The paradoxes of transparency: science and the ecosystem approach to fisheries management in Europe

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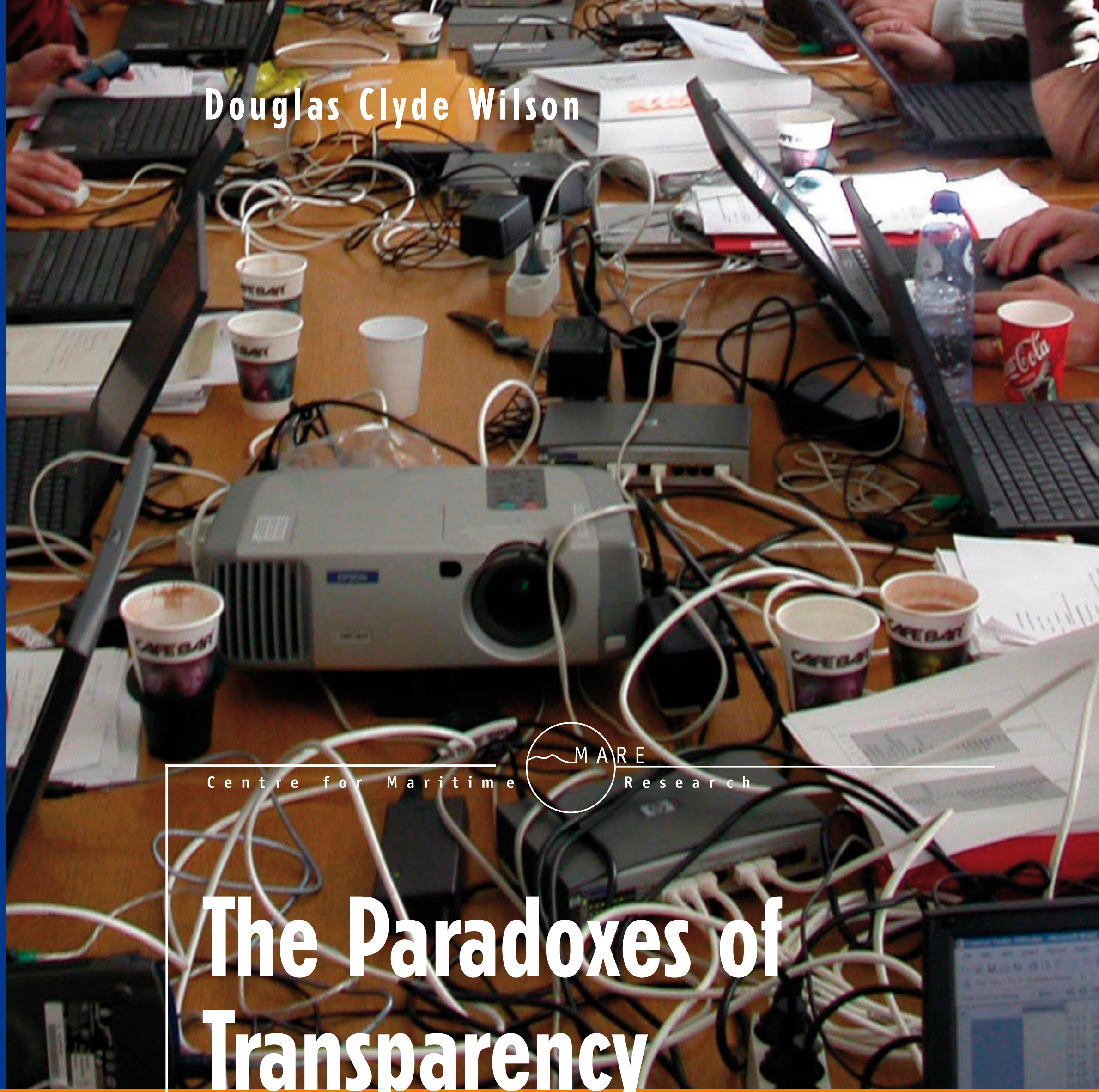

Douglas Clyde Wilson

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The Paradoxes of Transparency

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Amsterdam University Press

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SERIES FOREWORD

As editors of the MARE Publication Series, we are proud to present yet another major work on people and the sea. The topic of Doug Wilson's important and timely book is the role of natural scientists in fisheries management and environmental governance. Its focus is on the institutions that provide the scientific basis for decision-making with regard to European fisheries policy. A prominent organisation in this context is the International Council for the Exploration of the Sea (ICES), which involves twenty member states and serves as the hub of a network of approximately 1600 scientists.

The marine environment is difficult to observe so the scientific uncertainty is very high. Hence the crafting of scientific advice for an ecosystem approach to fisheries management is a complex challenge. How do scientists communicate uncertainty among themselves and with the outside world? And how well does science mix with advice? Wilson lucidly discusses these and other important questions.

Drawing on "communicative systems theory" and employing a wide array of data collection methods, Wilson provides deep insights into the challenges and dilemmas involved in providing scientific advice to a demanding political process. Can science really deliver what stakeholders expect and policy makers are asking for, i.e. rapid answers with minimum uncertainty to complex issues, without compromising what science is meant to be?

These are all pertinent questions for fisheries management but they also have more general relevance. Indeed, fisheries may well provide a test case for our capacity to deal with a range of environmental issues, in which science is called upon to provide the knowledge base necessary for effective and rational collective action. Thus, this book is also a contribution to the Science and Technology Studies that are now enjoying widespread interest within academic circles and beyond.

Finally this book is valuable in helping to cross the divide between natural and social sciences. It demonstrates how sociology of science perspectives and methods can help us understand the contribution of natural sciences, and their representatives, to resolving complex societal issues.

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To Prof. Joe Francis

Many wonderful people contributed to my formal education, but 20 years is time enough to know which lessons proved the most useful. Others did more to help me learn what to think about, but no one did more to help me learn how to think.

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Abbreviations

ACE	Advisory Committee on Ecosystems
ACFA	Advisory Committee on Fisheries and Aquaculture
ACFM	Advisory Committee on Fisheries Management
ACME	Advisory Committee on the Marine Ecosystem
ACOM	Advisory committee
ASC	Annual science conference
Blim	Lowest limit on size of a spawning stock biomass
Bpa	Precautionary target spawning stock biomass
BSFC	Baltic Sea Fisheries Commission
CEC	Commission of the European Communities
CEFAS	Centre for Environment, Fisheries and Aquaculture Science in Lowestoft, UK
CFP	Common Fisheries Policy
CGIAR	Consultative Group on International Agricultural Research
ConC	Consultative Committee
CST	Communicative Systems Theory
DCR	Data Collection Regulation
DG ENV	Directorate-General for the Environment
DG MARE	Directorate-General for Fisheries and Maritime Affairs
EAFM	Ecosystem Approach to Fisheries Management
EASE	European Advisory System Evaluation Project
EC	European Commission
EU	European Union
F	Fishing mortality
FAO	UN Food and Agriculture Organization
FRS	Fisheries Research Services in Aberdeen, UK
HCR	Harvest Control Rules
HELCOM	Convention for protecting the Baltic Sea
IMARES	Wageningen Institute for Marine Resources and Ecosystem Studies in Ijmuiden, the Netherlands
IMR	Institute of Marine Research in Bergen, Norway
ICES	International Council for the Exploration of the Sea
MCAP	Management Committee on the Advisory Process
MICC	Management Committee on the Advisory Process Meeting with ICES Client Commissions
MPA	Marine protected area
NAO	North Atlantic Oscillation

NASCO	North Atlantic Salmon Conservation Organization
NEAFC	North East Atlantic Fisheries Commission
NFI	National Fisheries Institute
NGO	Non-governmental organization
NOAA	National Oceanographic and Atmospheric Administration (USA)
NSCFP	North Sea Commission Fisheries Partnership
NUSAP	Number, unit, spread, assessment, pedigree
OPEC	Organization of the Petroleum-Exporting Countries
OSPAR	The OSPAR Convention for protecting the NE Atlantic Ocean
PKFM	Policy and Knowledge for Fisheries Management project
PNS	Post-normal science
RAC	Regional advisory council
REGNS	Regional Ecosystem Study Group for the North Sea
RSE	Royal Society of Edinburgh
SAFMAMS	Scientific Advice for Fisheries Management at Multiple Scales project
SCICOM	Science committee
SGFI	ICES/NSCFP Study Group on the Incorporation of Additional Information from the Fishing Industry into Fish Stock Assessments
SGGROMAT	Study Group on Growth, Maturity and Condition in Stock Projections
SGPRISM	Study Group on Incorporation of Process Information into Stock Recruitment Models
SSB	Spawning stock biomass
STECF	Scientific, Technical and Economic Committee for Fisheries
STS	Science and technology studies
TAC	Total allowable catch
ToRs	Terms of reference
VMS	Vessel monitoring system
VPA	Virtual population analysis, a type of stock assessment model
WGECO	Working Group on Ecosystem Effects of Fishing Activities
WGNSSK	Working Group on the Assessment of Demersal Stocks in the North Sea and Skagerrak
WGRED	Working Group for Regional Ecosystem Description

Preface

The book consists of three theoretical chapters, four empirical chapters, and a conclusion that seeks to pull the whole thing together. The theoretical chapters explain a number of concepts that I use to organise and relate the empirical material, which is mainly a description of the work of the International Council for the Exploration of the Sea (ICES). Fisheries management is an area of environmental management that contains many potential lessons for how we can relate to our planet in general. The point of social theory is to provide vocabularies and concepts that allow lessons learned in one kind of social endeavour to be translated for use in other kinds. Hence the value of this book, to me, lies as much or even more in the theoretical discussion than in the empirical work. It helps me, and I hope others, when thinking about new problems.

However, I realise that the interests of many readers will be simply on fisheries and marine management in Europe, and that what happens in ICES is what they will want to read about. Those readers will likely wish to start with Chapter Four. This should work fine for their purposes. However, I would encourage this group to skim Chapter One for the sake of orientation. I would also suggest that they read Section 2.2.1 about Mode Two Science and Section 2.2.3 about Post-Normal Science and examine Section 3.1 long enough to get a sense of the way I am using the terms saliency, credibility and legitimacy. I believe this effort will lead to a quicker understanding of the presentation of the case material.

Douglas Clyde Wilson
Hirtshals, March 2009

Acknowledgements

It is well beyond my ability to list all of the people who contributed to this book. For one thing, the most important group is very large. It includes the many fisheries scientists, fishing industry members, conservation advocates, fishery managers and policymakers across Europe who allowed themselves to be interviewed, questioned and/or observed. They were, almost to a person, helpful and forthcoming. Particular mention should be made of the ICES staff and leadership who directly facilitated this research in different ways: Bodil Chemnitz, Paul Connolly, Gerd Hubold, Hans Lassen, Solveig Lund, Inger Lützhøft, Martin Pastoors, Vivian Piil, and Poul Degnbol, who later also guided me through the labyrinth of DG MARE. Another group deserving special mention are those PKFM colleagues who carried out interviews that I have quoted in the text: Alyne Delany, Petter Holm, Kåre Nielsen, and Jesper Raakjær. Three EU Framework Research Projects have supported this work at various times: the Fifth Framework Programme supported Policy and Knowledge in Fisheries Management (PKFM), Contract no. Q5RS-2002-01782; the Sixth Framework Programme supported Scientific Advice for Fisheries Management at Multiple Scales (SAFMAMS), Project no. 013639; and the Seventh Framework Programme supported Judgement and Knowledge in Fisheries Including Stakeholders (JAKFISH), Grant Agreement no. 212569. This work does not reflect the Commission's views and in no way anticipates its future policy in this area. Finally, this book would not have happened, and it certainly would not have been readable, without the dedication of Kirsten Klitkou, Innovative Fisheries Management's secretary, meaning in reality our all-round document coordinator and quality control officer.

1. Introduction

1.1 Sensing the need for change

The topic of this book covers institutional aspects of providing natural science advice for the fisheries management programmes of the European Union and its immediate neighbours, particularly as they turn towards the Ecosystem Approach to Fisheries Management (EAFM). In many ways it is a straightforward application of the sociology of science. It applies various research methods and theoretical perspectives to the work of fisheries scientists in Europe to try to draw some insights on how we can do a better job structuring science for environmental decision-making in general and for the marine environment in particular.

That is the topic but not what the book is really about, to me at least. What drives me to write about the way that science fits into decision-making is an obsession with adaptation. I was trained as a human ecologist; I learned to think of society as a set of social systems embedded in a surrounding set of natural systems. What I know of the sociology of science I learned well after graduate school. I was drawn to it by my reflections on the four simple steps that General Systems Theory outlines for all adapting systems. If a system is going to adapt to its environment it must: 1) sense the need for a change; 2) have its own potential to change in response; 3) have a way to select the change to make; and, 4) have a way to implement and maintain the change (Buckley 1967). The first and third steps rely on a good understanding of nature while the second and fourth steps require the system to be capable of effective collective action.

