories of 'nature' and 'culture' as *outcomes* of semi-stabilized networks (Latour 1987; 1993). While ANT agrees with Gieryn that social categories are constructed, and moreover, that they cannot be used as explanatory resources but must themselves be explained (Latour 2005), ANT does not agree that matters of power are principally settled in 'the social'. Just as fact production is only possible through the careful construction of inscription devices in the laboratory (Latour and Woolgar 1986), argues ANT, sustained differences in powers can only be adequately explained by taking *materiality* into account.[87] ANT is about constructivism, not just a social constructivism (Latour 2002; 2005).

[86] ANT is obviously not a single, homogenous theory, but perhaps rather a set of related methodologies that are based on a *relational* heuristic as opposed to a 'substance' heuristic. Latour's (2005) *Reassembling The Social* is just as much a rather imperialistic, although interesting, attempt at reassembling the ANTs that are swarming out from the ANT-heap of the Ecole des Mines in Paris! I tend to stick to this rendering of ANT.
[87] Latour (2005) terms the sociology that neglects the role of materiality 'the sociology of the social', which he opposes to his own 'sociology of associations': 'It's the power exerted through entities that don't sleep and associations that don't break down that allow power to last longer and expand further – and, to achieve such a feat, many more materials than social compacts have to be devised' (Latour 2005: 70). In their usual mocking style, this makes Latour and Callon declare that the 'sociology of the social' is good for studying society of *baboons* in which power structures derive from face to face interactions. When studying humans, in contrast, one must take account of how they build powerful macro-actors by inscribing relationships into durable materials (Callon and Latour 1981). As Latour puts it elsewhere, 'technology is society made durable' (Latour 1991). After all, Bentham's panopticon would neither disciple nor punish (Foucault

The reason why we should not follow Gieryn advice and 'get STS out of the lab' is that the laboratory is the lever that can move society (Latour 1983). Let us take a look at what this means. From his study of Pasteur's science, Latour argues:

> Pasteur, representing the microbes and displacing everyone else, is making politics, but by other, unpredictable means that force everyone out, including the traditional political forces. We can now understand why it is so important to stick to laboratory microstudies. In our modern societies most of the really fresh sources of power comes from sciences (Latour 1983: 168).

Hence, we venture into the laboratory, in order to study 'the fresh sources of power' in society. Since we are interested in the science-politics boundary, we obviously have to return to the claim that 'science is politics by other means'. First, however, we need to get a better grip on Latour's understanding of a laboratory.

As will be recalled, Latour defined the laboratory as the place where the scientist works. It is important that we do not imagine this as a spatially confined location. If the laboratory was hermetically sealed off from society, it would not be a fresh source of power. Pasteur's success is not only a consequence of him being a brilliant and pioneering microbiologist in his laboratory; he was also a master of drawing attention to, and creating an interest in, the work that is performed in the laboratory. By switching readily between scientific, commercial and political repertoires, he 'enrols' not only scientists, but also farmers, veterinarians, hygienists, administrators and so on, convincing them to support him by letting them understand that his laboratory holds the key to their problems. While Latour (1983: 143) notes that '[t]he mere existence of this enormous interest shows the irrelevance of too sharp a distinction between the "inside" and the "outside" of Pasteur's lab', there are more reasons to give up such a distinction.

1977a) without being made in bricks or cement. It is important to note how technology or materiality in this heuristic does not, or not only, reinforce and solidify preexisting 'social' relations. Materiality plays it back to humans by having normative consequences and by making new agencies available (Latour 1992). Just as humans attempt to structure the material world, things frame human actions.

Here I take the opportunity to comment on ANTs gimmick of lending non-humans, including things, agency of the humanoid type. True, to claim that 'objects too have agency' (Latour 2005: 63) is fun. But although it has been very effective in putting off non-ANT exponents, I do not support it since I take it to imply little more than a symmetrical repositioning of an agency type that remains anthropomorphic. If we have to be anthropomorphic either way, I prefer the choice that causes less confusion. Instead I find it more helpful to analyse how human (or for that matter, animal) agencies respectively are constrained and enabled through their connection with things – a type of analysis to which ANT is so well suited. It follows that ANT is not committed to this gimmick in order to be methodologically inventive and inspiring.

Pasteur not only succeeds though his mastery of the microbes in the rather confined space we commonly refer to as ‘laboratory’, but just as much through the successful *extension* of his laboratory practices into the field. Yet, ‘the field’ is never indefinite; it is a carefully staged site in which the Pasteurians remain in control; in which their experiments work.[88] It is in this sense that, as mentioned above, Latour argues that science has no outside.

4.2.2 The co-production of science and society

When Latour (1988) entitled his book, *The pasteurization of France*, the point is literally that French society not only has been moved by a laboratory but indeed it has been turned into one; it has become a place in which the Pasteurian laboratory practices are performed. In other words we can say that science and society are co-produced. In general, Sheila Jasanoff proposed that ‘co-production’

> is shorthand for the proposition that the ways in which we know and represent the world (both nature and society) are inseparable from the ways we choose to live in it. Knowledge and its material embodiments are at once products of social work and constitutive of forms of social life; society cannot function without knowledge any more than knowledge can exist without appropriate social supports. […] Scientific knowledge […] both embeds and is embedded in social practices, identities, norms conventions, discourses, instruments and institutions – in short, in all the building blocks of what we term social. The same can be said even more forcefully about technology. (Jasanoff 2006b: 2-3).

Equivalently, we can talk about a ‘co-evolution’ of science and society (Gibbons 1999; Nowotny et al. 2001).

It is useful to contrast the notion of co-production with, for instance, Polanyai’s (1962) ‘Republic of Science’. In Polanyai’s view, science should be seen as a self-organizing knowledge producing organization. Guided only by the notorious ‘invisible

[88] ‘[T]he vaccine works at Poilly le Fort and then in other places only if in all these places the same laboratory conditions are extended there beforehand. Scientific facts are like trains, they do not work off their rails. You can extend the rails and connect them, but you cannot drive a locomotive through a field.’ (Latour 1983: 155).

hand', science is and should be *autonomous* – indeed a republic in society – authorized by what Latour termed the Modern Constitution, or what Gibbons (1999: C84) sees as the yet prevailing social contract between science and society by which science is 'left to make discoveries and then make them available to society'.[89] Similarly, Popper, Lakatos, Kuhn and Merton each portrayed science as, or proposed it should be, a rather autonomous entity. In general, we can consider this autonomy as a characteristic property of essentialist conceptions of the demarcation of science that Gieryn opposed with his ideas on boundary work. However, while Gieryn's notion of boundary work as mentioned is constrained to the discursive domain, this is not so regarding the notion of co-production:

> [W]ork in the co-productionist idiom stresses the constant intertwining of the cognitive, the material, the social and the normative. Co-production is not about ideas alone. It is also about concrete physical things. (Jasanoff 2006b: 6).

The notion of co-production hence complies with Stengers' (2000) and Latour's (1987: 69-70) understanding of how the laboratory is a source of authority, a device that allows for the separation of knowledge and opinion, *in combination with* the forms of boundary work, i.e., the deployment of cultural repertoires that Gieryn talks about. These are the reasons why the notion of co-production comprises the foremost theoretical stance in my work.

Below I propose a modification of Latour's scheme of translation and purification (refer back to Figure 1 above) which is consistent with Latour's (2005) recent rendering of ANT, and which allows for reflexivity in the sense that it takes the interpreter's role into account.

[89] Gibbons (1999: C84) suggests replacing this contract with a new one that is 'based upon the joint production of knowledge by society and science'.

4.2.3 Purification as a form of translation: from panorama to oligopticon

Although it has been illustrative, we should not take Latour's (1993) grand narrative, let's call it *never-modernity*, as the final picture. It is what Latour (2005) terms a 'panorama', at best it provides 'a prophetic overview of the collective, at worst it is a poor substitute for it' (Latour 2005: 190). This is why Latour (2005: 183-190) advises the ANT scholar to shift from the hypostasis of panoramas to the exploration of *oligopticons*.[90] In fact the very idea of grand narratives seems to be in conflict with central ANT tenets. A commitment to such an overall narrative would place the sociology in question at risk of being more *vampirical* than *empirical* (Latour 2005: 50). This is why ANT advises us to 'follow the actors themselves'(Latour 2005: 12) and to study how they 'deploy their own worlds'(Latour 2005: 23). Since the latter is what Latour so eminently has shown us how to do time and again in his case studies, this critique of Latour (1993), although not intended to be *ad hominem,* seems rather unfair. But I hope it will serve to illustrate how the hypostatization of dichotomies violates the ANT's methodological advice of taking a *flat* projection of the social (i.e., the collective) as the observer's default cartographic norm (Latour 2005: 190), leaving it to the actors to introduce different topologies.[91] Hence, while Latour's (1993) talk of absolute dichotomies has been illustrative, it takes the point a bit too far.[92] My suggestion is simply to eliminate these dichotomies since they tend to obscure how the work of purification is just one among other forms of translation[93] (see Figure 2).

[90] An 'oligopticon' is a place in which 'narrow views of the (connected) whole are made possible' (Latour 2005: 181). The notion of 'center of calculation' (see section 6.2 below) refers to a *numerical* oligopticon.

[91] 'I hope it is clear that this flattening of the landscape does not mean that the world of the actors themselves has been flattened out […]. The metaphor of a flatland was simply a way for the ANT observers to clearly distinguish their job from the labor of those they follow around. If the analyst takes upon herself to decide in advance and a priori the scale in which all the actors are embedded, then most of the work they have to do to *establish* connections will simply vanish from view. It is only by making flatness the default position of the observer that the activity necessary to generate some difference in size can be detected and registered' (Latour 2005: 220).

[92] Moreover, it is simply too confusing for the interpreter of science (at least for the one writing here) to shift between the realms of purification and translation, and to keep track of in which arguments we are 'modern', and in which we are not - or both.

[93] This is an idea that Petter Holm and I hinted at in the project-description (Holm and Nielsen 2003) that led to the funding of this work. Unfortunately we never found the opportunity to develop the idea further as I try to do here.

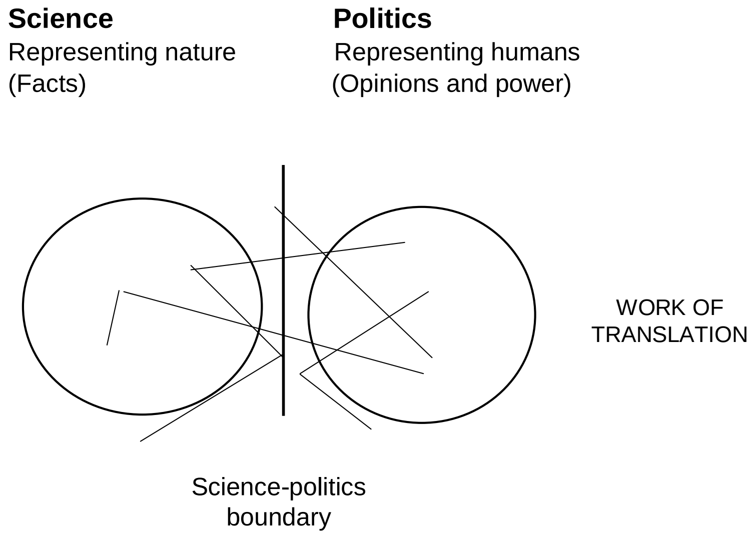

Figure 2. Co-production of science and politics.
An interpreter's map of 'science' and 'politics' as resulting from local 'co-production' processes in which boundaries are projected and transgressed, all of which are seen as translations. See text for explanation.