So how does a social system 'sense the need for a change' when it must adapt to changes in its natural environment? The term 'social system' can mean a lot of things. In my work, I think of a social system as a patterned set of communicative actions because this definition allows a firm distinction between what is social and what is natural. Society and nature are so intertwined that such a distinction would otherwise be impossible. Is a farmed field social or natural? Defining society as a set of communicative actions means that the substance out of which society is built is shared meaning. This has some strange implications – for example it places our bodies in society's environment – but it makes for a clear definition of the relevant system boundaries from both the ontological and epistemological angle. If the laws of thermodynamics apply, then the phenomenon is part of the environment of the social system. This also allows, in principle, human ecological reasoning to be applied in lots of areas that are not part of environmental studies, for example to health care.

Communicative Systems Theory (CST) (Habermas 1984, 1987; Wilson 2003) provides a set of conceptual tools useful for understanding society as a set of communicative actions. An approach that focuses on the realities between actors, i.e. shared meanings and the systemic requirements for reproducing them, provides a meaningful complement to the atomistic analysis of actors and their incentives that currently dominates social science. If there is interest in the volumes of argumentation for why this 'communicative turn' makes for good social theory and hence good human ecology, then I suggest going directly to Habermas. Otherwise, as Habermas (1990) himself argues, you will have to judge for yourself whether this approach offers a coherent account of the case at hand.

One thing that I hope this book demonstrates is that CST can make a contribution to Science and Technology Studies (STS), a broader field of which the sociology of science is just a part. A great deal of valuable recent work in STS has emphasised the alloyed social and technical nature of the modern world, and particularly the products of science and policy. The idea is that in most aspects of modern social life, scientific and social issues are woven together into complex hybrids (Freudenberg et al. 1995; Holm 2007; Latour 1991). Nature and technology are important drivers of social structures, and social structures determine understandings of nature. The co-production (Jasanoff 2005) of science and society through creative acts equally rooted in humanity and nature is ubiquitous. The *zeitgeist* of this literature is that an analysis that seeks to maintain the dichotomy between the social and the natural is unhelpful at best and likely to lead to illusion and error at worst.

While being unable to deny the empirical importance of the co-production of hybrid phenomena, I have never been comfortable with the conclusion that an analytical distinction between society and nature is unhelpful. One reason, mentioned above, is that the distinction is such a central one to human ecology. Furthermore, a background in critical theory makes one very distrustful of anything that suggests that reasoning about society and nature are similar exercises – I view the naturalisation and technicization of human interactions as a dangerous path.

CST allows us to recognise the hybrid nature of a technological society while maintaining a strong analytical distinction between society and nature. This distinction finds practical expression in differences in how people communicate with one another with respect to facts on the one hand, and values and interests on the other. The central CST concept of 'rational communication' is based on the requirement of establishing mutual understanding between two or more parties. Part of rational communication is the idea that the presuppositions of different kinds of communication are oriented according to different principles. As I will mention frequently and in more detail below, a discussion leading towards mutual understanding of facts presupposes the goal of consensus, while a discussion leading towards an understanding about values and interests presupposes a goal of compromise. This distinction does not ignore the emergent properties of socio-technical systems where facts, values and interests are connected in the substance of nearly all the issues we grapple with. It does, however, introduce a type of rationality which reflects our basic intuitions about how people understand each other. It is a basis for recognising the special role of the scientific assertion, and by extension a special role for scientists whose expertise is based on generating such assertions in a transparent way. This recognition, however, does not rely on the notion of science as a white-coated, objective other.

For a communications system to be able to adapt, 'sensing the need for change' has to be more than someone knowing that something important is going on in nature. It has to move to the next step of finding the potential to change in response – most generally, this means it has to become the basis of some sort of collective action, a term I use very broadly to include everything from effective policies and legislation to decisions taken at a community level. The knowledge about nature also has to be turned into something that people talk about, i.e. it has to become part of a discourse, and this talking has to be translated into some kind of collective action that has a chance to bring about change. First we need knowledge, and then we need that knowledge to influence collective action.

Knowledge in itself is a complicated social idea. Everybody has some knowledge, but the knowledge that scientists have has a special quality. In environmental studies we tend to think of two groups as having special knowledge about nature: the scientists who have research-based knowledge, and user groups that have experience-based, local, ecological knowledge. The scientists' knowledge is thought to be the best quality. Most people think that when a scientist says that some fact about nature is 'science', then that fact is as true as can be. I spend a lot of time with fisheries scientists, and when they tell me things about fish, I almost always believe them without question, and if I do not believe them right away, it is because some other scientist has told me something different.

But being somehow truer is not the real, special quality of scientists' knowledge. After all, they frequently change their minds. What makes the scientists' knowledge special in the imagination of human ecology is the way it links to the next step that is needed if the social system is going to adapt. Science has a uniquely high potential to be the knowledge base for collective action. This uniquely high potential is not a product of science's truth but of science's radical commitment to transparency. The scientific method is all about transparency. It is about the clear articulation of knowledge in a way that can be clearly challenged, i.e. held clearly accountable for the truth of its assertions.

This idea has an ironic tinge that stems, in fact, from the paradoxes of transparency after which this book is named. Science uses special tools and techniques, especially quantification, to create claims of truth that are clearly stated and able to be challenged. But these very techniques, meant to guard transparency, require skill and training to understand. They have also developed into cultural images with great rhetorical power. In practice, one might say, science suffers from transparency-induced opacity.

Effective adaptation is only possible when there is a link between learning and the decision-making that guides collective action. Our natural environment is large and complex. Keeping track of changes, whether brought about by human action or natural processes, requires large numbers of people and technologies, purely from the information angle. In practice, many of these people will be carrying out direct or indirect environmental monitoring as part of other activities such as extracting goods from the environment, taking pleasure in it, seeking to conserve it, or managing it on behalf of the public. All these activities generate knowledge, some of which will be useful for environmental management.

Some of this knowledge will be systematic, but most of it will be anecdotal. This systematic/anecdotal distinction is rooted, as are so many other things important to human ecology, in the question of scale. What makes anecdotal information less useful is not that it is less true; it is that it cannot be linked to information from elsewhere in order to characterise a phenomenon happening on a broader scale level. The point is that the systematic information needed to give a picture of widescale phenomena will rarely be available when environmental changes requiring adaptation are being recognised. Recognition will be based on anecdotal information that must then be supplemented by or formed into systematic information. In short, the knowledge base for adaptation requires a two-way process. It must tap into a lot of diverse information sources, and it must also find ways to shape that knowledge to get a broader picture. Thus, the need for wide and interactive participation in environmental management arises simply through information requirements even before we consider the decision-making aspects of governance.

Interactive participation (aka authentic participation, participatory democracy, discursive democracy, etc.) makes effective governance possible within an ecosystem approach. The translation of decisions into action is much easier when more people buy into decisions and the science they are based on. I do not intend to devote space here to showing that broad participation is necessary for good governance. This has been done extensively elsewhere by me and many other people. My general perspective is perhaps not as strong as that of many advocates of participatory governance in fisheries. While participation is one of a set of characteristics of effective environmental management institutions, the most basic of these requirements is that the institutions are rooted in the authority of a democratic government. This is a basic lesson from fisheries co-management systems around the world. Bottom-up effectiveness through participatory democracy happens best in a top-down, formal democracy context. Without this underlying top-down system, co-management and participatory democracy are lost because accountability has to operate both up and down. In any event, here I assume rather than defend the need for broad participation in environmental management and focus on the question of how participation in building a knowledge base by and with scientists is best achieved.

Participation and science do not go easily together. We analyze participation by observing the various 'discoursive themes' that different groups draw on in their discussions and arguments (Klenke et al. 2009). Such themes are formed by linking together facts, values and interests into coherent positions and arguments. Certain facts fit in with certain values and reflect certain interests. By interest here I mean economic and political interests, but there is also an illuminating connection to the other meaning of the term; your work and your values influence what you find personally interesting. The linkages between facts, values and interests have a direct impact on the ways that facts are learned, selected and presented to others. All too often they lead people to stubbornly ignore facts that are not linked to their values and interests. This can be the result of simple recalcitrance, but it happens among people who are committed to finding solutions as well. It is not some sort of moral failing, although it often feels that way when we listen to the people who oppose our positions. It makes arriving at collective action difficult, but not impossible.

The way we talk about natural facts[1] is different from how we talk about values and interests. This comes from a principle of 'communicative rationality': all speech aimed towards mutual understanding takes place against a cultural 'preunderstanding' which differentiates between claims about what is true, claims about what is right, and claims about what is sincere (Habermas 1984, p. 100). Claims about natural facts fall in the first category, while claims about values and interests fall in the second and third. The latter are subject to negotiation, and when we discuss them, we are seeking to find a compromise that people can live with. Discussion of facts, however, consists of trying to demonstrate that something is true, and it presumes the goal of agreement about that truth. To arrive at an agreement about what is going on, for example in the environment, participants have to explain how they know what they say they know.

The tools and techniques of science are the Rolls Royce of explaining how you know what you know because of the commitment to radical transparency. This is not an ideal that is ever reached; even on a philosophical level, all that can be shown is that something has not yet been disproved, and in the day-to-day practice of doing science to support decision-making, it remains distant. Nevertheless, it is this methodological quality, not the truth of any particular demonstration, that makes science the best basis for developing a knowledge base for collective action. Because broad participation is needed both to mobilise information and for effective governance, striving for this ideal form of demonstration cannot be left only to scientists. Other groups must also participate in creating a knowledge base for adaptive collective action.

The elephant standing on the table when applying all this theory to fisheries, and especially to the EAFM, is scientific uncertainty. Because of uncertainty, the particular accuracy of any picture of the environment cannot be the basis of collective action. Many important facts have not yet been or perhaps can never be demonstrated to a degree adequate for decision-making. Many have argued, perhaps most systematically Funtowicz and Ravetz (1990), that uncertainty reinforces the need for broad participation. One reason is that many perspectives and disciplines are needed to judge the extent of the uncertainty and its implications for policy. Another is that uncertainty about facts is commonly a mechanism for undermining decisions, especially when uncertainties are ignored or left unaddressed.

How do science and participation mix, then, in a world where scientific legitimacy is often a function of who is involved in doing the science and where all scientific activity suffers from transparency-induced opacity? How should this broader participation in science be carried out when science in practice is a complex activity requiring extensive skill and knowledge? What roles should scientists play? What roles should others play? How does knowledge get translated into advice about what collective action to take? This book examines what has been learned about these questions through the work of the International Council for the Exploration of the Sea as it provides scientific advice for marine management in Europe.

Fisheries are a particularly good place to search for such lessons. This is not true because fisheries management is a shining example of institutions successfully adapting to environmental changes. Fisheries management has hardly been a success at sustaining fish stocks or otherwise protecting the marine environment, and among developed countries the European Union has been the least successful fisheries manager. Nor is it true because fisheries are easy to study from a human ecological viewpoint. Sparholt et al. (2007) in an historical analysis of European fisheries management over 60 years for 34 stocks could only find one completely clear link between a particular management measure and stock recovery. However, as a laboratory for human ecology, fisheries are still unparalleled. Many stakeholder[2] groups are interested in the subject, very often with amazing passion motivated by greed, nature worship and seemingly everything in between. The marine environment is difficult to observe, and scientific uncertainty is high. Fishers have a great deal of experience-based knowledge of the marine environment that is available to supplement, or to contradict, the scientists' research-based knowledge. Fish stocks are biologically diverse, and approaches to the management of exploited stocks must often be different. The stocks also respond relatively quickly to changes in management approach compared with other areas of environmental management, making it possible to learn the results of institutional changes fairly quickly. In fisheries, in short, there are lots of approaches and lots of opportunities for comparison between these approaches.

The above is perhaps enough of a general orientation to the perspectives that guided the creation of the book. However, there is one question, a basic assumption I am making, that needs to be addressed before getting to the substance of the book. The assumption I refer to is that science actually matters in environmental management.

1.2 Does science matter in politics?

Participation is really just a nicer word for politics. So a question worth asking is, how much science actually matters in political processes? Is it true, as Haas (2004, p. 587) claims, that 'knowledge can speak volumes to power'? Does science really influence policy? Do people pay attention to demonstrations of facts? Do they change their minds? These are important questions if one is using a communication-based approach to social institutions that begins with the assumption that communication not only matters, but that structuring mechanisms of communication is what institutions basically do (Habermas 1984, 1987; Wilson 2003). Frankly, the commonsense answer to these questions, in the European fisheries community anyway, is (a big) 'No'! In the face of this commonsense assumption, we need to examine both the theoretical arguments and the record of experience to find out if it does matter in respect to science and fisheries management in Europe.