Instead of the panorama of science and politics of Figure 1, Figure 2 offers an 'oligoptic' illustration of 'science' and 'politics' as locally negotiated outcomes, as seen by a situated interpreter. Instead of discussing relations between science and politics in a grand narrative, we can study them as concrete instances of co-production if we find a suitable field location, and are able to scribble down a suitable map (or text) from all the diverging information we are presented with.

In line with ANT's principle of generalized symmetry, the interpreter should avoid pre-conceived concepts of the identities of the actors involved in the construction and stabilization of a network (Callon 1986). What science is, its role and responsibilities, how it interacts with politics, and so on, should not be decided by the analyst in advance but rather be seen as parts of the outcome of translation processes. Labeling some of these translation processes as 'purification' appears to be in conflict with this principle of symmetry. Instead of a invoking a dichotomy in advance (Figure 1), ANT provides a methodology suitable for studying ‘science’ and ‘politics’ as local translation processes of all kinds (Figure 2).

As I hope it follows from the above discussion, the model depicted in Figure 2 has, as Lakatos would have put it, more empirical content than Gieryn’s notion of boundary work because it takes the laboratory (the place where scientists work) into account. A study of local boundary constructions should pay attention to what goes on in the (extended) laboratory as well as how ‘cultural repertoires’ are deployed inside and outside this laboratory. Here, the terms ‘inside’ and ‘outside’ are not *a priori* categories of the interpreter, but are likely to identify, and contribute to, boundaries as they are projected by actors. Moreover, this flat model is more parsimonious than Latour’s (1993) since it does not introduce *a priori* differentiations between translation processes. Finally, Figure 2 takes the interpreter’s role (and careful work) directly into account, in contrast to the somewhat anonymous author lurking somewhere behind Figure 1; Figure 2 is her/his interpretation of the actor-network, illustrated by her/his map (or text).

4.2.4 Performing boundaries: critique and politics of boundary studies

The role of the interpreter brings me to the issues of performativity of theories and reflexivity. The notion of performativity claims that theories (and knowledges in a broad sense) are ‘performative’ (Callon 2007); they not only describe but also participate in the transformation of realities and practices (paper 6). Few would disagree with the contention that some interpretations of science, Popper’s for instance, have become part of constituting what science is and what people inside and outside it think it should be. Apart from what Popper and other interpreters discussed here have taught us about the demarcation of science, it has also been useful for us to study them because their interpretations contribute to the repertoires that are available for boundary construction today. Successful role models, made available by those who study and reflect on science, hence, are played back into science where they are imitated or rejected, and in either case they leave an effect. In general, interpretations of science and its role – sometimes in the descriptive mode, sometimes explicitly normative – have throughout history contributed to the shaping of science's boundaries. [94] The same, of course, goes for the shaping of 'politics' and 'society'. Note that I do not say that, for instance, Popper's theories of scientific discovery, or the discourse on 'democratization of science', are solely armchair inventions. They describe something that is present in the world to the extent that they are descriptive, and they are based on lines of reasoning to the extent that they are normative. What I am saying is that a concept of 'scientist', if it gains recognition, will interact with what it refers to. Since scientists generally are highly reflective people, working in an environment subjected to strong norms, we can expect this 'looping effect' (Hacking 1999) to be particularly potent as regards science.

Let me offer a crude illustration of how interpretations of science have made new repertoires available that not only are relevant to understand what science is, but also have a role in shaping it. A scientist today can aim at being a good Popperian scientist, based on her mandatory courses in the philosophy of science. We can also imagine that she has been interested in recent STS discourses on science which makes her work hard to achieve a 'democratization' of mode-2 science (Gibbons et al. 1994). I hence propose that both 'falsification' and the 'democratization' discourse represent examples of 'cultural repertoires' of science (Gieryn 1983: 2003), deployed as contributions to the construction, maintenance or transformation of science's boundaries.

In fact, we can imagine that our scientist aims at *both* being a good Popperian scientist *and* at contributing to the democratization of science, depending on the relevant context. The empirical possibility of the above 'both' is explicitly available to ANT since ANT is more about *addition* than about *distinction*.[95] Instead of defining science, or an event within science, as either Popperian or 'mode-2 science' (to use the above examples), this would for ANT represent an empirical question. It is, however, a question of which the exploration is particularly challenging since 'mode 2' and 'Popperian'

[94] This point is, as will become apparent below, reflexive such that it includes my own, it must be said, rather minor contribution. Jasanoff (1996) offers interesting examples of how she (as an SSK interpreter of science) became directly involved in disputes on the boundaries of science. The reader may refer to Lynch and Cole (2005) for a highly interesting and reflective paper on the boundaries and scientific status of STS – according to the courts.

[95] See, for instance, Latour (1996; 2005: 228; 1991). Concerning this point, the reader can also turn to Brian Massumi's (2003) discussion on the difference between 'correspondence' and 'nomad thought' in his foreword to *A Thousand Plateaus* (Deleuze and Guattari 1987). The 'rhizome' and 'nomad thought' of Deleuze and Guattari (1987) bear obvious and important parallels to ANT.

cannot be treated as stable entities, but must be analysed as subjects of specific translations. The model I have proposed above (illustrated by Figure 2) allows for the deployment of various available repertoires in different contexts.

Let us reflect: What about ourselves, the analysts? Gieryn's (2003: 394) voice is surprisingly firm: 'Essentialists *do* boundary-work: constructivists *watch* it get done by people in society'. While Popper and Merton and others thought they were objective, we know better. They were normative; they were recklessly supplying scientists with new effective repertoires by which they could fortify their boundaries and sustain their cultural authority. We, the constructivists, in turn, are empirical; we merely observe how boundaries are constructed.

As I have indicated[96], I do not agree with the second half of Gieryn's proposition. Constructivists don't just sit back with a (disinterested) empirical gaze and watch boundaries being constructed; the constructivist interpreter too contributes to the collective boundary work. Just as scientists perform boundary work by interpreting and deploying available cultural repertoires, so do interpreters of science because their descriptions – if effectively circulated – translate and modify these repertoires.

This invites some reflective remarks on the business of ANT. First, the relationism or perspectivism of ANT (Latour 2005) should not be confounded with deprived species of relativism that we can recapitulate with the phrase that 'any story is as good as any other'.[97] Latour succinctly explains ANT's view on the relation between the author and the described/performed as follows:

> A good account will *perform* the social in the precise sense that some of the participants in the action – through the controversial agency of the author – will be *assembled* in such a way that they are *collected* together. (Latour 2005: 138).

This implies that not just any text can contribute to the performation of boundaries. A text must be convincing in order to *move* its readers, and it becomes convincing through a combination of reasoning and careful empirical observations – and, of course, the text may fail in this respect. As Latour puts it, 'an account which accepts to be "just a story"

[96] See also footnote 94 above.
[97] Latour (2005: 95) cites Deleuze that: 'Relativism is not the relativity of truth but the truth of relation'.

[…] does not fret any longer being accurate, faithful, interesting, or objective'. This implies that 'textual accounts can fail like experiments often do' (Latour 2005: 127).[98]

The notion of performation of statements breaks with the divide between logics and rhetoric that has been traditional since the ancient Greeks (Callon 2007) – which brings us back to Plato and to how he opposed the 'sophists'. While, in Callon's words: '[t]he ontology of the world of logic is set and independent of the discourses describing it', rhetoric 'implies relationships of entanglement between propositions and their referents; it acts on the ontology of the entities to which it refers' (Callon 2007: 316). By the notion of performation, a statement implies a 'context' in which it works. The truth of the statement hence depends on, so to speak, its ability to rearrange its surroundings; its truth becomes a matter of its success (Callon 2007).

This is a suitable place to meet a standard anti-ANT reaction, however annoying and badly informed it is: 'If truth depends on success, then ANT would accept something to be true if just enough believe it; the Holocaust is fair if enough find it to be fair'. No. ANT represents on approach for *studying* the *processes* by which a statement obtains the status of being held to be true (or fair) or not so. By the *methodological* principle of (generalized) symmetry, ANT seeks to abstain from making judgments about what is true (or fair). Whether this methodology is fully achievable or not is something to which I will return. But to get back to the Holocaust example: ANT would be one among other methodologies that could be deployed in the attempt to *examine* claims – for instance those considered 'obvious' – and hence reopen them for epistemological and/or moral reflection. The 'obvious' may or may not be different from the analyst's personal moral beliefs.

The next but related question relates to the possibility of critique: Can a text be an accurate sociological description and normative at the same time? I have, as will be recalled, addressed this question above. Popper carefully stated that his methodology was not 'naturalistic'; his project was explicitly methodological, critical and normative. If scientific practices do not fit into Popper's picture, then so much the worse for science! As Gieryn's claim that constructivists watch others perform boundary work suggests, and as some quite empiricist renderings of ANT (e.g., the advice for an empirical rather than

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[98] This is the fifth uncertainty, as presented in footnote 81.

a vampirical sociology) indicate, at least some constructivists take a rather naturalistic position.

This brings us to another standard critique of ANT, namely that it is not critical enough, and hence betrays what should be the true sociologist's call: to defend the weak and abused against those who are in power (e.g., Mirowski and Nik-Khah 2006). This ties in with the problem of how to be critical without having a firm normative or epistemological fundament from which a critical edge can be developed. Since such a fundament would, in Latour's terminology, be associated with a risk of being vampirical, this could suggest that an interpreter is left with the following choice: either you are critical or you are empirical!

Let me explain why I disagree with such a polarization. It is possible to be critical without committing to a heavy normative/theoretical fundament. This is why Latour (2005) talks about 'critical proximity' instead of a 'critical distance'. Yet, this form of critique is different from, say, a fundamentalist's critique; it is closely related to Foucault's notion of critique that I have already introduced:

> A critique is not a matter of saying that things are not right as they are. It is a matter of pointing out on what kinds of assumptions, what kinds of familiar, unchallenged, unconsidered modes of thought the practices that we accept rest. (Michel Foucault 1981, cited in: Rabinow and Rose 2003).

My position, as it has been outlined, should not be taken to imply that I think of the notion of (generalized) symmetry as a methodological miracle that, if only correctly applied, will guarantee the researcher's epistemic or political neutrality in a controversy. Scott, Richards and Martin (1990) describe how each of them struggled to maintain such a neutrality when studying a scientific controversy, and how they were inevitably being enrolled by one or another side. Collins (1990) replied that symmetry as a methodological principle should not be confounded by asymmetrical consequences that arise from the type of analysis in which it is used. A main reason why this answer does not satisfy Scott, Richards and Martin (1991) is interesting because it brings us back to the relation between the observer and the observed in Figure 2. The problem here is that the researcher cannot conduct a 'symmetrical' study of each side in the controversy because the researcher's access to information depends on whether s/he is seen as a potential threat or an ally. The unequal access to information will in turn affect the researcher's perception of the controversy.

I think Scott and colleagues have a point here. Since there is no (neutral) view from nowhere, the interpreter can only draw a map by linking to, and interacting, with the networks in question. Since enrolment, to some extent, is a precondition to gain access to information, an interpretation can never imply absolute epistemic or political neutrality.[99] Therefore, I propose that it is better to think of (generalized) symmetry as dedication to reflection on the observer's role, as well as providing help to do so.[100]

The rejection of the possibility of political neutrality led Scott et al. (1990) to advise that the analyst should be critically involved. Given Foucault's notion of a critique, however, I find that this does not necessarily force the analysts to explicate alliance with one part of a controversy. I find it appropriate to let Jasanoff summarize much of the discussion in this section:

> By adopting a relativizing pose with respect to particular claims of scientific knowledge, science studies does not abandon the commitment to be explanatory and normative; instead, it adds to the repertoire of possible explanations, and illuminates new pathways for intervening in the production of both knowledge and power. (Jasanoff 1996: 412).

Above I discussed the contrast between autonomous conceptions of science and the notion of co-production of science and society. I have discussed perspectives of this contrast before, namely under the label of the 'science wars', and as a part of what I referred to as 'Plato's legacy and its modern challenges'. I find it appropriate to conclude my (main) theoretical chapter with a reflection on the 'science wars' and boundary studies.

As pointed out by Stengers in her book, *The Invention of Modern Science* (2000), scientists have been quite tolerant, if not indifferent, to the various accounts given by

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[99] 'The methodological claim of neutral social analysis is a myth that can be no more sustained in actual practice than can the scientist's belief in a universal and efficacious scientific method' (Scott et al. 1990: 491)

[100] This pragmatic interpretation of symmetry was offered to me in a lecture by Steve Woolgar at the PhD course "Managing Science in Society", which was held at Copenhagen Business School in Denmark in September 2006.

their interpreters of the science/non-science distinction. Kuhn's interpretation of science is by and large tolerated within science because it shares Polanyi's view on the autonomy of science, defining science as distinct social activities characterized by shared paradigms. 'Putting the distinction in question, by contrast, is not the matter of interpretation but the subject of conflict' (Stengers 2000: 63). This elucidates how the 'science war', as is the case with so many wars, is about autonomy, territoriality and interests. It is, therefore, as Stengers (2000: 63) points outs, in itself about politics:

> What is at stake in every question concerning the autonomy of the sciences is the distinction between those who have the right to intervene in the scientific debates […] and those who do not have this right. The opposition of scientists to any sociology of the sciences can then be understood in political terms.

Science studies, therefore, would not be fully reflective before recognizing that it, too, 'is politics by other means' (Whelan 2001).

5. Emerging dimensions of the science-politics divide

From a discussion of the various positions on science's boundaries that have constituted my theoretical obligatory passage points, I have developed a theoretical framework for studying the boundaries between science and politics, namely the co-production model, as illustrated in Figure 2 above. This model considers the science-politics boundary to be a product of translations that are performed in laboratories and in discourses and that are related to these laboratories. It is a constructivist approach that shifts the emphasis from 'the division of labour' to the 'labour of division'. In this chapter I will set out the reasoning of this model further from a somewhat more empirical perspective, namely by introducing two emerging dimensions of science in society, each of which seems to pose a challenge the conceptions of autonomous science. Simultaneously, these developments will motivate the type of empirical research conducted in this case study.

5.1 From science to research

The model of an autonomous science, to which Plato, Galileo, Boyle, and some modern interpreters of science have contributed, is challenged. While for the time being we can leave the question of whether this model characterized the dominant forms of scientific knowledge production in the past open, it at least no longer seems to do so. This is what Latour (1998) addresses by what he terms a change from a 'world of science to a world of research' [101]. He briefly characterizes this transition as follows:

> Science is certainty; research is uncertainty. Science is supposed to be cold, straight and detached; research is warm, involving and risky. Science puts an end to the vagaries of human disputes; research creates controversy. Science produces objectivity by escaping as much as possible the shackles of ideology, passions and emotions; research feeds on all of those to render objects of enquiry familiar.

Much in parallel with Latour's picture, Michael Gibbons, Helga Nowotny and their colleagues (Gibbons et al. 1994; Nowotny et al. 2001) talk about a shift from Mode-1 to Mode-2 science.[102] Mode-1 science is the autonomous pursuit of knowledge, as captured

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[101] I apologize for the terminological awkwardness that is set off by this change from 'science' to 'research'. I considered whether I should replace 'science' with 'research' in my title as well as in the text in general. In other words: a consistent terminological switch from 'science' to 'research' would have been one option. When I chose not to do this, it is because too many terminological constructions, phrases connotations, images etc. are tied in with 'science' – for example as in 'mandated science'. Therefore I have aimed at using 'science' as an empirical term, while – when necessary – using 'research' in Latour's (1998) sense as an analytical term.

[102] I now prefer Latour's concept, although it is less developed, and probably less referred to. The first and simplest reason for this is that research and science have a much better footing in common language and common understanding of scientific/research activities. I suggest that people will more easily get the idea of transition when we use the terminological pair 'science/research' instead of 'mode-1/mode-2'. The word 'science' is in its common usage a carrier of relevant connotations e.g., 'epistemic authority', images of scientific heroes like Newton, Pasteur and Einstein etc. 'Research' in turn is a term frequently used in our daily newspapers, extending from research on the Earth's fate in climate models and down to my choice of toothbrush. Science is venerable textbook material. Research is when what was the big news from the laboratories in yesterday's newspaper is challenged on tomorrow's front page. In contrast, I suspect that my neighbour will quickly lose track of mode-1/mode-2 talk. The concepts of science/research simply tick better.

The second reason why I prefer science/research to the 'mode talk' is that the latter seems to imply a chronological order in which 'mode 2' follows and replaces 'mode 1'. Was there ever a 'mode 1'? To some extent, the mode talk seems to accept this – at least I think it is fair to say that 'Re-Thinking Science' is a bit vague on this point. But isn't Latour's science/research concept subjected to this problem as well? Does not the transition to a concept of 'research' in which knowledge production is uncertain and entangled in the society it forms part of presupposing a concept of 'science' in which this was not the case? At the outset, Latour's concepts could seem to be vulnerable to this argument. But they are not. It is probably by Michael Polanyi's image of 'the Republic of Science' (Polanyi 1962). In Mode 2 (research), scientists, commercial interests, government representatives and clients/customers are in tight and continuous interaction. While Mode 1 (science) may characterize basic research in some university departments, much knowledge production nowadays is carried out as Mode 2 (research) in corporate laboratories, as 'mandated science' produced as an input to political decision-making (Jasanoff 1990), participatory research schemes (Callon 1999) , or in other forms of 'hybrid fora' (Rabeharisoa and Callon 2002).

The science-politics divide is different within these two images of science. While political intrusion is corrupting for Mode 1 (science), this is not necessarily so in Mode 2 (research). The two images are, however, not fully comparable in all dimensions. While both may be descriptive/analytical ideal models, Mode 1 (science) much more than Mode 2 (research) represents a widely distributed and institutionalized *normative* model – such as it is portrayed in Merton's 'Ethos of Science' (1996). Plato and Popper focusing on epistemology, and Merton and Polanyi focusing on social structures, have all contributed to the development of Mode 1 (science) as a cultural repertoire that informs people of what science is and should be. In contrast, Mode 2 (research) has not developed recognizable and widely distributed normative repertoires – at least not yet.

While 'the Ethos of Research', with a few notable exceptions[103], is not yet written, and hence, is pretty much up for grabs, this does not mean that there are no normative views on the new roles and forms of knowledge production in society. In fact, I tried to identify the contours of some of these normative trends by posing them as challenges to what I termed 'Plato's legacy'. They include discourses such as those of 'democratization of science', 'public understanding of science', 'public engagement with

difficult to find an interpreter of science who is more aware of the close interactions, or indeed, co-evolution of science and society, than Latour. This is exactly why he, as discussed above, stated that 'we have never been modern' (Latour 1993). In Latour's (1998) interpretation, science and society were entangled; research and society are just *more* entangled. This being said, I reveal upfront that occasionally below I will use 'mode-1 vs. mode-2' in situations where this is terminologically (or rhetorically) convenient (call me a terminological opportunist) – for instance because the term 'mode-2 society' to my knowledge is not matched by a 'research-society' notion.

[103] The (Norwegian) National Committee for Research Ethics in Science and Technology (NENT) very recently published a set of guidelines on research ethics (NENT 2007) that actually impose on the researcher quite demanding obligations and responsibilities (http://www.etikkom.no/English/Publications/NENTguidelines – visited 12.10.08).

science', 'transparency', 'accountability' and so forth. Further, the texts of Latour and Gibbons et al. are not only describing new forms of knowledge production, they also have quite strong normative implications.[104] Hence, Latour and Gibbons, Nowotny et al. all contribute to the development of norms of research in much the same ways as Plato or Popper contributed to the norms of science.

As I suggested above, these emerging norms do not necessarily preclude normative repertoires of the Mode 1(science) model. In contrast, these may still be used for legitimating purposes. They may continue to be invoked in the process of cleaning up and rationalizing scientific truth production as it is presented for external audiences, and to be used for presenting a sanitized version of ready-made science in processes of purification (Latour 1993). This, however, may provide some tensions in so far as the Mode 1 (science) model is called upon in order to establish and police the boundaries between science and other sectors, while Mode 2 (research) model focuses on the ways such boundaries are penetrated and dissolved.

5.2 Post-normal science

In Beck's 'risk society', the unforeseen consequences of techno-science and the associated public unease with science are portrayed as defining features of our time (Beck 1992). Slightly paradoxically, hence, the mirror image of the 'knowledge society' is a society that must come to grips with scientific uncertainty (Wynne et al. 2007). Funtowics and Ravetz (1993) introduced the notion 'post-normal' to characterize situations in which 'facts are uncertain, values in dispute, stakes high and decisions urgent'. These situation are 'post-normal' since they transcend the puzzle solving within Kuhn's (1970) 'normal science'. For instance, such post-normality is a feature of a growing number of areas of environmental science, since knowledge about complex and semi-open systems such as our environment(s) can only be limited. Since post-normality,

[104] Some of Latour's titles clearly illustrate this point: 'Politics of Nature: How to bring the Sciences into Democracy?' (Latour 2004a), and 'A Politics freed from Science' (Latour 1999: Chpt. 8). Both 'The new production of knowledge' (Gibbons et al. 1994) and 'Re-Thinking Science' (Nowotny et al. 2001) represent ambitions not only to describe the new modus of knowledge production, but also to encourage its further transformation (see in particular the chapter, 'Re-Thinking Science is not Science Re-Thought' in Nowotny et al. 2001).

identifies normative and epistemological issues as intrinsically entangled[105], it poses a challenge to the conceptions of a clear, distinct and inert science-politics boundary.