To get at the theoretical arguments for why science matters in politics, I need to dig a little deeper into Communicative Systems Theory. CST begins with some insights about what is needed for people to make sense out of what they are saying to one another, i.e. the requirements of 'communicative rationality' (Habermas 1984). The idea is that the most basic requirement for sense-making in a conversation is the presupposition behind every statement that mutual understanding is a possible goal. As mentioned above, communicative rationality also involves the idea that discussions of facts, social phenomena like values and interests, and inner feelings involve different kinds of mutual understanding. For facts, a mutual understanding is about whether something is true or not, for social phenomena it is about whether something is right or not, and for an inner feeling it is about whether something is sincere or not. We are concerned in environmental management with facts, values and interests, and to be communicatively rational means orienting our attempts at mutual understanding towards truth in respect of facts and towards a fair outcome in respect of values and interests. An orientation towards truth assumes that the goal of the discussion is a consensus; an orientation towards fairness assumes that the goal of the discussion is a compromise. One way to grasp this is to think about how deciding to 'split the difference' is an honoured principle in resolving a negotiation about values and interests, while splitting the difference in order to come to an agreement about facts is to admit that you do not know what the facts are. The rationality of communication requires recognising and dealing with these different communicative orientations. The requirement that a communication be oriented towards mutual understanding is even more basic than being clear and logical, because it provides the rationale for clarity and logic. When you make sense, you phrase what you are saying in a way that is oriented towards convincing the other person of something – and that is what allows what you are saying to 'make sense'. Although for our purposes here 'making sense' is usually oriented towards a mutual understanding of factual conditions, the basic logic applies to other kinds of mutual understanding; 'making sense' may be oriented towards someone understanding that our description of an internal feeling is a sincere one. Humour, irony and some poetry, of course, do not make sense directly, nor are they supposed to, but they are still oriented indirectly towards a common understanding.

Starting with this insight, Habermas has identified some basic social conditions under which making sense is possible: that there is no manipulation involved in the communication and that anyone involved can raise a question about any claim being made (White 1988). This is not saying that people always behave in this rational fashion, but it does say that these criteria are empirically important because people use them every day to judge for themselves if a discussion is making sense. The links between communicative rationality and science are an important part of the theoretical discussion in Chapters 2 and 3 and have an important influence on the interpretation of the case study.

You know you have a social institution when you see repeating patterns of behaviour over a period of time. Institutions[3] simultaneously face two realities that are often in tension with one another. On the one hand, there is the competition and struggle over scarce material resources, ideology, power, etc. On the other hand, there is the need to coordinate behaviour so that it remains patterned. Even competition requires coordination. What lies underneath an institutionalised pattern of behaviour is an iterative process of mutual understanding. The basic process is informal. People see other people doing things and hear other people making statements. They interpret these actions through their own knowledge of institutions. This interpretation then forms the basis of their responding actions. The behaviour pattern – i.e. the institution – is thereby recreated. It is also marginally changed because one interpretation and response will never be exactly the same as another. In modern, developed societies this change is usually very marginal because in long-term institutions, people have invested a great deal of resources and skill-building effort in the assumption that the patterns are going to be stable, and they will therefore place a lot of effort in keeping them stable (Giddens 1984). In less long-term institutions the changes are likely to be less marginal and will themselves become part of competition and conflict. Different people's interpretations of institutions vie with one another, with limits on this usually set in modern society by the formal rules that make up the official description of the institution.

CST moves to the institutional level based on the observation that people have to be able to read and respond to signals in order to follow patterns. Some mutual understanding is needed, so institutions either have to have a mechanism to make some communicative rationality possible or they need mechanisms that short-cut that process but still arrive at some mutual understanding. In practice, institutions use mixtures of these communication mechanisms. Money and authority are both examples of just such mechanisms; they are pre-packaged mutual understandings that maintain patterns (Wilson 2003). But no institution, not even the most globalised market, functions for very long without making some use of communicative rationality. Such an institution would be on automatic pilot, incapable of any adaptation to its environment.

The bottom line is that institutions contain a functional imperative that forces people to try to reach mutual understanding, and this means trying to make sense when they communicate. Even the most manipulative liar or recalcitrant bureaucrat is eventually required to make sense if he or she wants to participate in functioning institutions. Institutional imperatives also contain strong pressures to distort communicative rationality, for example by trying to make things appear as natural facts that really are not, but that is another part of the story. For now it is enough to establish that there are coherent theoretical reasons to expect that people are going to seek to make sense, i.e. to phrase their assertions in ways that are oriented towards convincing other participants, in order to create mutual understanding. In environmental management, that often requires references to valid knowledge about nature, even when those facts do not fit easily with one's interests.

What about the empirical case? Does science actually have an impact on political processes and the policies that emerge from them? Clark et al.'s (2006) review of international scientific assessments does conclude that the majority of the assessments are not effective in influencing policy and that even the most influential assessments do not directly determine policy choices. What science does do, however, is influence the direction of the long-term development of an issue through such mechanisms as influencing the issue's visibility, the stakeholders who will take an interest, the way questions and objectives are framed, and the selection of management alternatives (see also van der Hove 2007). These mechanisms add up to a considerable long-term influence on the outcomes of political games. In some cases credible science can directly shift the discussion even in the most political of fora. Lenhard et al. (2006) describe an argument around the Intergovernmental Panel on Climate Change's Second Assessment Report during which sceptics from the political wing of OPEC withdrew their veto in response to climatologists who were able to show that they were using the accepted methods and standards of their field. It seems that an approach to policy that examines how and when a political process is able to make use of a common understanding of nature will tell us more than simply assuming that power and interests tell the whole tale (Haas 2004). The idea of analyzing institutions in terms of both power and communicative rationality makes sense.

So what about the common belief that the CFP does not pay attention to science? Scientists, fishers, and conservation activists repeatedly assert that the decision-makers pay no attention to scientific advice. Several times biologist colleagues have asked me to do a sociological study on why 'they always ignore our advice'. This idea that scientific advice is simply ignored in European fisheries appears in the press, often as plain common sense. The following example appeared in a recent news article in *Time* magazine as part of a very brief description of the European fisheries management:

> Q 1.1 The European Commission typically points to scientific evidence showing a collapse in key catches, and suggests that quotas should be slashed across the board. And just as predictably, individual governments within the EU ignore these proposals, and raise the quotas – usually around 50% higher (Cendrowicz 2007).

This statement was presented as a straight news fact while the following statement was attributed to a conservation activist:

> Q 1.2 Mocking scientific advice has become standard practice in the decisions made by the European fisheries ministers (Cendrowicz 2007).

Even ignoring the fact that individual governments do not set quotas and assuming that the author meant the Council of Ministers, the first 'news fact' is absurd in its characterisation of both scientific advice and the managers' response. The second quote is merely untrue unless we take the word 'mocking' to mean 'not following automatically'. This is not particularly surprising because a journalist who interviewed four or five random people from the fisheries management community in Europe, if his experience is anything like mine, could easily have heard something like this from every one of them. It reflects what has become common sense.

But this is a case where the common sense is way off the mark. Two scientists from the (supposedly ignored) Commission's fishery team have done an analysis of 436 records of scientific advice and policy result for fish stocks 'that are of principal interest to the European Union' between 1987 and 2005. They found that 'management decisions have been moderately responsive to ICES advice in setting TACs' (Patterson and Résimont 2007, p. 716). The normal pattern is that TACs move in the direction of the advice, but not as far as the advice recommends, which is what would be expected if the science is being considered, given that the economic and social considerations that the Council is weighing in the balance are all going to pull in the direction of more fishing. It is the Council's job to take other aspects of the fishery situation into consideration along with the scientific advice. The situation that *Time* magazine reports is the usual one – raising the quota 50% after a recommended cut – that corresponds to exactly one of the 436 instances. The basic finding is that the Council opts for more stability in catches than the direct application of ICES advice would allow (Patterson and Résimont 2007).

Science does matter, but it is not decisive because fisheries management in a democracy is fundamentally a political activity rather than a technical one. This political exercise revolves around how values, interests and facts are linked to one another. But facts have a special place in this triumvirate because their discussion assumes a goal of agreement rather than a goal of compromise. The key, then, is that the groups involved in fisheries management must be willing and able to explain in a transparent fashion how they know what they say they know. Science is rooted in this transparent accountability; indeed, scientists are the transparency experts. The scientists examined in this book have learned a great deal about how to resolve the paradoxes of transparency that make it so hard to develop effective science institutions. It is their experience that provides the substance of any lessons derived from this research.

1.3 The case: The International Council for the Exploration of the Sea

This book examines the institutions that provide the scientific advice for European fisheries management. This means mainly the Common Fisheries Policy in the form of the European Commission, but it also involves other regional management bodies and nations bordering the European Union, notably Norway which negotiates the division of very important fish stocks with the EU. The central focus of the study is the International Council for the Exploration of the Sea (ICES). Formally, ICES is a multilateral scientific organisation made up of 20 member countries. Informally, it is a network of approximately 1600 marine scientists, according to its website at www.ices.dk. ICES's charge is to coordinate and promote marine science, particularly in the North Atlantic. It has two main functions: one is the general promotion of quality marine science in the region, and the other is to provide specific scientific advice to a set of clients in response to their requests. The largest of these clients is the European Commission, particularly through DG MARE. The advice is used in large part to set total allowable catches (TAC) for commercial fish species of interest to the Common Fisheries Policy (CFP).

A helpful way to understand the institutional and historical development of forms of scientific advice for the CFP is what Holm and Nielsen (2004) term the TAC Machine. They outline the co-evolution of the problems that managers and politicians needed the advice to solve – mainly the need for TACs in order to apportion the fish among the various polities and stakeholders – and the family of stock assessment models that has been developed to solve these problems. Their basic argument is that the CFP has been an institutional success in spite of having done a poor job of managing fisheries, because it solves important political problems around the division of fish resources among EU member states.

The CFP is one of the few areas where member states have given EU institutions full decision-making power. The Council of Ministers makes decisions about the management of European fisheries resources beyond 12 nautical miles from each country's shoreline. Arriving at this agreement required the creation of the rule of 'relative stability' under which the EU cannot change the historical share that member state fleets have enjoyed for various fish stocks. The main job of the CFP has been to conserve fish stocks while allocating the allowable catch among member states following the relative stability rule.

This political requirement fits very nicely with quotas that provide a mechanism for dividing up the fish. The quotas in turn are generated by a group of age-structured stock assessment models. What age-structured models do is follow groups of fish in the same 'age class', i.e. fish that are born the same year, through their life spans while trying to keep track of how many fish of a particular age live to become one year older. These models produce, in principle, an estimate of how many fish can be sustainably removed from a fish stock each year. They require a massive amount of scientific effort and data, and therefore a huge infrastructure has grown up around these annual fish stock assessments. The result is a mutually reinforcing system of political and scientific institutions that works well in terms of EU politics, but much less well in terms of sustainability (Holm and Nielsen 2004).

The scientific advice system is being asked to change. Several priorities are emanating from ICES clients, who are themselves responding to shifts in European environmental politics. The CFP has failed in terms of sustainability. Indeed, Sparholt et al.'s (2007) analysis of fish stocks in the ICES area found that the implementation of the CFP has had no real impact on the condition of fish stocks. The pelagic stocks that are in relatively good condition currently were already being fished at a moderate level before the CFP was implemented in 1983. More tragically, the high fishing pressure on the demersal stocks that are currently in very poor shape simply continued. Both the fishing industry and conservation groups are deeply dissatisfied with the situation. The industry is pushing for more long-term management plans that allow rational business planning. The conservation groups want to reduce fishing pressure, implement marine protected areas and bring about an ecosystem-based approach to fisheries management (EAFM). The European Union's developing Marine Strategy strongly reflects these concerns, particularly the EAFM. Both long-term management plans and the EAFM present new and extremely complex problems for scientific advice; the TAC Machine will soon be as politically inadequate as it is environmentally inadequate. How it can be changed, and what it can be changed into, is the challenge that European marine scientists are trying to meet.

1.4 Research methods

The qualitative aspects of case study research include in-depth interviews, observation of various public and private meetings, and review of many documents. Notes from observations, informal interviews, and original documents were analysed using NUD*IST textual data analysis software. A detailed description of this process as it was applied to the particular qualitative research goal of identifying the main themes in the discussions around a reorganisation of the ICES Advisory Programme is found in Section 7.3.

This research also makes extensive use of the anthropological method of participant observation. Part of the time I was researching and writing this book, I was also serving as the Chair of the ICES Working Group on Fisheries Systems. This not only means that I was a participant in the system I was studying; the assigning of a sociologist to such a role is a reflection of the opening to new perspectives that characterises recent changes in ICES. As any participant observation would, I am sure that this introduces some biases. Indeed, my fisheries science colleagues often tease me about whether I am being an observer or a participant at any given moment. I cannot be aware of the biases that this participation introduces to my analysis – although I do admit to a general admiration of fisheries scientists – so I must be content with cautioning the reader that it is present.

This book is based mainly on research that was supported by two European research projects. The Policy and Knowledge in Fisheries Management (PKFM) project, which ran from 2003 through 2005, supported the detailed observation of five scientific deliberations within the ICES system and two meetings overseeing such deliberations.[4] The PKFM project also allowed us to carry out 29 formal interviews with fisheries scientists or close observers of the fisheries science process. The Scientific Advice for Fisheries Management at Multiple Scales (SAFMAMS) project ran from 2005 through 2008. In addition to allowing the observation of ten more ICES meetings[5] and six more in-depth interviews, the SAFMAMS project supported the observation of the March 2005 meeting of the North Sea Commission Fisheries Partnership and the May 2005 meeting of the North Sea RAC Spatial Planning Working Group. In addition, the SAFMAMS project supported nine workshops at various geographical levels on developing scientific advice for fisheries management, the results of which have often contributed to this case study.

In addition, a large number of documents were reviewed. They included ICES and STECF reports, the Memorandums of Understanding between ICES and DG MARE, and a number of internal ICES documents, mainly those distributed in conjunction with the meetings being observed. In the end the access to both meetings and documents that ICES allowed for this research was extensive, and I am very grateful for that. It is ICES's stated policy that quotations from any of their expert group documents must be cleared ahead of time, and this certainly applies to some of the even less public documents that I was given access to. Therefore, the ICES Director General has reviewed a draft of this manuscript. However, he did not request any changes.