In the international environmental discourse, precaution can be seen as a response to the acknowledgments of the limitations of scientific knowledge. Brian Wynne (1992b) identified the precautionary principle as an element in a broader directional change, the preventive paradigm, which ‘implies acceptance of the inherent limitations of the anticipatory knowledge on which decisions about environmental discharges are based’. He argued that the precautionary approach ‘involves much more than shifting the threshold of proof to a different place in the same body of knowledge’, and noted that: ‘The different social premises which that shift implies also open up the possible reshaping of the natural categories and classifications on which that scientific knowledge is constructed’(Wynne 1192b: 112).

The opening of scientific knowledge production relates to how Funtowics and Ravetz (1992; 1993) propose to come to grips with post-normal issues, namely through an extension of science’s peer communities. This approach thus converges with a general development of the relation between science and the public, characterized by a shift from the model of ‘enlightenment governance’ to models of public dialogues and engagement with science (Callon 1999; Elam and Bertilsson 2003). Here, the extension of science’s peer communities is not only proposed to increase the legitimacy through increasing the transparency of scientific knowledge by opening up science to a broader audience, but is also thought to enhance the saliency and quality of the knowledge production since ‘[k]nowledge of local conditions may help determine which data are strong and relevant, and can also help to define the policy problems’ (Funtowicz and Ravetz 1993).

In general, ideas of stimulating public engagement with science, and extending science’s peer community, are progressing. Recently, a high profile expert group on science and governance was convened by the European Commission Directorate-General for Research. The mandates of this group were: to respond to the ‘widely-recognised problem of European public unease with science’; to ‘improve the involvement of democratic civil society in European science and governance’; and to ‘address urgent

[105] For related reasons, Latour (2004a; 2004b; 2005) prefers to talk about ‘matters of concern’ instead of ‘matters of facts’.

European policy changes that are often taken as strongly scientific in nature – including climate change, sustainability, environment and development' (Wynne et al. 2007: 10). A main conclusion of the extensive report of this working group was that the 'promotion of diverse civic "knowledge-abilities" would perhaps be the most effective single commitment in helping address legitimate public concerns about Europe as a democratic knowledge-society' (Wynne et al. 2007: 10).

5.3 Steps towards an empirical philosophy of science/research in society

Although I have presented them as two different developments, the issues of the transition from 'science to research' and 'post-normality' are related; they are both about uncertainty, and both address how science and society become increasingly entangled. It seems obvious that what has been discussed here as 'geographical' conceptions of science's boundaries are ill suited to address these developments.

When Nowotny and colleagues (2001: 47) talk about co-evolution of (mode-2) science and (mode-2) society it is because 'it has become increasingly difficult to establish a clear demarcation' between them, and because they are 'subject to the same, or similar, driving forces'. This, in turn 'opens up the intriguing possibility not only that science can speak to society […] but that society can answer back to science' (Nowotny et al. 2001: 48). This merges with Funtowics and Ravetz's proposal for dealing with post-normality, namely through extended peer communities of science. It also merges with Latour's invitation to ANT scholars, namely to help counteract 'a premature transformation of matters of concern into matters of facts' (Latour 2005: 261).

Hence, I propose that these developments call for new approaches for interpreting science/research and its co-development with society than those pertaining to the standard geographical conceptions of the science-society boundaries. As Latour noted, there is a philosophy of science, but there is no philosophy of research (Latour 1998). Similarly, while there is an epistemology of science, the epistemology of post-normal science – Funtowicz and Ravetz (1993) name it 'political epistemology' – is still in its early stages. I hereby end my theoretical discussion with the hope that it may contribute to the development of such a new philosophy. Since the aim of such a philosophy would be to take contemporary developments of the relationships between science/research and society into close account, I propose that Mol's (2003) notion of 'empirical philosophy'[106] could be its appropriate label. It is time for this introduction to take another empirical turn.

6. Boundary construction in ICES advice

When I left the case of fisheries science and politics at the end of section 2 above, it was in a somewhat bewildered state. The only thing that seemed clear was that I needed a theoretical language better suited for exploring boundary construction practices in this context. Apart from that, I have not yet addressed the critical task of this introduction, which is to find way to integrate my papers[107], and to discuss how together they have contributed to our understanding of boundary construction processes and to the ongoing discourses on fisheries management. As will be remembered, a particular critical aspect of this task is that the papers differ significantly in terms of language, interested audience and framing of problems. Hoping that the co-production framework that has been outlined above can help me in meeting these challenges, I now switch to STS language in order to present and discuss the papers jointly. Besides STS concepts, I will draw on metaphors developed in the papers in so far as they help to summarize complex issues.

6.1 Situated methodological reflections: between boundaries

According to the outlined co-production framework, a study of the relationships between science and politics is not fully relational until the interpreter too is included in it. Hence it is pertinent to introduce this character more directly into the story. No doubt to Robert Merton's (posthumous) dismay, this project has indeed been 'an adventure in polymathy' (Merton 1996: 267). I am (or at least I was) actually more a student of fisheries

[106] 'Philosophy used to approach knowledge in an *epistemological* way. It was interested in the preconditions for acquiring true knowledge. However, in the philosophical mode I engage in here, knowledge is not understood as a matter of reference, but as one of manipulation. The driving question no longer is "how to find the truth?" but how "are objects handled in practice?" With this shift, the philosophy of knowledge acquires an *ethnographic* interest in knowledge practices' (Mol 2003: 5).
[107] See list of papers on page 3.

management than of STS theory.[108] In other words, I have in fact personally been caught up in different boundaries; between STS theory and the empirical world of fisheries; between the theoretical and the applied; or, perhaps indeed, between science and politics. Could it be the case that this actually comprises an ideal position for interpreting such boundaries – of, can I say, *Mode-3* science?

While I find my experience with the field has generally been an advantage, it is important to draw attention to the repercussions it has for my position in relation to the theoretical framework I developed above. The first repercussion is that I cannot really pretend to be a naïve observer. This implies that the world of fisheries to some extent was already structured for me when I started on this research project. Turning this world into a 'flat' projection would be difficult if not impossible for me. As I suggested above, the image of the 'flat' projection and the principle of symmetry, however, may be more useful for the interpreter if they are not followed to the letter, but rather are seen as a offering her/him a permanent challenge and a guide to provide fresh interpretations of a study-object, and to enhance her/his ability to reflect on the interpreter's role in constructing this interpretation. The sceptical ideal of Latour's five uncertainties remains just that; a useful ideal. In the case at hand, for instance, I knew where I could expect to find 'science' and 'politics' in the first place and approximately also what to expect from them. This, in turn, provided me the ability to be surprised in cases of deviations from those expectations.

The advantages of my previous experience with the field include, for instance, that I had contacts within the ICES community, and that I am a somewhat competent speaker of, say, the language of assessment modellers, fisheries regulations, and so on. It would be an exaggeration, to say the least, to assert that I 'went native'. Nor did I aim at being an STS agent 'undercover'. Instead, I was trying to produce texts within the world of fisheries resource management, while at the same time I tried to make sense of what I learned in the language of STS. Conversely, I was drawing on STS when I tried to contribute to the literature within fisheries resource management.

[108] In my work on this project I have drawn upon my background in biology (Bachelor's degree), and International Fisheries Management (M.Sc.). I have studied a little philosophy in the past (two years) but STS, notably ANT, was almost completely new and exciting (although at times almost painfully foreign) to me when I started on this project in 2004.

As discussed above, the interpreter needs to link in with the networks that s/he intends to study or be left out in the cold with meagre research resources at hand. Knowing the language and being familiar with the key issues in the field is an obvious advantage when trying to connect to the networks in question. For instance, I first met Knut Korsbrekke, the co-author of paper 5, while following him in action as an assessment scientist in the ICES working group on demersal stocks in the North Sea[109], in which he, among other things, was in charge of a subgroup that worked on problems of ICES's Precautionary Approach (see below). I later met Kjellrun H. Hauge, the other co-author of paper 5, when we were both participants in the ICES Working Group of Fisheries Systems in 2004, in which she enrolled me in the task of writing a joint paper on ICES's Precautionary Approach. The following year I was following *her* in action when she was struggling to make sense of the North-East Arctic Haddock stock assessment, and when she was participating in a subgroup dealing with the Precautionary Approach. And so on. This illustrates how, as I have suggested above, the comprehension of a network implies some kind of a mutual involvement of observer and observed.

I stress that my examination of the science-politics boundary in question has not been symmetrical in the sense that I have spent much more effort in understanding the 'science side' than the 'politics side'. This skew towards the 'science side' is also reflected in my knowledge and use of theory and literature – both as regards my theoretical world of STS and my empirical world of fisheries. While this asymmetry is unfortunate, it represents one of the many necessary you-can't-do-everything-in-a-PhD constraints of my research.

However, I do not think this limitation *undermines* my work. My work consists of my collection of papers in addition to this introduction, in which I aim to connect and position them in a theoretical landscape. In *Science in Action* (Latour 1987), science is seen as a certain form of text production. The validity of a scientific text is here measured by a pragmatic criterion, namely its ability to defend itself against opponents.[110] By the same token, the validity of my work can be judged to be no less and no more than the

[109] The Working Group on the Assessment of Demersal Stocks in the North Sea and Skagerrak. This was in 2003 as a part of the Policy and Knowledge in Fisheries Management (PKFM) project (see paper 3).
[110] See also 'the author's confessions and challenges to sceptical readers' above (p 28-29).

ability of my texts to sustain critique. Of course, this criterion of validity comes in addition to other criteria of success (or failure), notably including that the combination of my texts should be *relevant* and *informative* to the overall problem I address. To be more *complete*, however, I would have to follow this study up with a study that has more emphasis on the 'politics' side. This does not mean, however, that I have *disregarded* the 'politics side', which I believe is evident from all six papers. Moreover, I find that the 'theoretical skew' discussed here is somewhat ameliorated by the fact that the notion of 'co-production' of science and politics has been pivotal to my interpretation of the science boundary. In fisheries, politics is never far away when you look at science – and *vice versa*.

A Leibnizian constraint

I here wish to introduce a methodological criterion that I find to be useful in relation to an 'empirical philosophy' as introduced above. This criterion is what Stengers terms a Leibnizian constraint, namely that:

> [P]hilosophy should not have as its ideal the 'reversal of established sentiments.'[…] If [Leibniz's] aim was to 'respect' established sentiments […] it was much as a mathematician 'respects' the constraints that give meaning and interest to his problem. And this constraint – not to clash with, not to reverse established sentiments – does not mean not to clash with anyone, to make everyone agree. (Stengers 2000: 14).

I interpret this as follows. Take, for instance, Feyerabend's assertion that science is a myth (see above). This is a good example of a 'reversal of established sentiments'. I am not saying that Feyerabend's argument is not interesting. I think it is, but I am saying that it could be in conflict with an aim of an 'empirical philosophy'. With a starting point in Wittgenstein's dictum that 'meaning is use', we could respond that Feyerabend's language use is inappropriate. From the perspective of an empirical philosophy, however, it appears worse that he is not worried about what 'the natives' think; how they think myth and science are different, and how they struggle to separate them.[111] Similarly, if

[111] The challenge Stengers poses for herself is '[t]o try to articulate what we understand by science and what we understand by politics, without clashing with "not all sentiments," but what I will call, following

Latour said that 'science *is* politics' he would be violating the Leibnizian constraint. Instead he says 'science is politics *by other means*' (Latour 1988: 229, emphasis added). The focus for an empirical philosophy would rather be on the work deployed to attain a separation of myth and science, that is, on the labour of division.

The notion of the 'TAC Machine' exemplifies a case in which I find that we satisfy this Leibnizian constraint. This model of the fisheries resource management system (see paper 2) was generally well received – not only in ICES, but also among members of the fisheries bureaucracy.[112] In general, those of our papers that have been published as contributions to the fisheries literature (papers 2, 3 and 5) satisfy a Leibnizian constraint, which as mentioned does not imply that there is full agreement on it. The Leibnizian constraint provides a 'reality check' of the interpreter's map in Figure 2 above when it is fed back to 'the natives'. It may provoke, it may cause dissent, or it may be enthusiastically commented upon. But if it is utterly *rejected* this would send the empirical philosopher straight back to the drawing board.

6.2 Co-production of science and politics by the TAC Machine device

In paper 1, the background and consequences of the institutionalization of modern fisheries resource management are in focus. We propose the notion of the 'TAC Machine' to summarize key aspects of the (cybernetic) fisheries system (paper 2). It is a 'machine' since it involves an automatization of resource management practices.[113] The TAC Machine is a system in which stock assessments, the policy process, the rule

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Leibnitz, the established sentiments, those that provide a point of reference, that cannot be threatened without leading to panicked rigidity, indignation, or misunderstanding' (Stengers 2000: 15).

[112] 'The TAC Machine' was generally well received at two WGFS meetings in which it was presented. After the WGFS meeting in 2004 in which it was launched (Holm and Nielsen 2004), we observed that the notion to some extent started to circulate within ICES and among scientific advisors on fisheries in EU. In some instances our informants used the notion in interviews (before the model was published). Moreover, the notion of the TAC Machine helped in structuring the contributions from different partners from different disciplines in the PKFM project, and in structuring the summarized findings of this project (paper 3).

[113] In the cybernetic tradition, Beer (1967: 25) defines 'machine' as 'a name for any purposive system'. In our case 'resource management' can be regarded the overall purpose of the TAC Machine, whether this is sanctioned in more or less explicated objectives of sustainability, economic efficiency or socio-economic stability or some combination of these (paper 2). See also how the 'machine' metaphor is invoked by a director of the National Marine Fisheries Service to characterize an interlinked process of assessment science and management in an US fisheries management context (Wilson and Degnbol 2002: 5-6). See also footnote 45.

production, their implementation, and the control of them, follow each other in tight and consecutive in order to produce ‘sustainability’ on a routine basis. As Degnbol (2003) suggests, modern fisheries management as such comes with a rational ideology by which science provides information to enable an efficient management of fisheries.

The VPA and the TAC – respectively the mechanisms for representing fish stocks and intervening in fisheries – are at the centre in our account of the institutionalization of modern fisheries management. These two instruments appeared and jointly proliferated on the stage of North Atlantic fisheries management during decade from 1965 to 1975, and they had focal roles in a transformation of this stage. While it may be a coincidence that the two instruments appeared about simultaneously in this context, we argue that their joint proliferation was not coincidental; they mutually promoted and supported each other. The VPA made possible the form of TAC management that soon was widely practised, namely the form committed to the production of more accurate catch forecasts. We propose a positive feedback mechanism to explain the rapid proliferation of TACs. This proliferation in turn created a demand for a development of databases, procedures of data standardization and VPA model tuning and so on, which we can summarize as the development of the ‘metrology’ (Latour 1987) required to produce stock assessments and catch forecasts.

The VPA models and the catch forecasts they sustain are ‘centers of calculation’ (Latour 1987) located in ICES assessment working groups. Together with the data-collecting infrastructure (e.g., national marine laboratories and joint research surveys) they depend on, these centres comprise what we could call ‘the laboratory of fish counting’.[114] To paraphrase Latour (1983), it is this laboratory that produces the ‘lever’ – the catch forecast, including the potential TACs it suggests – which can ‘move society’, namely move the fish and the fisheries (and the coastal communities), into ‘sustainability’. As we develop the concept, the strength of this lever is not an attribute of this laboratory alone; it is dependent on a range of conditions which we can summarize as the (cybernetic) ‘integrity’ of the TAC Machinery (see papers 2 and 4).

Through the catch forecast, the TAC comprises the link between representation and intervention, and we suggest that the institutions and technologies for intervening in

[114] In paper 4 this laboratory is referred to as the fish-counting ‘macroscope’.

the fisheries are developed around the TAC. This includes decision-making on resource/fishing levels, rule making, the development of monitoring and control systems and resource allocation. This does not imply that the TACs comprise all the regulations on the fishery. In addition to the TACs, there are regulations on mesh-size and other gear restrictions, closed areas and seasons, and sometimes even fishing days limits, all added on top of the TACs. Besides these short-term regulations, there are important long-term policies and legal frameworks relevant to the effectiveness of the fisheries system, for instance, the 'structural policy' of the fisheries sector. What we do propose, hence, is that the TACs, so to speak cybernetically, hold a key position in the fisheries system: the system is set up as if the TACs can regulate the stock levels in the short-term (on a year-to-year basis) and in the medium-term (e.g., on a five-year basis).

We can summarize this as the emergence of a 'Fisheries Leviathan' (paper 6) that has the TAC-VPA as a backbone. The effectiveness of the TACs in this respect is, among other things, conditioned on the fact that the TACs are supported by other regulatory forms, such as those mentioned. The underlying assumption of this cybernetic machinery can be expressed in terms of its favourite parameters as follows (given a constant 'M'): 'TAC' controls 'F', which controls 'SSB', which controls 'recruitment' and which provides 'sustainability'. [115]

It follows from the previous paragraphs that our story is a story of co-production of science and politics. We describe the process in which the institutions of science and politics in the modern regime of fisheries management are mutually fitted to each other, paying close attention, as ANT advises us to do, to the metrology that makes them work. We also pay attention to the role of 'cultural repertoires' of science in this process, for instance Mertonian norms, deployed in the attempt to clarify and stabilize the boundary between science and politics.

From the repeated discussions in ICNAF in the 1960s and 1970s we get the impression that the science-politics boundary seems difficult to stabilize within the combination of CPUE indexes and/or Beverton-Holt models and effort management

[115] 'M' stands for natural mortality (all other mortality than that caused by the fishery). 'F' denotes Fishing mortality. 'SSB' stands for Spawning Stock Biomass. As regards the last link it would be more accurate to say that keeping 'SSB' at or above Bpa implies an acceptably low risk of impaired recruitment (or in fact, as we point out in paper 5: an acceptable low risk of a risk of impaired recruitment!).

measures (e.g., days at sea limits). Not least when viewed against the background of lurking conflicts between the international fishing fleets in the ICNAF area at the time – a sort of cold war acted out in terms of the science and politics of trawlers instead of in terms of nuclear missiles – this problem appeared rather irresolvable. In turn, the VPA-TAC model seemed much more politically simple – perhaps because the VPA stipulates 'absolute' stock estimates in contrast to dodgy indexes that can be put together and calibrated in endless numbers of ways.[116] The price of simple politics here, however, was complex science, which in turn required investments in the development of 'the fish counting laboratory'.

While the separation of science and politics to ICNAF predominantly seems to have been perceived as a functional matter (i.e., related to the difficult challenge to secure a somewhat agreeable knowledge base for volatile international negotiations), the situation in the ICES context seems also to have involved sentiments on scientific norms.[117] A bit too crudely, we can say that ICES not only wanted to serve its clients but also aimed at complying with traditional scientific norms, and that such norms were 'inscribed' into the 'laboratory' associated with VPA based methodology. This inscription was both founded on ICES's previous scientific tradition and it became representative, or even constitutive, of ICES's model of producing scientific advice. As suggested by the scientists we quote in paper 1 (at page 37), the heavy advisory machinery mobilized to make TAC advice has become part of the 'ICES culture'.

The principal mandate of the TAC Machine is to produce 'sustainability'. We describe how this production of sustainability is conditioned on the production and maintenance of a range of boundaries: a boundary between the TAC, which is to be fished, and the part of the stock that is to be protected to maintain its reproductive capacity; a boundary between advisory science and decision-making that is sufficiently

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[116] If we take a closer look, of course, the VPA estimate is not 'absolute' but, with an ironic twist, indeed *virtual* (paper 4). The VPA too can be calibrated in endless ways (Darby and Flatman 1994).

[117] We observe interesting differences between ICNAF and ICES. ICES strives to maintain its image of being a producer of 'unbiased' and 'non-political' advice, and has come to doubt 'which master to serve' (i.e., science for science's sake or for the sake of management) by the growth of its advisory role. In ICNAF, science and management were conducted in close and reciprocal interaction. While we cannot say for sure what lies behind these apparent differences, we suggested that ICES's long and proud history within marine science may have played a role. This is to be seen in contrast to the problem-oriented ICNAF, in which the mandate of science, namely to serve management, was more evident and singular.

identifiable to be seen as legitimate by (other) critical stakeholders; and a workable boundary between those who represent in politics and those who are represented. At its best, the machinery not only produces 'sustainability' but also routine 'politics' in terms of allocations of pre-agreed and legitimate quota shares, which are the translated into catches, landings and data for the next assessments, which in turn are used for the production of routine 'science' for the next round of the TAC Machine.

A smoothly running TAC Machine is conditioned on a range of assumptions, of which I here mention two (see paper 2 for a more comprehensive list). *First*, it is important whether the fishery of a stock is (or can be) bounded from the fisheries of other stocks, such that they can be singled out in catch forecasts and TACs. When this is not the case, as for instance regards a range of demersal North Sea fisheries, the TAC Machine easily gets into trouble. Moreover, we discuss how the stipulation of such singularity in mixed fisheries contexts covers implicit political stakes. If such stakes are made explicit by new models and metrologies, sacred political allocation compromises too risk getting into trouble; mixed fisheries easily lead to mixed science-politics. *Second*, the ability to control and enforce regulations is important. When these or other important conditions are not satisfied, the production of boundaries fails. Failure in one or more function within the machinery may easily set off negative cascade effects throughout the cybernetic circle (paper 2).