The quantitative methodology used in this case was a random sample survey of European marine fisheries scientists employed in the countries around the North Sea, namely Denmark, Norway, Sweden, Belgium, France, Germany, the Netherlands, Ireland, United Kingdom and the Faroe Islands. A total of 465 valid responses were received. The sample size was 900, which indicates a response rate of 51.7% – a relatively high response rate for a non-telephone survey. The survey procedures and methodologies are described in Appendix 1.

1.5 Plan of the book

This book has two basic parts. The first third, Chapters 1 through 3, is mainly theoretical and is offered to provide background on the sociological concepts used in the case study analysis. The present chapter provides a general theoretical and methodological orientation. The second and third chapters cover most of the relevant theory. They are basically selective reviews of studies and theoretical insights drawn from a wide number of areas in which science and policy-making are joined.

Chapter 2 begins by reviewing three general challenges we face in the relationship between science and the rest of society. The first challenge is the 'inflation' of the science boundary that results from constant institutional pressure to define more and more issues as being capable of resolution using scientific methods. The second challenge is the uneasy relationship between the scientific culture and that of decision-makers, which becomes particularly sharp when scientific ideas about what constitutes evidence clashes with legal ideas about evidence. The last challenge is the uncertainty that not only pervades our knowledge of the environment but is also a general condition of modern society with far-reaching consequences. The chapter concludes with a review of three perspectives on the changing relationship between science and the wider society that have proven useful in research on fisheries science.

Chapter 3 focuses on the narrower question of general experience with the production of science for policy advice. A large number of studies of how and when science is effective in aiding policy development have been carried out in many different arenas over the last two decades, especially in respect to environmental protection. Out of these studies have come three qualities of science that tend to improve its effectiveness for policy. These are credibility, legitimacy and saliency. Most of Chapter 3 is spent exploring these three qualities. The chapter then turns to a discussion of the boundary between science and non-science and ways that have been found to work across that boundary. This is followed by an explanation of the idea of the paradoxes of transparency. Chapter 3 concludes with a summary of the main theoretical ideas.

The remainder of the book, Chapters 4 through 7, presents the case study. Chapter 4 orients the reader to the institutions and issues of scientific advice for fisheries management in Europe, particularly the role played by the International Council for the Exploration of the Sea (ICES), which is the central institution in the case study. The demands for scientific advice that various clients make on ICES, these clients being the European Commission and various other multilateral marine management bodies, are one of two major forces determining the content of the knowledge base for European marine management. The other major force is the interests and plans of the European marine scientists themselves, and these interests and plans are also played out within ICES to an important degree. These two statements, however, are true for European-level activities. The same two forces operate at national levels through marine science laboratories owned by national governments, which I will refer to collectively as National Fisheries Institutes (NFIs). The two levels are deeply intertwined. Most funding for ongoing, day-to-day 'turning the crank' on fish stock assessment comes from the NFIs and their respective member state ministries. Much of this money is actually expended through the ICES system. The NFIs relate to each other through ICES, and the directors of these NFIs are the single most influential group in the governance of ICES. They control the bulk of the funds used for ICES activities. Most forward-looking research funding for new approaches comes either directly from the EU or from matching grants made by member state governments to European-level research projects.

Chapter 5 focuses on the experiences of the individual scientists who work within the advice production process. It is Chapter 5 that draws the most heavily on a formal attitude survey of fisheries scientists in northern Europe.[6] Much of the information reported in Chapter 5 is based on comparisons of the survey responses of fisheries scientists involved in the advisory process with those who are less involved. A central finding is that these scientists find the advisory production process frustrating and demoralising because it both places tremendous demands on their professional lives and uses their work in ways that do not meet their expectations of what science should be about.

Chapter 6 returns in a very direct way to the question of adaptation that is so close to my heart. One of the central challenges that ICES is facing is a demand stemming from multiple clients to produce advice for an ecosystem approach to fisheries management (EAFM). It would be difficult to pull together good scientific information for an EAFM even if it was clear what such information should consist of, but the practical application of the EAFM is unclear from the perspective of both the knowledge base and implementation. ICES has moved ahead as best it can under these circumstances, and the institutional issues they are confronting in the process provide valuable lessons about how you organise an adaptive learning process.

Chapter 7 completes the case study with the story of how ICES has reorganised itself to meet the complex demands of providing scientific advice for the EAFM as well as a set of other demands related to the practical implementation of fisheries management. A central rubric of this reorganisation has been an integration of diverse kinds of knowledge that has presented both technical and institutional challenges. Tracing this reorganisation process allows the case study to examine in depth the political dimensions of creating an effective knowledge base for policy.

Chapter 8 draws together a set of conclusions about what lesson this case might provide, in light of the theoretical issues outlined in the first three chapters, for how we organise ourselves to develop a knowledge base for environmental management.

This book was a long time coming and seeks to bring together a great deal of sometimes disparate research. The inadequacies of the attempt will no doubt become clear to the reader. If the book is able to make a contribution, most of the credit by far will belong to the fisheries scientists whose activities and insights I am recording and presenting.

2. Some general theoretical guides for understanding the role of science in society

2.1 Three challenges in science and society

2.1.1 Science and culture: Pressures to inflate the science boundary

Culture and science are bound together in ways we are often barely conscious of. While the more radical reflections on science and society may not have much practical use, they remind us of the weight of the cultural baggage on the science we are trying to harness to practical ends. To me, the most illuminating of these radical reflections comes from the Frankfurt School. This group of thinkers began to blend sociology and philosophy in the 1930s and 1940s to explain the rise of fascism. Communicative Systems Theory (CST) arose from this tradition a generation later. For them, science is the ultimate expression of 'instrumental rationality', i.e. rationality focussed on achieving ends. Technical considerations about *how* to do things are pushing out moral and practical considerations about *why* we do things. The *why* question has become a private matter; our collective responsibility is to provide each other with the services and tools needed to reach our individual goals. Ecological degradation is one place where we are being forced to bring back the collective *why*, so it is perhaps not a co-incidence that questions about science and participation arise in response to environmental problems.

Science is powerful rhetoric. Technical arguments are more convincing than soft appeals to values because we have so fully internalised the difference between negotiation and demonstration, while often missing the practical and rhetorical links between them. Pelletier et al. (2000) did a study using before and after attitude measurements of agriculture stakeholders involved in a participatory action conference. Shifts in attitude as a result of the discussion were much stronger in response to technical arguments than value-based arguments. The presentation of something as 'science' can be and is used to silence people's concerns (Beck 1992; Irwin 1995). Kaminstein (1996), for example, analyzed responses to a public meeting in which an agency was presenting information about a toxic waste site in the United States. He found that the technical and bland language calmed the audience. When people did express emotions and worries, they seemed very out of place. The community people reported feelings of frustration afterwards. They experienced the officials trying to be friendly and positive as mockery, and compliments such as 'that is a good question' made them feel less able to complain, dispute and disagree over what was in the end a political rather than a technical question.

The concept of the science boundary, the boundary between what is and is not scientific knowledge and who is and is not a scientist, is an important concept in STS (Gieryn 1983). People make use of this rhetorical power of science. They try to present their values and interests as technical requirements, undermining the credibility of science in general when they do so. This phenomenon, then, might be called 'inflating' the science boundary. Habermas (1984) warns against a tendency, rooted in a desire for control, to try to redefine culture phenomena into technical ones. When this happens, social relationships are made to appear as natural and inevitable, rather than as the concrete results of real decisions made by real people who could have chosen another route. Herbert Marcuse (1964) points out that instrumental rationality is built into the very heart of natural science; because the methods of hypothesis and experiment consist of prediction and manipulation, and hence the domination of nature, science is inescapably linked to control. Suggesting that something is the appropriate object of a scientific analysis casts that object into a particular and subordinate cultural role. This casting can be and is done inappropriately to the detriment of both science and policy.

One point CST makes is that bureaucracies and markets have particularly strong institutional imperatives to make the subjective appear objective, often by getting things stamped as 'scientific' facts. The argument is that some kinds of institutional coordination require 'empirically motivated ties' that are based on objective facts, rather than 'rationally motivated trust' based on social relationships (Habermas 1987). The empirical ties are needed by institutions that rely on pressure to create compliant behaviour, rather than on convincing people to behave a certain way (this distinction is discussed in more detail in Section 3.3.4). Coercive coordination is needed for institutions operating on large scales because it makes behaviour predictable. Markets use market pressures that take final form as take-it-or-leave-it offers, while bureaucracies use legal authority backed up by sanctions. Neither requires the rich and nuanced communication about choices that is found in institutions that coordinate action by convincing people that something is the case. Indeed, that would be impossible because rich communication on large scales would be too costly.

To make the pressure work, such institutions have to use decision rules that make reference to something 'objective' so such decision rules require material facts. Social psychologists have long recognised the different behavioural implications of 'facts' versus 'social attitudes', the difference being whether or not it is possible in principle to check some objective reality (Festinger et al. 1950). This need is clear in respect to bureaucracies that have to meet objective legal standards in decision-making, though – as discussed in the next section – 'objective' when it comes to legal standards means something subtly but importantly different from the scientific idea of 'objective'.

The functional differences between facts and attitudes are also important for how markets function, although this is much less studied because it is so far outside what can be accommodated in standard economic theory. It was shown in one study of an African labour market that natural risks affected labour prices exactly as a market model would predict, i.e. people paid to reduce risk. Risk arising from social relationships, however, such as increased danger of being cheated when working with people who are not kin or not from the same ethnic group, did the opposite of what the market model would predict. People paid more money while taking on greater risks. This was found in data for kinship and, more strongly, ethnicity (Wilson 1998).

The bottom line is that institutions trying to coordinate behaviour across large scales have to be able to make operational references to 'indisputable facts', while having very limited communicative resources for convincing people that these facts are true. The more questions that can be defined as issues of fact with objectively true answers, the easier it is for such institutions to function because they are able to bring more contingencies under their direct control. Bureaucrats are continually trying to expand the arena of questions that can be answered by 'scientific facts' into areas governed by moral and practical rather than technical rationality.

Scientists, however, have their own reasons to resist these trends. Science depends on 'rationally motivated trust' to coordinate behaviour (Habermas 1987, pp. 182-184). Scientists must convince other scientists that something is true and make use of rich and complex communications to do so. This requires focussing on questions where a rationally motivated factual truth (Habermas 1987, p. 184) can be reached through processes of consensus, characterised by disinterest and scepticism and oriented around universal criteria such as precise definitions, the falsification of hypotheses and replicability (Merton 1968b). Only this narrowness of focus makes possible the internal trust of both people and results that enables scientific inference (Barnes et al. 1996) because it defines the criteria and extent of such trust. Therefore, scientists resist external pressures to change the subject matter and the operating modes of science. An extended example of both bureaucratic pressure to inflate the science boundary and the fisheries scientists' attempts to accommodate this where they could and resist it when it went too far is the discussion of mixed-fishery management in Section 6.1.

The temptation to try to change political, social or cultural phenomena into technical ones is always present. Environmental management requires us to address social behaviour, and so we search for techniques to do so. Management involves manipulation, and it is out of this tension that the question of governance arises when democratic societies seek to address social and environmental problems. This problem is exacerbated when actors seek to obscure rather than clarify the distinction between technical and cultural phenomena. The usual motivation for this is to make a policy choice appear as a technical necessity. As one scientist involved in environmental management put it 'If ... a manager suggests that a decision is based solely on scientifically-derived biological considerations, the manager either misunderstands the nature of science ... or is deliberately trying to disguise ... a value judgement' (Decker et al. 1991, quoted in Minnia and McPeake 2001).

2.1.2 Clashing cultures and notions of evidence

Science for legal and policy uses places demands on science and scientists that challenge their modes of operation. Policy processes use different standards of evidence and burdens of proof than science. Scientists want to see that there is a small probability of a null hypothesis, while policymakers are more concerned with the costs of being wrong (Kinzig et al. 2003). As an exaggerated but illustrative example, a policymaker would hardly want to reject a potential cure for cancer because there was only an 89% chance that it would work.

Managers employ scientists, but they see things through different cultural glasses. Cullen (1990) observed interactions between water use planners and limnologists and found many differences in expectations and mutual perception. The managers viewed limnology as a curative practice while scientists saw it as preventative. Planners saw scientists as unable to agree on a conclusion and driven by a need to publish rather than getting a planning process done. They also thought the scientists were poor communicators. The scientists felt isolated and underutilised, they saw the planners as unable to interpret data or even find information, indeed as so ignorant that they did not know what they did not know. They also thought the planners were poor communicators. Cohen et al. (2001) did 55 semi-structured interviews with scientists working in eight public-sector research science institutions in the UK. A majority of their respondents objected to the idea of being accountable to politicians and managers whose purposes they saw as being at odds with science.