We characterize the interdependence of the means of representation and the means of intervention (Hacking 1983). When the stocks are down, control becomes more difficult (e.g., by way of increased discards and black landings), which in turns means that the assessment quality deteriorates. We term this property of the TAC Machinery 'non-precaution' (papers 1, 2, and 3); the more you need it, the more precarious it tends to be. Biases and increased uncertainty in assessments, combined with advices for low TACs in turn promotes dissent of angry fishermen who dispute ICES's science, commonly arguing for higher quotas (e.g., 'I am on the sea every day, I see a lot fish out there – ICES's talk about low stocks is rubbish').[118] A corollary of this is that, to paraphrase Stengers, the assessment laboratory no longer separates knowledge and

[118] Importantly, both ICES and the fishermen may simultaneously have a point in this situation: ICES, as regards the spatio-temporal scale of the 'stock' as it defines it; the fishermen as regards the scale of his activities (Degnbol 2003).

opinion; lay experts claim authority and work to legitimize the established advisory science. In other words, when the TAC Machine fails, the boundaries it produces, including the science-politics boundary, are disputed and undermined.

TACs as boundary objects

Switching to STS language, we can now recognize the TACs as 'boundary objects' (Star and Griesemer 1989), namely

> scientific objects which both inhabit several intersecting social worlds […] *and* satisfy the informational requirements of each of them. Boundary objects are objects which are both plastic enough to adapt to local needs and the constraints of several parties employing them, yet robust enough to maintain a common identity across sites. (Star and Griesemer, 1989).

The TACs, based on the catch forecasts, enable the cooperation of 'science' and 'politics'; they contribute to the maintenance of their common boundary and to sustain their differences. The maintenance of a 'common identity' of the TACs when they circulate across the various boundaries in the TAC Machine is a crucial issue of the TAC Machine. In Paper 2 we propose and explore mechanisms that undermine this identity. We can say that the conditions that need to be fulfilled for the TAC Machine to run, including efficient regulations and control, modest levels of mixed fisheries data of sufficient quality, etc., can be summed up as the embedding of the TAC boundary object in a wider 'standardized package' (Fujimura 1992).

Even at its best, the form of 'sustainability' that the TAC Machine delivers is rather 'slim' (paper 1). Stated differently, the TAC Machine's view on sustainability is 'oligoptic'. As discussed in paper 4, the TAC Machine defines, and makes visible, and seeks to control the population size of a particular creature, namely what Holm (2003; 2006) aptly named the 'cyborg fish' since it is both a *cybernetic organism* and a key referent in a *cybernetic system*.[119] The framing of 'sustainability' here is slim since in

[119] As explained in paper 4, I use the metaphor of the 'cyborg fish' (see http://www.cyborg-fish.net/ visited 1.03.08) to summarize the long chain of standardizations and assumptions that frame and make possible the calculation of single stock assessments and forecasts. Why didn't I just write out what these assumptions etc. were? I actually tried that. I worked on a draft in which I intended to write a detailed, but to the layman understandable, text on how a standard ICES VPA-based (i.e. XSA) assessment is performed in practice,

practice a range of concerns that seem to be relevant to it have been excluded from the operationalized production of ‘sustainability’.

In summary, the TAC Machine (paper 1) offers an account of how science and politics become co-produced, featuring on the one hand the development of the extended fisheries laboratory – the macroscope that makes possible the observation of the cyborg fish – as an apparatus that makes fish *count* in the double sense; and on the other hand, the Fisheries Leviathan (paper 6) installed to take decisions, divide lots, and control. The Leviathan is the composite body that this macroscope serves.

We are not saying that the co-production story outlined here is the only possible account of the institutionalization of fisheries resource management in the North-East Atlantic. But I hope the reader will agree that it is a significant story – not least, of course, when the focus is on science-politics dynamics. Our story is, so to speak, an oligoptic account of an oligopticon (Latour 2005). We, the authors, are the cartographers who map the TAC Machine (figure 1 in paper 1: figure 2 in paper 2). While this makes our role explicit and reflective in the sense discussed in the theoretical framework outlined above[120], it does not follow that our account is arbitrary. We have *reassembled* (Latour 2005) the TAC Machine, struggling to put together the heterogeneous elements it comprises in a tight and convincing order.[121]

Perhaps paper 3 can be regarded as a preliminary indication that the notion of the TAC Machine does not violate the Leibnizian constraint. Paper 3 is the ‘policy brief’ from the EU-funded project, Policy and Knowledge in Fisheries Management, with which we collaborated closely. This project was carried out as a case study of knowledge production and management of the North Sea cod. Since the management of the North Sea cod has fared poorly, this case was useful for an exploration of weaknesses of the established framework for knowledge production and management. It was in the context

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and what assumptions are made in this process. This proved to be a much more demanding task than I had expected. I gave up when my text was around 50 pages. At this point I had made it just a bit more than halfway through the ICES Working Group’s 2003 assessment (and forecasts) of the North Sea cod stock! The reader may turn to (ICES 2004) for the assessment example mentioned here, and to Darby and Flatman (1994) and Lassen and Medley (2001) for technical guidance.

[120] The role of the interpreters is made explicit in footnote 5 in paper 1 (page 16), which concerns the analytical status of ‘the TAC Machine’.

[121] We are painfully aware that the ‘TAC Machine’ as a text could be much tighter and probably also more convincing. There is a lot of editing left for us to do. We simply ran out of time.

of this project that we offered the first version of the ‘TAC Machine’ (Holm and Nielsen 2004), which later appeared to be helpful in structuring the contributions from different partners from different disciplines (including assessment science, sociology, economics, anthropology and history) in the PKFM project, and in the structuring of the summarized findings (i.e., ‘policy brief’) of this project.

ACFM as a boundary organization

As mentioned, the ‘TAC Machine’ stipulates the generic roles of science and politics in the modern regime of fisheries management in the North-East Atlantic. The role of science to assess single stocks and provide catch predictions on which the TACs can be based; the role of ‘politics’ to decide on TAC levels and to divide and implement and enforce TAC shares. Closing up on the changes in ICES’s Form of Advice by the introduction of its Precautionary Approach, papers 4 and 5 address the further developments and the refinements of the science-politics boundary.

Before we get to these studies of ICES’s form of advice, I want to make note on the Advisory Committee on Fisheries Management, which has the responsibility to review the assessments and forecasts made by the working groups, and to formulate the advice to ICES’s clients. It is useful to consider ACFM a ‘boundary organization’, i.e. an organization that exists ‘at the frontier of the two relatively different worlds of politics and science’ but which has ‘distinct lines of accountability to each of them’(Guston 2001: 401).

Notably, Guston (2001) explicitly ties his notion of boundary organizations in with the notions of ‘co-production’ of science and politics, ‘boundary objects’ and ‘standardized packages’:

> Boundary organizations are involved in coproduction [of ‘knowledge and social order’]: They facilitate collaboration between scientists and nonscientists, and they create the combined scientific and social order through the generation of boundary objects and standardized packages (Guston 2001: 401).

In the cell membrane metaphor of the science-politics boundary, ACFM is a membrane complex that organizes transport across the boundary, hence stabilizing both the boundary and the environments it separates. While located in ICES, ACFM indeed has different lines of accountability. Towards its clients, the management authorities, its responsibility is to provide an advice that is reliable, consistent and helpful; towards ICES assessment working groups it has the responsibility of coordinating tasks and to perform technical reviews of the assessments.

To wrap things up so far in a STS co-production idiom: the TAC is a *boundary object*; its circulation in the TAC Machine is stabilized by way of the *standardized package* that comprises the conditions that makes the Machine work and hence allows it to produce and maintain its multiple boundaries; the ACFM is a *boundary organization* that facilitates and authorizes exchanges across the science-politics boundary through its processing (including review and certification) of the boundary objects, and through its contribution to the standardized package that supports them.

Separating and re-entangling fisheries science and politics

As mentioned previously, ACFM's advice offers a concrete site to study the science-politics boundary. ACFM formalizes an important part of its role as a boundary organization by inscribing it into its Form of Advice. This advisory format and its changes through time (paper 2: pages 674-675) are in focus when I explore the science-politics boundary in this context. In papers 4 and 5, we explore how ACFM conceptualized and communicated uncertainty in its advice as it became formalized in 1997 by the implementation of ICES's Precautionary Approach (PA).

Uncertainty is a key boundary issue in mandated science because it is not *a priori* clear whether or how uncertainty is the responsibility of 'science' or 'politics'; it depends on how uncertainty is conceptualized, and it appears that such questions must be sorted out *in situ*. This is why the framing of uncertainty is central to Jasanoff's studies of the negotiated boundaries between (advisory or regulatory) science and politics (Jasanoff 1986; 1990). The challenge to organize the science-politics boundary in relation to uncertainty is acute when we talk about complex and semi-open systems, such as the fisheries and the ecological and social contexts relevant to them. Recall that Funtowicz and Ravetz (1993) characterized science in contexts where 'facts are uncertain, values in dispute, stakes high and decisions urgent'. Papers 4 and 5 explore such post-normal features of ICES's advice.

As discussed above, the TAC Machinery, at least as it is practised currently, is committed to a high degree of accuracy in the catch forecasts – an accuracy that cannot always be delivered. Moreover, the generic role division between science and politics, as stipulated within this machinery, becomes threatened when predictions fail. These are two reasons why uncertainty is imperative in this context, and that may help explain the considerable efforts ICES has invested in making its PA framework operational.

In paper 4 I explore how the general discourse on precaution was translated into ICES's Form of Advice in the mid 1990s. In parallel with our argument in paper 1, namely that 'sustainability' becomes rather narrowly framed when made operational, I argue that 'precaution' too became narrowly framed when it met up with fisheries resource management, which in the ICES context had settled into the TAC Machine format at this time. While Wynne (1992b) proposed that the precautionary approach should involve 'much more than shifting the threshold of proof to a different place in the same body of knowledge' in order to constitute a resource to reform environmental science and policies, I propose that the set of reference points that comprise ICES's Precautionary Approach effectively can be regarded as little more than new handles within the TAC Machine device – indeed 'within same body of knowledge'. I explore ICES's Precautionary Approach framework as it is laid out in the introduction to ICES's advice and look at one example of its use for providing a concrete stock advice. I analyse and discuss forms of uncertainty that are conceptualized and communicated, and forms that are not.

ICES's PA framework simultaneously translates 'precaution' and 'uncertainty' in the assessments and the catch forecasts. The aims of this framework appear to be twofold: To conceptualize and communicate uncertainty to its clients, and to clarify the science-politics boundary. In the latter respect its strategy pertains to how 'risk governance' fundamentally has become institutionalized in the US and in Europe, namely through an approach that

> centres on the assertion of a clear-cut separation between the realms of science and politics, or between 'risk facts' and 'values'. Intended partly as a means to inhibit manipulation of scientific representations by political interests, this distinction is formalised as a categorical separation between 'risk assessment' and 'risk management'. (Wynne et al. 2007: 32).