Salter (1988) explores in some detail what happens to scientific practice when it is mandated as part of a policy process. The burden is placed on the scientist to produce work that is sufficiently credible, salient and legitimate to support the policy. Policymakers want science that is intelligible to non-scientific audiences, and in doing so represents a clear body of evidence and appears to be rational. The ideal it is meant to project is that of something free of value judgements, using clear methods that produce credible results. At best, it is characterised by open debate, anonymous peer review, and academic publication. In addition to projecting this sterling public image, it must present the policymakers with clear policy choices. Of course, as Salter points out, real science in support of policy conforms to none of these ideals. It makes moral dilemmas explicit, produces conflicting results that cannot be resolved by further studies, and is often seen as corrupt. The policymakers desire transparent knowledge to facilitate and justify their choices, but the actual transparency just reveals more uncertainty.

Where these differences really become apparent is when science is drawn into legal proceedings. As Smith and Wynne (1989) argue, legal institutions have their own ways of defining what counts as fact, and they are not the ways found in science. In court it is the law that decides what the factual question is that the scientist must answer. They further argue that 'adequate evidence' is fundamentally problematic in courts because of the unremitting scepticism. In Latour's (1987) terminology, opposing lawyers always push scientific facts back towards the conditions of their production and expose the role that scientists' tacit assumptions, experimental skills, and professional judgements played in the production of knowledge. Within the scientific community these perfectly normal aspects of scientific work are handled by expectations of trust in intellectual integrity and the resulting importance of scientific reputations (Barnes et al. 1996). In court they can be portrayed by opposing counsel as simply unprofessional.

2.1.3 Uncertainty

In the 1980s, prominent social theorists began to argue that the West has become a 'risk society' in which anxiety over uncertainty is the driving force in the development of both the self (Giddens 1991) and institutions (Beck 1992), and that science is the key institution that we look to in order to relieve these anxieties. We now live in a 'post-modern' society characterised by inescapable uncertainty due to both information overload and the loss of the ability to trust traditional sources of valid knowledge. Science has become an arena where disagreements over how to respond to risk and uncertainty are played out (Irwin 1995). People simultaneously look to science as an institution for answers, while viewing individual scientists and their results with scepticism. The desire for scientific answers is great, while the automatic authority of science is a thing of the past. Funtowicz and Ravetz (1992, p. 251) tell us that in any policy arena where the stakes are high, 'the political manipulation of uncertainty is now the focus of any relevant epistemology'. As research results presented in this volume strongly illustrate, scientists cast in this central role as arbiters of uncertainty have their own identities as scientists, challenged in ways that have a direct impact on their morale.

Any number of categories of that phenomenon for which I am using the generic term 'scientific uncertainty' has been proposed. Harwood and Stokes (2003), for example, suggest a fairly simple and concrete classification of uncertainty from their direct experience with fisheries management. They suggest that uncertainty in fisheries consists of:

- process uncertainty, which is also called natural variation or stochasticity;
- observation error, in which data input is faulty;
- model error, in which the model does not accurately specify the causal processes; and
- implementation error, in which the resulting measures are not translated into actual behaviour in the way the models had foreseen.

To this list Kell et al. (2005) add error related to the estimation of parameters as a form of model error, as distinct from that related to accurately reflecting system dynamics. Harwood and Stokes (2003) also contrast epistemic uncertainty, which is related to things that can be measured, and linguistic uncertainty, which is uncertainty in the language used to describe or classify desired states.

These categories come from fisheries scientists and reflect the steps of fish stock assessment estimation and its communication. We can also classify uncertainty in terms of its degree. Wynne (1992) suggests that the term:

- 'risk' be used when behaviour is known and outcomes can be assigned probabilities;
- 'uncertainty' be used when important system parameters are known, but not their probability distributions;
- 'ignorance' be used when what is not known is not known; and
- 'indeterminacy' be used when causal chains, networks or processes are open and thus defy prediction.

Funtowicz and Ravetz (1990) take a similar approach to classifying the uncertainty of numbers, but they also link the degree of uncertainty to scientific capabilities. They begin by arguing that there are scientific tools available to deal with three kinds of uncertainty. The first deals with measurement errors and assesses the reliability of instruments. The second deals with the probabilities that arise through the combinations of different variables. The third deals with the error terms related to statistical sampling. They relate these three types of tools to what they call the three limits of scientific knowledge: errors in measurement made by real instruments over and above that which can be handled by reliability testing; the randomness that stems from the limits to causal understanding in the real world; and the correspondence between descriptive objects and the reality to which they refer. Funtowicz and Ravetz (1990) conclude this analysis by proposing three kinds of uncertainty:

– ‘inexactness’, which is usually expressed by numeric confidence limits and reflects the spread found in any data set;
– ‘unreliability’, which is expressed by the level of the confidence to be placed around a statement and is often expressed by semi-qualitative statements such as ‘conservative by a factor of 10’; and
– ‘border with ignorance’, which can only be addressed by describing the state of the art that produced the quantity.

Technical responses can be made to improve exactness, and for this standard routines, such as statistical techniques, are usually adequate. Unreliability is addressed in the search for improved methodologies. ‘Border with ignorance’, they argue, calls for significant shifts in the way that science is done and requires the support of an extended peer community of other experts, with practical, experience-based knowledge being a possible source of such expertise, including both user groups and policymakers (Funtowicz and Ravetz 1990).

Scientists involved in supporting policy are constantly required to deal with uncertainty. This is especially problematic at the border with ignorance because this moves them beyond their training and even their understanding of what it means to do ‘science’. This can mean a direct challenge to their professional identities and conflict with their scientific standards, values, or ideals. Our research with fisheries scientists in Europe found that many of them experience being asked to play a difficult role under sometimes trying conditions and then having the results of these efforts pulled out of the cultural context that produced it, in other words from the background understanding that makes the results ‘science’ in the eyes of the scientists.

Scientists look for ways to respond to these challenges while maintaining their self-image of doing quality science. One basic choice is that when formulating scientific advice they can choose to simplify the uncertainty, or they can choose to emphasise the uncertainty. Both of these involve trade-offs. If they simplify the uncertainty, the caveats they offer may be ignored; if they emphasise the uncertainty their advice may be ignored entirely (Harwood and Stokes 2003).

Shackley and Wynne (1996) review some of the strategies that scientists have used in their presentation of uncertainty in the context of global climate change. One approach they term the ‘clarification and management of uncertainty’. Here scientists are selective about the uncertainty they are willing to communicate to policymakers, and understandings within the scientific community, such as a general agreement about the precautionary approach or other policy directions, reduce the probability that other experts will challenge these selective reports.

The second approach is to ‘do more research to reduce the uncertainty’. This is in some ways an obvious response, but it has also been criticised as a ‘devil’s pact’. Policymakers are able to avoid changes, while scientists use uncertainty to justify research budgets. Shackley and Wynne (1996) argue that policymakers like ambiguity because it shields their authority. They use the reduction of uncertainty as a rhetorical device to proceed with current knowledge, while the scientists use it both to get funding and because areas on policy uncertainty are also areas where they find interesting research questions. In the area of global climate change, for example, space agencies are in search of new tasks while governments want to postpone action and therefore welcome uncertainties. Governments want to fund more research rather than change energy policies; scientists are happy to play up uncertainties to keep funding coming. Policy becoming more science-dependent reflects a convergence of interests between opponents to policy change and the scientific bureaucracy (Boehmer-Christiansen 1994).

Shackley and Wynne (1996) call the third approach the 'transformation of uncertainty'. The phenomena Wynne (1993) terms indeterminacy is recast as uncertainty while those involving ignorance are recast as risk. This makes things appear more tractable to the managers while helping the scientists to maintain authority. The extreme application of this strategy they term the 'condensation of uncertainty'. In this approach all the uncertainty is collapsed into one category, in their example this is risk, and the other forms are treated as superfluous in communications outside the scientific community. Degnbol (2003) argues that this basic error was made in fisheries science when they first began to address the precautionary approach. Uncertainty was treated as error that could be estimated and used to support decision rules built around limit reference points. This approach reflected a perceived need for clear communication of the content and implication of scientific advice, but it was an approach to the precautionary principle that excluded too much of the other forms of uncertainty that decision-makers needed to be cautious about.

Using Wynne's (1993) categories, Harremoes et al. (2001) argue that it is ignorance, not risk, which is the main issue that precautionary decision-making must address. They argue for the benefits of casting a wide net for knowledge to address ignorance by reducing obstacles to interdisciplinary learning and making use of lay and local knowledge. The white-coated expert is passing away, from the perspective of both what is being called for to improve the use of science for decision-making and what is observed to actually be happening to the day-to-day practice of science. The shifts are rooted in how to handle the problem of uncertainty. Funtowicz and Ravetz (1992) argue that addressing the limits of ignorance requires skills and practice in the form of a 'learned art' that resembles the activity of a consultant more than that of a research scientist.

The uncertainty of the 'risk society' appears in fisheries science to a greater extent, perhaps, than in other areas dealing with more easily observable phenomena. It has become a key, perhaps the key, principle in the organisation of fisheries science institutions. The discussion of the changes in ICES in Chapter 7 outlines how they are developing a form of 'consistency' that applies the standard questions of methodological reliability to social processes. This is what I refer to as Type E consistency in Section 7.3.1; it is consistency in the linkage of scientific methods to advisory problems. The need for this kind of consistency is rooted in scientific uncertainty.

2.2 Three perspectives on science and decision-making processes

Almost all observers of the present state of science are seeing profound changes in scientists' roles, based on different ways of interacting with decision-making processes. These observers also see some different things happening, or at least they place considerably different emphases on the changes they think are important. In the remainder of this chapter, I will review three of the more prominent approaches. I consider them all useful theories because in my research on European fisheries scientists I have seen the patterns that each one would predict, and all of them have improved my understanding. The first, called 'Mode Two Science', sees science taking on a much more subservient role to other social institutions than it has in the past. Some of the people taking the Mode Two approach celebrate this shift, while others see cause for worry. The second perspective, 'Epistemic Communities', sees scientists much more on top of things, as equal partners or even drivers in policy changes, but only when they have a strong scientific consensus. The third perspective is called 'Post-Normal Science' and is more of a philosophical reflection on how science can be the most helpful.

2.2.1 Mode Two science

Mode Two science is most closely associated with Michael Gibbons. He suggests that science is operating in a new mode that is trans-disciplinary, characterised by shifting rather than permanent institutions, and constantly moving back and forth between the fundamental and the applied (Gibbons et al. 1994). The driving force here is that society has learned to speak back to science. Society is transforming science by demanding that it respond to specific contexts where innovations require 'trans-disciplinary' efforts. 'The epistemological core of science has, over time, become crowded with norms and practices that cannot be reduced easily to a single generic methodology, or more broadly, to privileged cultures of scientific inquiry' (Gibbons 1999, p. 83).

Science is shifting from a search for truth to a more pragmatic aim of providing a provisional empirical understanding of the world that works in a practical sense. Reliability has been redefined as what works (Gibbons 1999). Scientists have had to learn to fit their research agendas into the agendas of funding agencies and firms (Gibbons et al. 1994). Many Mode

Two authors seem to shift between offering explanations of empirical changes in science institutions and advocating their encouragement. This advocacy seems driven by the idea that science has been operating as an unaccountable elite, and the changes pointed to by the Mode Two approach are a timely corrective (Elam and Glimell 2004).

Scientific labour has become more like a commodity, driven by contracts offered through tenders that deal more with scientific person-hours than with actual scientists (Waterton 2005). Managers and funding agencies have made scientists into interchangeable units of labour. This is very different from the ways they used to be employed when they were thought of more as creative intellectuals. They are also more strongly subjected to social accountability through such things as greater concern with research misconduct, data availability, conflicts of interest, human subjects research, and cultural limits being placed on the pursuit of knowledge (Guston 2001a).

Mode Two raises serious questions about what is happening with scientific quality control as the current peer review system is closely associated with established disciplines. Gibbons et al. (1994) argue that quality control has become more localised and rooted in the institutions where the research is taking place. Mode Two science grows and expands in a different way than did 'Mode One' science, i.e. scientists fitting the classical understanding of independent intellectual pioneers organised in disciplines. This new form of much less disciplinary expansion makes the application of widely accepted standards in quality control much more difficult. More intensive communication has arisen, between practitioners and between science and society, as people seek to address issues of quality (Gibbons et al. 1994). How adequate this communication is in ensuring quality is questionable, especially as universities as well as the private sector are now crowded with researchers with financial stakes in research outcomes (Guston 2001a).

An important distinction is the one between multi-disciplinary approaches, where scientists from different disciplines work together as a team on a particular problem, and trans-disciplinary approaches where scientists seek to create new questions and methods that draw upon many disciplines. Mode Two scholars see trans-disciplinary work becoming more common, and many celebrate this shift. But trans-disciplinary science is another source of quality control challenges. Cohen et al.'s (2001) survey (p. 31) found a consensus that discipline-based approaches could not deal with the complex environmental issues that were the central concern of the organisations where the scientists they studied were working. Disciplinary standards for testing new knowledge are no longer adequate. New kinds of standards have to be developed as part of the research process. External quality control focuses on the procedural mechanisms, rather than taking the form of peer reviews, and the research process itself becomes the object of evaluation (Guggenheim 2006).

The transition to a consumer-contractor relationship has been the 're-form' which has had the biggest impact on work and organisation. This includes new structures of accountability that replace a collegial environment of trust (Cohen et al. 2001). It is in these structures that new forms of quality control arise. Guggenheim (2006) studied 20 environmental consulting companies, a well-developed form of Mode Two Science. The tendencies he found are startling but will likely not be nearly as well developed in more traditional science institutions such as universities, non-profit research organisations, and public sector research institutions. However, in the EU context, research projects organised around time sheets and project deliverables that are subject to limited peer review are increasingly important in these institutions as well.

In the companies Guggenheim (2006) studied, the scientists have no time or incentives to write journal articles, and traditional mechanisms of peer review do not apply. The quality control mechanisms they do use are mainly internal – quality management systems (QMS) and time sheets. They also have supervisory oversight from clients and universities. Because the companies are not organisationally bound to a traditional scientific institution, they have to be monitored by 'real' scientists.

The internal quality control mechanisms have the greatest impact on how science is done. The QMS are quite the opposite of a requirement to use transparent methods. They are proprietary, secret and aimed primarily at singling out the company from their competitors. They are based on internationally recognised variables but also on the company's own standards in respect to these variables. They are usually formalisations of previously existing rules. The companies keep them secret because they believe that it is their organisational procedures which distinguish them from their competitors. Their real accountability is in the usefulness of the actual products. This is where reputations are built (Guggenheim 2006).

Time sheets are the main mechanisms used to coordinate projects and the key expression of the reduction of scientists to interchangeable units of labour. The time sheet approach has a strong impact on science because the 'interchangeable units' work on parts of projects, rather than on the intellectual object of the research (Guggenheim 2006). In Cohen et al.'s (2001) survey, scientists working under contract arrangements reported that they missed the opportunity to focus on one question for a sustained period of time.

This new way of producing knowledge replaces quality control based on reviews of results with quality control based on procedures and processes. This reinforces the Mode Two move away from the search for general knowledge to answers to specific practical questions. The scientific contribution is judged only with respect to its effectiveness in pleasing a client by solving a problem, rather than with respect to the development of theory within a scientific discipline in order to achieve a common and expanding body of knowledge. These are 'non-scientific' mechanisms in the sense that the quality control is shifted to agents and mechanisms outside of scientific organisations (Guggenheim 2006) and even outside the scientific community.

Recent scholarship has challenged some of the empirical arguments underpinning the Mode Two approach. One thread suggests that the frequency and importance of trans-disciplinary research have been exaggerated. Universities, industry and the government remain highly differentiated, and there are strong structural reasons why the current basic division of labour between the three will continue (Shinn 2002). Indeed, some of the ground that Mode Two is argued to have gained in respect of disciplinary science may even be lost. One piece of evidence for this is provided by a study of the recent history of science funding in Sweden. A number of political challenges to the value of funding independent basic science arose in the early 1990s. A special fund was set up to support work addressing specific problems of concern to industry and environmental regulation. These funds grew to the point where they were seen as a challenge to the university establishment. The universities were able to reassert a commitment to basic research activity within national funding (Elam and Glimell 2004). Nevertheless, as will be seen in later chapters, many of the trends described by the Mode Two approach are present in fisheries science as it is carried out in Europe.

2.2.2 Epistemic communities

Peter Haas (1989) developed the concept of the 'epistemic community' within the discipline of international relations as a way to try to explain the successful emergence of the Mediterranean Action Plan, a pollution control regime around the Mediterranean Sea. He took the term from the philosophy of science where it was used to mean scientists who share certain assumptions about what questions are worth asking. He argued that the Mediterranean Agreement came about because of the existence of an ecologically oriented epistemic community, made up mainly of people working in the environmental ministries of the various countries. They shaped their governments' policies, hired people who thought like them and gained international support (Haas 1989).

The idea of the epistemic community has proven useful to many observers of science-based international management regimes. Haas (1992a, p. 3) defines an epistemic community as:

> Q 2.1 A network of professionals with recognised expertise and competence in a particular domain and with authoritative claim to policy-relevant knowledge within that domain or issue-area. Although an epistemic community may consist of professionals from a variety of disciplines and backgrounds they have:

(1) a shared set of normative and principled beliefs, which provide a value-based rationale for the social action of community members;
(2) shared causal beliefs, which are derived from their analysis of practices leading or contributing to a central set of problems in their domain and which then serve as the basis for elucidating the multiple linkages between possible policy actions and desired outcomes;
(3) shared notions of validity – that is, inter-subjective, internally defined criteria for weighing and validating knowledge in the domain of their expertise; and
(4) a common policy enterprise – that is, a set of common practices associated with a set of problems to which their professional competence is directed, presumably out of the conviction that human welfare will be enhanced as a consequence.

Epistemic communities are a particular type of policy network characterised by this general agreement. Members of an epistemic community share a strong normative orientation (Haas 1992a). Epistemic communities are an example of the 'international civil society' where people divide their allegiance between domestic constituencies and international peer groups (Engles et al. 2006). Haas (1992b) argues that the success of the Montreal Protocol on Substances That Deplete the Ozone Layer can be attributed to an epistemic community. The first countries to actively encourage control were those in which both the epistemic community and a tradition of pro-environmental sentiment were strong. Once channels between other countries' national administrations were established and the epistemic community broadened, then the other countries began to support action. His analysis found that it was these contacts that made the real difference, rather than public opinion or the actions of environmental NGOs. These things only became important later in the process after government regulations had been introduced. Interestingly, for Haas (1992b) the key actor was the DuPont Corporation, which broke ranks with other chemical companies in an act that was critically important to the momentum for international action. Haas (1992b) argues that the difference was that key decision-makers on this issue at DuPont were all chemists who modified their positions in reaction to advances in scientific understanding.

The epistemic community approach takes a more deferential attitude towards science than is usually found in STS. The effectiveness of the scientific community in international regime formation is rooted in the ability of the scientists to reach a consensus and to overcome their natural inclination to extreme caution. These characteristics can be seen in the formation of international regimes that were science-driven, including the Convention on International Trade in Endangered Species and the International Union for Conservation of Nature and Natural Resources (Young 1989). The key to effectiveness is that knowledge on which the epistemic community is based is 'accurate, accessible, and contributes to the achievement of collective goals'. It must 'represent consensus and be provided through a medium that is politically palatable' (Haas 2004, p. 575).

The emphasis on consensus has been the characteristic of the epistemic community approach that has brought the most criticism, especially from scholars trained in STS. Lidskog and Sundqvist (2002) argue that Haas was right about the importance in international relations of a common picture of reality to challenge the idea that national interests alone drive outcomes. However, they accuse him of a naive view of science, almost to the point where he introduces a consensus notion of truth as part of the epistemic community argument. Others question the strong programmatic emphasis on consensus because disagreement is not only going to be found within any scientific community; it is a positive force driving new thinking (Young 2004).

Both the strength and the weakness of the epistemic community approach are that it posits an ideal situation: a strong consensus among scientists reflecting truth about nature that has clear implications, and policy alternatives that all of the scientists can gather around. It describes what has actually happened in several successfully negotiated environmental protection regimes. These empirical examples, while limited in number, do show that success is possible and provide a set of experiences of success from which lessons can be drawn. The weakness of the epistemic community approach is also rooted in its focus on an ideal situation. What about the majority of environmental problems where the degree of agreement among scientists is a mixed bag of agreement about some facts (and some values), but not others? Haas's (2004) response is that we must wait for the consensus, and until that consensus emerges, scientific and policy developments must be kept insulated from one another. This response is inadequate. It is grounded in a naive view of both the way the science boundary works and the degree of urgency we face in tackling many of these issues. Uncertainty is a reality we have to learn to deal with.

The epistemic community idea sees cooperation as an either/or proposition (Sebenius 1992) that fails to consider the multiple bottom lines that participants in negotiations face. Ways that policy development can move forward do not emerge all at once. What is most critical is that the people involved in ongoing negotiations are able to move forward when the opportunity arises, in a way that their ability to work together in the future is enhanced rather than impaired. Such a community may become a true epistemic community when uncertainty is reduced and the way forward becomes clear, but it must continue to learn and adapt in small steps the rest of the time.

2.2.3 Post-normal science: New forms of scientific practice

Silvio Funtowicz and Jerome Ravetz developed the idea of Post-Normal Science (PNS). PNS happens in policy areas characterised by both high uncertainty and high stakes. Under these conditions it is very difficult to keep facts separate from the interests and value positions, especially as they are usually expressed as probabilities. Where the stakes are high, interests and values determine how participants perceive the associated risks. Their basic argument is that such conditions require a more participatory decision-making process.

A central concept in PNS is the 'extended peer community'. To deal with new problems in a high uncertainty/high stakes area, an open dialogue is required because the quality of the science depends on an 'extended peer review'. The important thing to keep in mind to understand PNS is this link to quality. The idea of the extended peer community is close to, but not synonymous with, stakeholder involvement in science. The extended peer community is about the science itself, it is a new kind of quality control. It is made up of experts, even if some of these people base their expertise on experience-based knowledge, say of a policy process or a fishery, rather than research-based knowledge.

The ideas of PNS begin with a set of philosophical developments about the nature of quantification. Their arguments are mathematical ones broadly understood and do not begin with empirical observations the way that both the epistemic community approach and the Mode Two science approach do. It is from this philosophical perspective that they developed the NUSAP notation for quantitative statements. In the NUSAP notation, in addition to the familiar categories of number, unit and spread, Funtowicz and Ravetz (1990) introduce the new categories of assessment and pedigree. Assessment and pedigree are both about the characterisation of uncertainty.

Assessment relates to uncertainty rising from problems in the reliability of a quantity. It could be expressed, for example, in arguments about what level of confidence should be placed around a statement. Should it be 95%, 99% or some other level? Assessment is about the different kinds of judgements that one would see expressed by statements such as 'conservative by a factor of 10'. As Funtowicz and Ravetz (1990, p. 28) describe it, 'our knowledge of the behaviour of the data gives us a spread, and our knowledge of the process gives us an assessment'.

Pedigree relates to uncertainty rising from the border with ignorance, discussed above (Section 2.1.3), in the context of their categorisation of types of uncertainty. Here Funtowicz and Ravetz (1990) step entirely into the qualitative aspects surrounding the quantity. Pedigree addresses the relevant epistemological, historical, sociological and institutional contexts needed if one is to understand the implications of a quantity for policy. They develop 'pedigree matrices' based on a hierarchy of modes of knowledge: a deductive argument is stronger than an inductive inference which, in turn, is stronger than an analogical argument. All three of them are considered stronger than conventional definitions. They offer a number of examples of pedigree matrices. A very basic example is this pedigree table for research information (Table 2.1).

Table 2.1 Research pedigree matrix

Theoretical structure	Data input	Peer acceptance	Colleague consensus
Established theory	Experimental data	Total	All but cranks
Theoretically based model	Historic/field data	High	All but rebels
Computational model	Calculated data	Medium	Competing schools
Statistical processing	Educated guesses	Low	Embryonic field
Definitions	Uneducated guesses	None	No opinion

Source: Funtowicz and Ravetz (1990, p. 140)

The table establishes a hierarchy which allows the receiver of the quantity communicated with the NUSAP notation to evaluate basic levels of certainty. The NUSAP notation and the pedigree table help clarify the idea that it is in the extended peer community that the relevant forms of quality control for the quantity arise, in contexts of high stakes and high uncertainty. The extended peer community is made up of the various groups that can contribute their perspectives on the policy and their own knowledge. The extended peer community consists of the people who have the knowledge to fill out the pedigree table, knowledge that is found in both the scientific disciplines themselves and in the sociology of knowledge of the policy arena. Ravetz (1999) argues that effective science-based policies in arenas of high stakes and high uncertainty require an open dialogue with all those affected. The extended peer community improves quality by mobilising 'extended facts' to help develop a shared understanding of the uncertainty in areas of conflicting values and agendas (Healy 1999).

This extended peer community is not meant to reduce the authority of science or to make it explicitly political. It addresses the problem that in these high-stakes, high-uncertainty areas, the traditional mechanisms for assuring quality are not adequate. Accredited experts need the assistance of the extended community to get the necessary job done. Establishing the pedigree of a quantity requires a broad understanding of its saliency, credibility and legitimacy that only a broader group can provide. Ravetz (1999) expresses concern that seen out of context, these ideas may appear to reduce the authority of science; he stresses, however, that PNS is not about traditional areas of research, but new areas with high social and economic importance where traditional mechanisms for assuring quality are not adequate. PNS is not meant to be an attack on accredited experts, but a way of describing the kind of assistance needed (Ravetz 1999).

The way the relationship between scientific experts and other participants should be structured is one of the most helpful insights of PNS. Funtowicz and Ravetz (1990) often make use of the contract between 'knowing-that' vs 'knowing-how' when discussing scientific quality control in contexts of high uncertainty. Traditional science has seen itself basically as the first, but PNS requires a new emphasis on the second. There are several dangers in trying to maintain traditional styles of practice in uncertain conditions. Scientists over-sell their science, feeling that they have to show more confidence and authority than the situation warrants. This leaves them, in turn, facing exactly what they were trying to avoid: a continuous decline in respect for expert claims (Irwin 1995). Science policy settings in Europe and Canada often involve ongoing discussions with interest groups, and these discussions help guard against attempts to justify value-based choices with post-hoc scientific arguments (Jasanoff 1986).