ICES's 'reference points', which comprise the back-bone of its PA framework, are exactly designed to institutionalize this divide between 'risk assessment' and 'risk management'. In practice, this is done by inscribing reference levels into the cybernetic parameters of the catch forecast, i.e., the 'F' to control 'SSB'. The PA framework defines critical reference levels (limit reference points: LRPs) and buffer reference levels (precautionary reference points: PRPs) for each of these parameters, such that the critical levels with an acceptable level of confidence are avoided when the stock is assessed to be on the appropriate side of its buffer level.

In paper 5 we explore in technical detail how the LRPs and PRPs are defined. The PA framework's separation of science and politics, that is, of 'risk facts' and 'values', hinges on the postulate that the LRPs are 'biological', while the PRPs, apart from the assessment uncertainty, depend on 'the amount of risk society is prepared to take' (ICES 2005). We propose that this conceptual clarity of the PA framework does not match well with the bewildering practices associated with defining and using reference points. We find a wealth of both linguistic and technical definitions, some of which do not correspond to the official PA Framework. It proves to be tricky, if not somewhat arbitrary, to define a threshold SSB level in relation to nebulous SSB-recruitment plots; there is an ambivalence regarding whether the PRPs should take the assessment uncertainty or the prediction uncertainty into account (which is much higher than the former) and so on. It appears that no methods for defining reference points are both general and plausible. Accordingly, 'risk facts' and 'values' – risk assessment and management – inevitably become entangled (paper 5).

Further, the catch prediction's dependence on a range of assumptions, of which some depend on how 'managers' regulate and enforce, and how fishermen abide by regulations', produce indeterminate forms uncertainty. Since the science-policy boundary depends on the asserted predictive ability, this dependence involves new forms of entanglements; the particular framing of uncertainty in this science-politics system produces particular overflows that are explored in paper 4.

To recapitulate: paper 4 and 5 explore a process of refining the science-politics boundary in relation to uncertainty by the implementation of ICES's Precautionary Approach. This boundary work involves the deployment of the cultural repertoire of striving for a separation of risk assessment/facts – which is for science – and risk management/values – which is for 'managers' or society's representatives. This cultural repertoire is mobilized by way of its inscription into calculative practices, mediated by the 'laboratory of fish counting'.

Collective performativities and *agencements*

The last theme in this limited review of the papers in regard to the outlined theoretical framework concerns the issue of performativity, which Callon (2007: 316) defines as when a discourse 'contributes to the construction of the reality that it describes'. Performativity was explicitly discussed in the theoretical section above, regarding the relationships between the boundaries of science and the observer of those boundaries.

Paper 6 is a contribution to Michel Callon's programme of performativity of economic theory (Callon 1998), which forms part of a pragmatic turn in economic sociology (Callon, Millo and Muniesa 2007). The idea of the former is that economic theory not only *describes* the economy but also contributes to the standardization and formatting of markets; it contributes to the framing in which agents can calculate economic possibilities; in general it contributes to *make* markets (MacKenzie, Muniesa and Siu 2007). Specifically, the paper forms part of an edited book project titled, *Market Devices* (Callon et al. 2007). The concept of 'market device' here identifies 'the material and discursive assemblages that intervene in the construction of markets' (ibid.: 2).

We study the emergence of the Norwegian market for Individual Tradable Quotas (ITQs). The question we pursue concerns to what extent we can attribute resource economists a strong performative role, that is, an orchestrating agency, in the chain of events that leads to this particular market and its particular characteristics. We suggest that the notion of performativity of economics may, if not qualified further, invite the suggestion of such a strong agency. We quote a resource economist who asks, in a paper in the *Journal of Environmental Economics and Management*, what difference resource economists have made. His answer, that 'the profession's most important policy achievement must surely be its influence on getting the ITQs on the agenda as a viable instrument' (Wilen 2000: 321), in fact suggests a rather strong agency.

Yet we find that the notion of performativity of economics in this case may invite an exaggeration of the influence that resource economics had in the development of this ITQ market – an exaggeration that would tend to cover much more complex processes that we try to unfurl in paper 6. This, however, does not mean that we aim at undermining the notion of performativity but that we find that many other agencies than those pertaining to the particular discipline of resource economics contributed to the construction of the ITQ market. Just as Popper or Merton cannot perform science's boundaries alone (while Gieryn watches them do so) the resource economists cannot alone make ITQs.

At this stage it may not come as a surprise to the reader that the principal market device we propose here is the TAC Machine. The TAC Machine not only renders the right to fish an exclusive good, but also enables property rights to be inscribed and stabilized in terms of legally defined shares of this good. In other words, the TAC Machine is the principal device that makes property rights stick to slippery fish, and thereby turns them into (possible and realized) commodities. Hence, we do not analyse the TAC Machine as the result of a grand design (designed and devised by a strong pre-established agency) but as an outcome of complex co-construction processes (paper 1 and paper 6).

Along with the editors' introduction to *Market Devices* (Callon et al. 2007), the idea of the TAC Machine as a result of collective performance, involving the enrolment and transformation of a range of heterogeneous actors and entities, brings us to explore and reconsider the relationship between agency and devices. When, in paper 1, we attributed to the TAC Machine the analytical status of a '*dispositif*'[122] – which is the French word for device – we follow the editors in preferring a Deleuzian understanding of the '*dispositif*': 'For Deleuze, the subject is not external to the device. In other words, subjectivity is enacted in a device.' (Callon et al. 2007: 2).

[122] See footnote 5 (page 16) in Paper 1.

To underline that there is no divide between the device and the subject and the statements that refer to the device, Callon and colleagues (Callon 2007; Callon et al. 2007) prefer to talk about *agencements*.[123] By the emergence of the device we have named the TAC Machine, a range of new agencies/subjectivities[124] become available: ICES scientists become fish counters and stock forecasters; ‘managers’ become those who decide, enforce, and regulate TACs; fishermen, in addition to their previous fishing related identities, become potential compliers or violators of TACs and property owners and traders; resource economists become experts on the calibration of quota markets; and – not to forget – the authors (of papers 1, 2, 4 and 6) become experts on TAC Machines!

To summarize abstractly, the practical division of labour between fisheries science and management, as well as norms that address this division, are tied in with what we term the TAC Machine device. The stability of this boundary hence depends on the stability of the device – not least the stability of the knowledge production it supports. To paraphrase Jasanoff (2004), the TAC Machine device stipulates a ‘co-production of science and social order’ and even a new natural order insofar as the oceans can be said to have been transformed from a wilderness to a semi-transparent aquarium (Pálsson 1998). I have illustrated how discursive practices captured by the notion of ‘boundary work’ mediate the development and refinement of this device, and how contextualized boundary work practices reciprocally are mediated by it; in this sense these are co-produced and must therefore preferably be empirically studied as such. We cannot expect science’s norms and boundaries to have the same shape in practice as when they are conceptually distilled in the laboratories of the philosophers and sociologists of science, and therefore we need to study them *in situ* too.

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[123] ‘*[A]gencements* are arrangements endowed with the capacity of acting in different ways depending on their configuration. This means that there is nothing left outside *agencements*: there is no need for further explanation, because the construction of its meaning is part of an *agencement.* A socio-technical *agencement* includes the statement(s) pointing to it, and it is because the former includes the latter that the *agencement* acts in line with the statement, just as the operating instructions are part of the device and participate in making it work’ (Callon 2007: 320).

[124] See the latter part of footnote 87 for a related point, concerning a relational concept of agency.

7. Concluding remarks

> [T]he precautionary approach has no science basis – it is but a set of values. […] How far can science go: where does science stop and the decision process begin? […] There should be a clear border line and […] the two issues should not be mixed. (ICES client, cited in paper 5).

Is there an identifiable point where science stops? Is that the point where politics begins? What does it mean that ICES's Precautionary Approach 'has no science basis'? In what sense are ICES's PA reference levels 'but a set of values'? Can there be a clear border line between issues of science and issues of politics? Why should they not be mixed? I hope that we are now better prepared to comprehend, discuss and evaluate the significance of the statement cited here.

As will be recalled, the theoretical objectives of this dissertation has been to explore processes of construction, maintenance, and transgressions of science-politics boundaries in mandated science based on a case study of ICES advice on fisheries resource management in the Northeast Atlantic. With a starting point in the perspective I aimed to develop in order to explore this, the practical objective was to contribute to the discourses on the fisheries management. Although such a division by the notion of empirical philosophy is artificial, it is convenient to divide the main outcomes of this work into theoretical and empirical/practical findings.

The project's theoretical contributions are mainly presented in this introduction, in which I have aimed at developing a framework for studying the construction of boundaries in question more effectively. This framework is centred on the notion of co-production of (mandated) science and politics. It combines Gieryn's concept of boundary work, in particular his notion of 'cultural repertoires' with (Latourian) ANT, which takes scientists' practices in their laboratory (understood in a broad sense) into account. The 'cultural repertoires' available for defining what science is or should be contribute to the development of this laboratory, which co-evolves with the political institutions that the science in question is mandated to serve. These repertoires, however, become translated in the process. This framework simplifies Latour's (1993) scheme in considering 'purification' as a form of 'translation'. The framework builds on, and is consistent with, ANT as it is rendered in Latour's *Reassembling the Social* (2005).

The framework is intended to be reflective in a sense explained in the following. While Gieryn (2003: 394) invokes a contrast between 'essentialists' who '*do* boundary-work' and 'constructivists' who '*watch* it get done by people in society', the framework proposed here observes and takes into account that the interpreter too performs boundary work. This is for two reasons. *First*, regardless of the extent to which an interpretation of social practices is deployed in a normative or a descriptive mode, it will, if well argued and otherwise successful, loop back and interact with what it refers to. *Second*, the interpreter will, in order to comprehend it, in some sense need to engage with the practitioners who sustain the object of study. This enrolment is mutual and excludes the fiction of detached view from nowhere. Instead, the (unattainable) ideal of 'generalized symmetry' is proposed to enhance reflection on the interpreter's role in assembling the object of study. A specific interpretation of Stengers' notion of a Leibnizian constraint is offered as a necessary (but not sufficient) acceptance criterion for the proposed form of boundary studies.

Recent developments in science/society relations, namely the shift from 'the world of science to the world of research' (Latour 1998) and the emergence of 'post-normal science' (Funtowicz and Ravetz 1992; 1993) may motivate the further development of such a co-production framework. In general, there seems to be a need to complement a traditional philosophy of science – in so far as it conceives science as an autonomous activity – with a philosophy that aims to take contemporary developments of the relationships between science/research and society into close account, and which therefore could be termed an 'empirical philosophy' (Mol 2003).

As regards the project's contributions to the academic fisheries literature, one of its major outcomes is the conceptualization of modern resource management in the Northeast Atlantic as a 'TAC Machine'. Focusing on the co-evolution of VPA based assessments and catch forecasts and TACs, we have offered a few steps towards an account of the institutionalization of this machinery, hoping that they can lead on to more effective histories of modern fisheries management. We argue that these methods for representing fish stocks and for intervening in fisheries mutually promoted each other and were central in this institutionalization process.