Knowing-that is about the ultimate attainment of truth, while knowing-how is about practice. Knowing-how is about using skill, it is rooted in tacit knowledge and not part of the traditional philosophy of science. In resource management scientists playing this kind of role can be seen in certification programmes where management programmes are evaluated according to a complex set of criteria. Issues of uncertainty become areas of ongoing negotiations between the scientists and those who desire certification.

Within a high-stakes, high-uncertainty context, scientific skills provide 'rubrics, guidelines and elicitation procedures, for the expression of uncertainty, for the assessment of quality, and also for the training in both skills' (Funtowicz and Ravetz 1999, p. 68). Scientists working interactively with others can act as facilitators of transparency, while simultaneously taking advantage of different kinds of knowledge to try to describe and deal with the implications of uncertainty and ignorance.

Scientists are currently not trained to be consultants, but it is the skills of the consultant that are required here. These are the skills to work with policymakers and other stakeholders in a process linking the uncertainty and quality of the information with the needs of the policy. They point out that where experts in consulting professions normally have very long, practical, apprentice-type training after their formal educations (e.g. doctors), scientists generally do one major research project under supervision and then are certified as able to operate as an independent scientist. They argue that the ideas of skill and craftsmanship can be the basis of a way to reformulate the science boundary in areas of high uncertainty (Funtowicz and Ravetz 1990). The shift is from trusting science as a 'truth machine' to trusting the scientific institutions and procedures to make science that works in practical situations (Healy 1999).

Again, the strengths and weaknesses of the approach are closely related. Because it begins from an essentially philosophical perspective, the NUSAP notations provide a clear rationale, grounding the science boundary in epistemological rather than practical considerations. At the same time, the emphasis on scientific skills and the consultancy model suggests an appealing programme for implementing science in areas where boundary work can become very difficult. The idea of the extended peer community, however, needs sociological examination. The extended communities involved in science-based policies include groups with conflicting objectives who bring their own sets of relevant facts, values and interests into the discussion. Legitimacy can be lost if scientists are seen as closer to one interest group than another. Given the level of social conflict involved, what are the conditions under which an extended peer community adds to the quality of the science, rather than just adding some chaos to the complexity and uncertainty?

All three of these approaches to understanding the changing institutional context of science are helpful in understanding the case study presented here. While each of them can effectively supplement a human ecological approach, none of them addresses the question of adaptation directly. Nor do any of them even begin to address the complexity of understanding scientific institutions that seek to take an ecosystem approach to fisheries management. In seeking to support European fisheries management policy, the scientists in the ICES system both reflect and speak back to these three tendencies. Because they are doing so within a formal policy context, they share a set of more specific demands on their science with other scientists who work with policymakers. It is this exchange between science and policy that Chapter 3 addresses.

3. Developing scientific advice for policy

3.1 Saliency, credibility and legitimacy

Over the last 25 years or so, many social and natural scientists have begun to ask, some more and some less systematically, under what circumstances scientific findings are able to influence policy. Most of these investigations have been motivated by frustrations over the seemingly slow response of policymakers to environmental issues.

One of the most useful efforts to examine the use of science in policy is the Global Environmental Assessment Project, a large comparative research project carried out by the Kennedy School on the policy uptake of results from global scientific assessments (Cash and Clark 2001; Cash et al. 2002; Clark et al. 2002, 2006). As mentioned in Chapter 1, they conclude that the majority of the assessments are not effective in influencing policy, but they do affect the long-term development of an issue through such mechanisms as influencing the issue's visibility, the stakeholders who will take an interest, the way questions and objectives are framed, and the selection of management alternatives (see also van der Hove 2007). Hence, an approach to understanding international policy that examines the creation of common understanding within a political process is more instructive than simply examining issues of power and interests (Haas 2004). This influence of science on policy is reflected back on science through the influences of the policy process on scientists and their work.

Questions of policy and the needs of policymakers have a strong influence on science. As Young (2004) argues, international institutions influence the topics being studied and the kinds of models being created. Policy influences science directly by determining what scientific questions will receive funding, and less directly through changes in standards of quality control. When policy addresses areas of high uncertainty, and therefore competing interpretations among scientists, changes in the standards that determine the 'best available knowledge' become extremely important (van der Hove 2007). Debates about the credibility and uncertainty of results forming an important part of the negotiations are a very common feature in diplomacy related to environmental issues (Backstrand 2004). Scientists deciding which directions to take in the creation of new knowledge are strongly influenced by their experiences with what has proved useful and interesting to policymakers in the past. This desire for policy influence can also lead to downplaying disagreements in the search for consensus, in spite of the fact that 'within reason, disagreement is a positive force in the scientific world' (Young 2004, p. 223).

The Global Environmental Assessment Project made use of three analytical concepts that proved very useful in understanding the uptake of science to policy. Clark et al. (2002, p. 7) define these three as follows:

> Q 3.1 'Saliency' reflects whether an actor perceives the assessment to be addressing questions relevant to their policy or behavioural choices;
> 'Credibility' reflects whether an actor perceives the assessment's arguments to meet standards of scientific plausibility and technical adequacy; and
> 'Legitimacy' reflects whether an actor perceives the assessment as unbiased and meeting standards of political fairness.

A scientific result needs all three of these attributes to some degree if it is to influence policy. An important difficulty, Clark et al. (2002) argue, is that there are often trade-offs between the three. Indeed, they suggest that efforts to bolster one usually only succeed at the expense of another. For example, efforts to increase saliency by narrowing the questions to be investigated can decrease legitimacy by making the process appear to have a political bias. Conversely, adding stakeholders to increase legitimacy can reduce saliency as issues are raised that are outside of the policymakers' remit. While these relationships may usually involve trade-offs, they can also lead to mutual reinforcement and complementarity. For example, an effort to increase credibility by including new knowledge may also increase saliency and legitimacy. These observations form the basis of their empirical investigations of science policy institutions, suggesting that the main differences lie in the ways that they shape and balance the trade-offs among saliency, credibility and legitimacy.

It is important to note that these or similar categories also emerge in various ways in studies of the perception of scientific information in local contexts. It is these local contexts, rather than abstract ideas about science, that provide the tools that people use to make sense of the information (Irwin 1995). Michael (1996), for example, conducted a series of interviews with the general public about radon. They were interested in how people understood the categories of 'ignorance' and 'expertise'. They found that three kinds of responses were the most common and these three corresponded roughly to credibility, legitimacy and saliency. One was the basic, unsurprising idea that some people know more than others and the experts should be listened to. The second was the notion that understanding radon is 'not my job', suggesting an understanding of ignorance and expertise that was based on a legitimate division of labour. The third main response was in terms of saliency, the information about radon was simply not interesting to them. Yearley (1999) held a series of focussed group interviews with different stakeholder groups around air pollution models being used by a local government agency. They found that three factors influenced the public understanding of the models' output. The first two corresponded to legitimacy and credibility. The first was the respondents' assessment of the trustworthiness and agenda of the agency, and the second was their confidence in their own technical knowledge and ability to judge the technical merits of the model. The third was also related to credibility and reflected the importance that people attach to direct experience: they judged the models in terms of their evaluation of the assumptions about social behaviour that underlay them. All three of these factors were more important than the face credibility of the models based on realistic-looking simulations and projections.

3.1.1 What is saliency?

Saliency is very similar to 'relevance', in fact 'relevance' appears in the definition, and my fisheries science colleagues have from time to time wanted to know why social scientists insist on using this weird word instead of just saying 'relevance'. The difference is that many scientific findings are relevant to policy in the sense that they could logically be considered in making the policy without actually being salient. By using the term 'saliency' we are emphasising that particular facts become prominent because of their usefulness in responding to the needs of policy development.

In practice, policy-making must select from among a broad range of relevant facts to steer activities. The competition among stakeholders in environmental management in developing the knowledge base for management consists of each group trying to move a set of facts from being merely relevant to being salient. This competition happens within management systems that already presuppose a great deal about what facts are salient. Considerable aggravation is experienced by conservation interests, user groups and scientists alike when the facts they see as most relevant are not salient in the decision-making process. Many feel that there ought to be an objective, 'scientific' way to determine which facts should be salient. We expend a lot of energy in identifying the right 'indicators' and 'drivers' and experience a great deal of frustration when these lists quickly become very long. While science can perhaps eliminate some candidates for saliency in an objective manner, the movement of facts from relevance to saliency is unavoidably a political process.

3.1.2 What is credibility?

Credibility is about making sure that the scientific result reflects nature as closely as possible. Credibility comes from applying the scientific method, i.e. the testing of a falsifiable hypothesis, along with that method's guardians: quantification, replication and peer review. In fisheries science this methodological ideal is often difficult to achieve in the day-to-day world of trying to understand what is happening in the ocean. But the ideal remains a very real force because it provides the standard against which a scientific effort is judged. It is adherence to the ideal of the scientific method which confirms the scientific result to be an objective fact. Credibility is the concept most closely related to the phrase 'best available science' when it is used in a legal context as the required basis for policy. As a concept standing beside legitimacy and saliency, we can think of credibility as the idea that is most 'internal' to science as it is usually understood.

The question of credibility comes up in a number of ways within the CFP. Managers at DG MARE are constantly challenged with having to determine the credibility of particular pieces of scientific advice. In an interview, a high-level officer at DG MARE said:

> Q 3.2 I think we in the Commission are very careful before we choose to deviate from scientific advice, but in taking that position we are aware that the quality of advice is very uneven. And that could be for many reasons; it could be that scientists simply haven't been given worthwhile or reliable data on which to make any analysis at all. It could be that the number of people involved in the analysis has been maybe too small to allow the injection of alternative ideas or alternative approach, it could be that ICES itself has not really applied some form of quality control peer review, to make sure that everybody is working to the same standards in terms of quality of data. It may be that scientists aren't being honest enough.

The line between scientific credibility and legitimacy is a difficult one; in a practical sense we are always dealing with degrees of subjectivity as the application of the scientific method can almost always be challenged. Replication is a laboratory ideal that is very difficult in biology and environmental sciences; even in physics replication is heavily dependent on scientific skills (Collins and Pinch 1998). Peer review, for example, is closely tied to the social processes of science discussed below. Studies of peer review have shown that standards are a matter for negotiation and compromise. Peer review is a social process created and influenced by the communal needs of modern science (Jasanoff 1990).

3.1.3 What is legitimacy?

While Clark et al.'s (2002) definitions of saliency and credibility are fairly straightforward, their definition of legitimacy needs some fleshing out. I hope to show in this section that there is a lot more to the idea than simply 'unbiased'. Traditionally, legitimacy has meant a valid claim to a status. Someone is a legitimate child or a legitimate ruler. In our case we are talking about a claim that something is 'science' or, put more strongly, a claim that something has the status of fact rather than opinion. While the ideas of legitimacy and credibility are certainly different, they are linked together in a way that neither is with saliency. The ideas are distinct, but it is hard to raise the one question without considering the other. In this section I try to think through this distinction using a thought experiment in which I apply to scientific legitimacy some of the concepts developed by people studying political legitimacy. By doing this I am trying to tease out the part of scientific legitimacy that is purely political from the part that is rooted in the credibility of the science itself.

Political legitimacy is operating when people claim the authority to govern, and others assent to that claim. Max Weber was the founder of political sociology. His work offers a model of 'legitimate orders' based on three categories of legitimate domination (Weber 1968). For Weber a regime is legitimate for no other reason than people believe it is legitimate. He observed that such beliefs have historically been based on one of three broad categories of grounds: rational grounds, traditional grounds or charismatic grounds. A rational ground rests on a 'legally established impersonal order'. In other words such legitimacy comes from respect for the rules which are administered impersonally without anyone having special privileges. A traditional ground rests 'on an established belief in the sanctity of immemorial traditions'. A charismatic ground rests on 'devotion to the exceptional sanctity, heroism or exemplary character of an individual person' (1968, p. 215). For Weber legitimacy is simply conferred, it is in the eye of the beholder.

Some scholars, among them Hyde (1983) and Jentoft (1998), challenge Weber on this point. Both draw on Habermas and argue that legitimacy contains dimensions, particularly social justice and democratic participation, that are independent of people's attitudes – legitimacy is not just in the eye of the beholder, it stems from the characteristics of a process. I am very much in sympathy with the idea that there are universal rational grounds for legitimacy, and so much so that I relate an idea below – like my colleagues drawing on Habermas – that I think achieves this.

On one level, we can say that if credibility comes from the application of a methodology for establishing something as an objective fact, then legitimacy is something additional that makes that something a fact rather than an opinion 'in the eye of the beholder'. When applying Weber's categories, it is fairly easy with the first two to identify this additional thing. But for the category of 'rational legitimacy', which is a kind of legitimacy drawn upon frequently in discourses about science, these two things are not easily distinguished. What question can be raised about the rational legitimacy of a scientific process that is not a question about its scientific credibility derived from the application of the scientific method within that process? This problem is addressed here by arguing that one kind of rational legitimacy – process legitimacy – is best understood as the beholder asking precisely if the process meets a set of universal criteria. Those criteria encompass the scientific method but extend further into the social processes in which the scientific claims are made. It is the greater complexity of these criteria that allows us to think of this kind of legitimacy as being more than just the question of credibility.

Are there 'traditional' grounds for scientific legitimacy?