Half a century ago, talk about 'precaution' and 'sustainability' in the marine environment was non-existent or, indeed, just talk. Today we have a precautionary TAC Machine, designed to produce sustainability on a routine basis. It may not always succeed in that, and it may be a slim sort of sustainability and a risky sort of precaution that it works to attain. But its presence cannot be disputed. In fact, the presence of this resource management machinery is so pervasive, and has so many and such diverse consequences, that it makes sense to think of it in terms of an 'invisible revolution' (Holm 2001). We have discussed whether and how we can talk about it as a failure (paper 2). In the short term, the TAC Machine evaluates itself, e.g., by the sustainability criteria it defines. In the long term, it also produces a critique in which the appropriateness of such evaluations is evaluated. In any case, the TAC Machine device makes a difference that will continue to make new differences.

One such difference is in this project's focus, and concerns how this device stipulates roles of science and politics in this modern form of fisheries resource management. In general, the TAC machinery produces and depends on a range of boundaries. The stability of these boundaries in turn depends on a range of practical and technical factors. To the extent these factors are in place, the machinery may work adequately to achieve stability by way of negative feedback mechanisms. Conversely, we have indicated how failures (which are likely to arise in mixed fisheries, low stocks or contexts of limited control) may set off destabilizing cascade effects (positive feedback mechanisms). This is why we suggest that this machinery in a certain sense is fundamentally non-precautionary.

In particular, the stability of the science politics-boundary is conditioned on the ability of assessment science to quantify stocks and to provide catch forecasts. In the contexts of high exploitation rates, there is a high degree of commitment to relatively accurate catch predictions since the stocks are generally kept in the vicinity of the lowest acceptable limits. Since this precision often cannot be delivered, uncertainty is of crucial significance to the science-politics boundary issue. The uncertainty in ICES advice has been formally addressed by the development of ICES's PA framework. This framework can be seen as a further development of the organization of the science-politics boundary that builds upon and refines the generic role division stipulated by the TAC Machine.

The science of assessing fish stocks is difficult and uncertain. Since it is nevertheless used directly in political decision-making, it easily becomes the subject of controversies. In crisis situations not only do the assessments of stock levels become disputed, but also the reference levels defined by ICES to evaluate them – as illustrated by the above statement of ICES's client. This indicates how ICES's advisory science, in particular in a crisis context, can be characterized as post-normal, implying that facts and values – risk assessment and risk management – become entangled.

Plato did not consider it necessary to defend the authoritative epistemic status of geometry. In fact, he could take it for granted and use it rhetorically in his knowledge-politics. In contrast, the epistemic status of ICES and its advices – in spite of the large 'laboratory of fish counting' it commands to back them up – easily becomes disputed. This spurs ICES into the immense struggle of collecting more data and improving its quality, and into developing alternative assessment models, and making endless numbers of trial-runs of assessments, each with different assumptions and associated caveats. It also makes it necessary for ICES to invest in various forms of boundary work, deployed to maintain its epistemic authority. Much in this work points to the fact that ICES adheres to norms implicit in the critical client's remark. ICES too believes that the key to epistemic authority is epistemic autonomy; the client and ICES both commit to and rehearse interpretations of science akin to those represented by Popper, Lakatos and Merton. Using Latour's (1993) terminology, ICES and its client agree that the epistemic authority of science (and its legitimacy) is approved or disapproved by reference to the Modern Constitution.

To wrap up and conclude: Fish stock assessments are subjected to high uncertainties and a large extent of politicization of knowledge claims. Therefore, the maintenance of epistemic authority implies hard and sustained work on the boundaries. This is the case for ICES not least because it is so close to the decision-making on resource management, which in turn has immediate consequences for resource users. How to be apolitical when practically framing political decision-making? How to sustain epistemic autonomy when mandated to serve politics? To the extent ICES and the client quoted above disagree, their disagreement is made possible by agreement on the underlying norms by which the issues at stake should be evaluated. What they may disagree on is whether ICES is able to satisfy these norms. I have suggested that this in turn not only depends on organizational aspects of science and politics but also on what we can refer to abstractly as cybernetic features of fisheries management. Boundary construction by the TAC Machine is a double-edged sword; it may create workable boundaries when it runs well, and they appear to break down when it doesn't. In either case we need to take this device, and how it mediates norms, into close account.

Science fictions in fisheries governance

We have now reached the end of this study of the boundaries between mandated science and science-based politics. Since the empirical part is concluded, we may take the opportunity to speculate and reflect on the properties and norms of such boundaries: Can we say what the best form of organizing the interaction between science and politics is? Granted that science and society increasingly becomes 'mode-2', does this imply that the 'mode-1' norms that it took such a long time to develop and distill are to be rejected? Or should the ideals of mode-1 science be aimed for in certain contexts, however difficult or impossible it would be to realize them? What contexts are those? Are mode-1 norms no longer appropriate, or should they be reconfigured to form parts of a set of virtues appropriate to a new science mode?

A particular dimension of such questions pertains to whether science should aim at being closed, autonomous, and *disinterested*, or whether different groups of users in society that are *interested* in, and affected by, this knowledge should be integrated into both the production and use of this knowledge. Perhaps they should even be included in the process of reframing the normative basis by which the knowledge production can be evaluated? I suggest that we can reflect on this issue by revisiting the case of fisheries resource management – but this time in the form of two scenarios or (mode 1 vs. mode 2) science-fictions:

Mode 1: the Ultimate TAC Machine

The Ultimate TAC Machine involves the completion of the TAC Machine's implicit design: it must be more accurate, more powerful; it must revolve swiftly and faultlessly.

On the science side, this entails increased investments in the collection of data and development of assessment methods. This involves increases in research surveys, discard-samplings, and improved estimates of misreporting. Moreover, assessment and forecast models must be expanded to take ecosystem interactions into account. This not only concerns predator-prey relations but also abiotic factors (e.g., oceanographic changes). Investments in data bases and methodology required to improve the definitions of Precautionary Approach reference points must be made but must also be complemented by the development of other ecosystem health indicators.

As regards management/politics, the regulations must be appropriately enforced through investments in monitoring and control (e.g., satellite based and/or video monitoring systems). The legal framework must be strengthened and simplified, and the efficacy of trials and penalties enhanced. Importantly, politically representatives must agree on and commit to specified management objectives. These objectives in turn must be made operational by integrating them into specific harvest control rules.

To sum up: *First*, investments are needed to make the existing resource management system work. *Second*, society's representatives must decide on and sanction a management plan. *Third*, once the management plan is agreed on, politics must withdraw to let ICES and the control apparatus run the machinery effectively in accordance with the plan.

Mode 2: Participatory Governance

The underlying idea is that stakeholders' concerns and their knowledge of local conditions must be taken directly into account if a legitimate management system is to be obtained. The key property for knowledge in governance is not that it is epistemologically authoritative but that it is salient and socially robust. Moderate co-management schemes involve stakeholders through information/consultancy on scientific advices that informs a centralized decision-making, or focuses on the implementation of such decisions. This form of co-management, in turn, goes further by including resource users in normative and cognitive aspects of the institutional design and practical operation of the management system. An important step here is to design shared indicators to enhance communication and cooperation between scientists and resource users (Degnbol 2005). As proposed in paper 4, one way to organize this would be to shift the burden of proof to resource users, such that they would carry the burden of justifying the sustainability of their practices. This would provide strong incentives for a close cooperation between resource users and scientists.

To sum up: The emphasis of this model is on bottom-up governance systems in contrast to top-down command-and-control management. Instead of defending the boundaries of a scientific authority towards its audiences, stakeholders are encouraged to cooperate in the knowledge production. Instead of relying on the 'more science' approach to reduce uncertainties, this approach recognizes post-normal features of the knowledge basis, and aims at dealing with them by extending science's peer-communities.

These two models are to some extent caricatures. I have twisted the former slightly in the direction of a gloomy discipline-and-punish (Foucault 1977a) image while letting the latter be slightly overloaded with concepts with positive connotations – which remain somewhat under-defined in practical terms. Maybe this is just a bit too familiar: whenever serious issues are at stake, ANT takes cover in cheap irony. While I actually think that the form of irony that is common in ANT can be useful for reopening 'matters of facts' as 'matters of concern' (Latour 2004b; 2005), the situation here is different because we talk about two possible paths into the future – which from the outset is a matter of concern. Before making up our minds, however, I suggest we take a closer look at them.

I have labeled these models mode-1 and mode-2 to hint at certain characteristics of their implied science-society (or science-opinion) relationships. The first could be characterized as 'enlightenment government', the second as 'democratic governance' (Elam and Bertilsson 2003). Thus I have not intended to suggest complete designs of fisheries systems, but only to bring these relationships into focus. TACs, for example, could be deployed as key constituents of a system based on participatory governance. Moreover, mode-1 and mode-2 science models could be combined. For instance, if resource users were to carry the burden of evidence (in cooperation with scientists), there could still be a need for an external institution to evaluate the conclusiveness of this evidence. This could be an appropriate task of ICES, which in fact would bring it much closer to its (mode-1 type) epistemic norms, since it would take it one step further away from political decision-making.

However, if we imagine a crossroad between these two simplified images of science in society, which path to prefer? Hard pressed, I suggest that both models are possible. Both represent proposals for how to address a range of problems. The appropriateness of these proposals depends on characteristics of their socio-natural environments. While the Ultimate TAC Machine would be overly expensive to install, established TAC Machines may in some contexts be seen as both functional and legitimate. This could be the case for some important Norwegian fisheries, in which ICES advice, and the TACs that are decided on the basis of them, largely appear to be regarded as legitimate. Politics could accordingly shrink into fine-tuning of regulations and shares. In the EU's Common Fisheries Policy things look different. There is a pending crisis in many fisheries, and both the legitimacy of the management system and the credibility of science are low. While the stickiness of the TACs and the machinery that sustains them should not be underestimated, there appears to be a growing discourse on participatory governance, decentralization, democratization of expertise and so on in Europe's fisheries. The developing Regional Advisory Councils[125] on fisheries issues could be seen as extended peer-communities of science, although they have not been granted decision-making powers – at least not yet.

If I still appear disappointingly indecisive regarding the crossroads conceived here, it is for two reasons. *First*, the performative aspects of the discourses that form part of the *agencements* in question make it somewhat difficult to determine what will be the appropriate path and what will not. It is even too early to tell whether my own humble contribution will be taken to have added more glue to sticky TAC Machines or whether it will be mobilized to the empowerment of lay experts, dedicated to overthrowing them. *Second*, my aim has been to be critical in Foucault's sense explained above: I have tried to point out on what assumptions and modes of thought – and modes of science – these

[125] See http://www.nsrac.org/ (visited 06.03.08).

models rest. I have aimed to open up science and politics in the hope of contributing to a discourse in which the repertoire of deliberations on their relationship can be renewed. As a prospective STS scholar, I leave it open whether my answer to the question of what path is preferable would count as science or just an opinion. But I hope that I have offered you more than that.

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