An important book by Barnes et al. (1996) suggests that the answer is yes; grounds that any scientist would recognise in his or her own day-to-day experience doing science. These sociologists were writing partially in response to extreme ideas about the 'social construction of nature' that are found among many sociologists of science. They agree with the basic idea that social considerations influence our scientific picture of nature, but also believe that external reality plays an important role in determining what will count as fact. The role they see social considerations playing comes from the traditions of scientific communities, but not in a way that sees tradition as a source of dogma or as restricting the importance of facts in nature. They borrow Kuhn's famous idea of the scientific 'paradigm' and think of it as a kind of community. Then they look back to the founding work in the sociology of science in the 1950s and revive Robert Merton's (1968a, 1968b) concern with the epistemological importance of the scientific community.

What makes scientific inference possible, they argue, is the trust provided by the culture of a scientific community. A theory that seems initially implausible will have a hard time competing with one that seems initially plausible, and this plausibility comes from the consensus of the scientific community – i.e. a cultural tradition. Scientists operate as a social community, but all scientists believe that external nature determines their findings, and they use this basic belief to coordinate their actions. Hence there is no contradiction between recognising that science is governed by social processes, including traditions, and believing that science deals in facts (Barnes et al. 1996).

Such traditions are clearly not grounded in the sanctity of the immemorial, but they are very real and have an important influence on what science is seen as legitimate. In fisheries science in Europe, these communities include both the broad scientific community and more narrowly defined ones. Reference to the broader scientific community can at times be brought directly into the politics around the science being used in support of fisheries management. One such event that created a lot of tension was an inquiry by the Royal Society of Edinburgh (RSE) (2004) that focussed, among other things, on the scientific validity of the ICES system. Their report suggested that scientists had to take some of the blame for the condition of the North Sea cod stock as well, because their advice for a quota value in 1997 was too high and led to the destruction of a strong year class that could have led to the recovery of the stock. Specifically, the executive summary reads:

> Q 3.3 A major strategic error was made in the management of the abundant 1996 year-class in the North Sea, when scientists recommended increases in TACs instead of recommending low fishing mortality that would, if accepted, have averted the current crisis. In general, cod stocks have been over-fished in compliance with erroneous advice from ICES Advisory Committee on Fishery Management (ACFM) until the last few years when advice was to reduce fishing for cod to the lowest levels and then to close the fishery altogether (Royal Society of Edinburgh (RSE) 2004, p V).

This report received a great deal of publicity in the UK. One of our respondents experienced an uncomfortable amount of public attention and blame. He pointed out that his lab had been reviewed by six outside groups in the past year, and that none of the other five had placed any direct blame for the state of the stocks on fisheries scientists. He argued that the Royal Society had actually gotten quite a bit of its information wrong about how stock assessment is currently carried out:

> Q 3.4 They have got a lot of information wrong on what we do. They talk about using VPA, we have not used VPA for years, they talk about we should use statistics based models, which is precisely what this lab in particular has been pushing out through the assessment world. Their description of VPA is wrong, indeed elements of the report are inept to the point of incompetence ... For example ICES has not recommended TACs since 1990, and it said it isn't going to recommend TACs because they don't ... regulate fishing mortality and that is what you need to regulate. ICES has said for years after that that you need a direct reduction in fishing effort. Part of the MoU between ICES and its customer groups like the EU, the customers demanded numbers in terms of quotas, but nevertheless the advice has always recommended direct reduction of a substantial amount in fishing mortality through direct control. So if the Royal Society had read the report they would have realised that ICES did not, as they said it did, recommend a TAC.

This respondent felt that the fisheries science community should have adopted a more aggressive defence against this report. He felt strongly that this episode called into question the level of his own future participation in stock assessment. This had been an activity on which he had been willing to spend a good deal of time, in spite of limited personal rewards and substantial professional costs such as lost chances for research and publications. This incident made him feel that doing stock assessments was not being appreciated, either by the broader scientific community or, to some extent, by ICES and his own superiors who were unwilling to defend their work sufficiently from unfair criticism. The point here is not so much the merits of the RSE critique as the importance that this critique held for this scientist, even when it was just one of six reviews.

Other sub-groups of scientists are also important, for example ones working in environmental protection, marine scientists, or more specific disciplinary categories such as biologist or ecologist. These boundaries are necessarily fuzzy and will change depending on which common understandings one is pointing at. This fuzziness is not really important. The main point is that there is a 'traditional' basis for scientific legitimacy in sets of common understandings reproduced through such things as organisations, task-oriented professional networks, conferences, and journals that form loose communities with shared ways of looking at problems.

Their particular importance in fisheries science is reflected in many ways, but two prevalent general attitudes stand out and have a strong and growing influence on the fisheries scientists working in Europe. Both of them, without any particular empirical content of their own, have a powerful influence on which scientific results and interpretations of those results are going to be seen as legitimate. These are the general support for the ecosystem approach to fisheries management and the precautionary approach to environmental risks. Many examples in this book will demonstrate that these two 'scientific ideologies' are having a profound effect on the legitimacy of marine science in Europe.

Are there 'charismatic' grounds for scientific legitimacy?

The importance of charismatic grounds, i.e. of 'devotion to the exceptional sanctity, heroism or exemplary character of an individual person' (Weber 1968, p. 215), is somewhat easier to establish for science than traditional grounds were. Even the editors of tabloids recognise that only 'top' scientists would be qualified to invent a new pill to reverse the aging process. We can all recognise that the endorsement of some scientists lends greater legitimacy to a finding than the endorsement of others.

More important perhaps for fisheries and other areas of science-based policy is the charisma of certain journals. In the public eye *Science* and *Nature* in particular play the role of the 'go to' source of information for non-scientists who want the straight scientific facts. The legitimating power of scientific charisma, and the difficulty it presents given the attractions of publicity, is illustrated by the recent controversy surrounding the article by Worm et al. (2006) in *Science*. This article contained dire predictions about a global fish collapse based on evidence that was strongly disputed by a large number of fisheries scientists. According to Longhurst (2007) a number of critical comments were offered to the editors of *Science* but none have appeared to date in the journal. In response to Longhurst's own submission, the editors informed him that they were aware that 'areas of legitimate disagreement exist' (2007, p. 1), but how this observation justifies not publishing critiques of the article is not clear. Similar controversies related to the earlier work of Worm among others led Ray Hilborn, himself a top scientist, to opine as follows in the journal *Fisheries*:

Q 3.5 Two journals with the highest profile, *Science* and *Nature*, clearly publish articles on fisheries not for their scientific merit, but for their publicity value. Beginning in at least 1993 with an article I co-authored .... *Science* and *Nature* have published a long string of papers on the decline and collapse of fisheries that have attracted considerable public attention, and occasionally gaining coverage in the *New York Times* and the *Washington Post*. I assert that the peer review process has now totally failed and many of these papers are being published only because the editors and selected reviewers believe in the message, or because of their potential newsworthiness (Hilborn 2006, p. 554).

The *New York Times* (Dean 2006) did indeed publicise the Worm article. The article quoted Boris Worm saying, 'there is not a piece of evidence' that contradicts the dire conclusions. To be fair, the article also quoted Steve Murawski, Director of Scientific Programs and Chief Science Advisor for NOAA Fisheries, saying that the conclusions did not jibe with the trends that NOAA was observing, although he was the only one of the four scientists quoted that questioned the results. The *Times* also published a column by their right-wing columnist (Tierney 2006) that did express scepticism about the Worm prediction.

Other examples of the charismatic legitimation of science come easily to mind. To some extent there is a real need to christen certain sources of information as 'trusted' to make day-to-day information processing feasible. But such christening is dangerous; peer review no longer seems to function, if it ever did, as a sufficient guarantor of credible information. The scientific community can, in the long run, correct particular failures of peer review, but this may be too slow a process to provide an adequate filter to keep controversial results from being the basis of policy when top scientists and top science journals have blessed them.

Are there rational grounds for scientific legitimacy that are distinct from scientific credibility?

This is the most complex question about the nature of scientific legitimacy. There are two kinds of rational legitimacy, and the response is strongly influenced by which kind is being considered. Several scholars who have examined the question of political legitimacy as it touches on policy-making have suggested a division of the rational grounds for legitimacy into two basic types: process legitimacy and outcome legitimacy (Jentoft 1993; Meunier 2003; Scharpf 1999). Meunier (2003) describes these two types of legitimacy as follows:

Q 3.6 Process legitimacy: This vision of legitimacy focuses on the process by which decisions are made. It is based on the assumption that the people

> are the only source of sovereignty. Policies are legitimate when policy-makers are representative, accountable and placed under public scrutiny…
> Outcome legitimacy: This vision of legitimacy focuses on the policy eventually made, and not on the process through which it was made. In this case, what makes a policy legitimate is its capacity to solve problems requiring collective solutions, and solve them in a way benefiting the 'public interest' (2003, p. 76).

The distinction between process and outcome legitimacy means that there are two basic ways that people rationally evaluate whether a decision-making process is legitimate. The first is based on the process by which the decision gets made; the second is based on the characteristics of the decision itself.

Even when science is evaluated in terms of process legitimacy, the answer to the question in the title of this section is yes, but the scientific legitimacy is very closely tied to the question of credibility. The beholder is in fact evaluating aspects of the process that are directly linked to the question of credibility, and the response to challenges to procedural legitimacy is made by defending the credibility of the science. For process legitimacy to say that a scientific process is illegitimate is tantamount to saying that the methodology followed was unscientific. This is not quite as straightforward as it might seem, though. What I will try to convince you of in this section is that the rational source of process legitimacy for any decision is based on the same underlying principles as the scientific method. The answer to the question is yes because the questions of process legitimacy include but are wider than simply the scientific method – they also extend to surrounding social processes.

Is it possible to have a concept of the legitimacy of a process that is not just a set of opinions about what a process should be like? Can there be process legitimacy that is not in the eye of the beholder? Meunier (2003) says that a legitimate process should be 'representative, accountable and placed under public scrutiny'. That sounds fine. But does it have any basis beyond 'this is the way we like things done in Europe'? I think there is such a concept, which can be arrived at by an argument I find logical and compelling and, more importantly, which links up directly to our general understanding of what makes science credible.

Again I am referring to the concept of communicative rationality that is at the centre of Communicative Systems Theory. To repeat the basic idea, communicative rationality is the logic that we use when we talk to each other to make sense of what we are saying. It can be thought of in a pure sense that would be useful for looking at philosophical arguments, but we also use it in day-to-day speech to understand normal situations. At the social level, it depends on two general rules: that there is no manipulation involved in the communication – meaning that no one is prevented in principle from participating or is forced or paid or tricked into making statements that do not reflect their true beliefs – and that everything communicated is open to question about its validity (White 1988, p. 56).

Now this ideal is not meant to describe a real situation. Habermas himself uses the term 'partly counterfactual' (Habermas and Nielsen 1990, p. 105) meaning that while these ideals exist only in people's heads, they are still a very real, concrete aspect of a situation. This is because people are always using them to evaluate the communications they are engaged in, and they have to believe that the standards are met, at least to some degree, if they are going to trust the communications enough to be able to build a shared reality. For example, your boss can tell you to be quiet because he says so, but that does not help you and your boss come to a mutual understanding about what your situation is. In fact, it is irrational because it keeps you from reaching a better understanding. We see this ideal operating every day. No one takes statements made in marketing or political contexts very seriously because we know that in these arenas communications are highly manipulated and rarely have to respond to serious questioning. Planning meetings with colleagues – at my workplace and I hope at yours – are taken very seriously, people really have to explain why they see things the way they do, and serious violations of the ideal standards would be seen as major betrayal. Of course, these standards play a role in broader discourses as well. The rules against manipulation are the basis of accusations that scientists have been corrupted by their funding sources. As discussed in the next section, these are perhaps the most common challenges to scientific legitimacy.

To a degree, this is all a fancy and detailed way of saying that people have to be fair and listen carefully to one another if they are going to understand each other. It is pretty much common sense. It has to be very common sense because we all have to live together and coordinate our actions all the time, so we all have to make use of these standards in day-to-day speech. But underneath this common simplicity Habermas (1990) has found an elegant result, one that yields a concept of process legitimacy that is not just an opinion.

These standards of ideal speech are not the basis of process legitimacy because of their functional usefulness. We cannot assume that a process is legitimate just because it works well to achieve some end. This would then raise the question of the outcome and goals of the process, and these outcomes and goals cannot turn around and determine the legitimacy of the process that created them. Legitimacy is a normative concept, an evaluation of legitimacy requires showing objectively that the process is as it *ought to be*. You cannot settle an argument about what ought to be by pointing at evidence. However, you can settle it if you show an internal contradiction in the opposing argument. You can prove X by demonstrating that any argument that objects to X already assumes X (Habermas 1990). The ideal speech standards achieve this. In fact, they are very likely the only thing that does. They are inescapable presuppositions of any argument – just by raising an argument you are assuming that they are in force because otherwise there is no point in making the argument. In other words, you cannot logically raise an argument against the basic rules that make raising arguments meaningful in the first place. Therefore, these commonsense rules of how we make sense to one another also point to context-independent criteria that can always be used to evaluate the *rational* legitimacy of communicative processes, including decision-making processes.

The concepts that we commonly use when talking about process legitimacy, where they are valid, are all ways of trying to uphold the standards of ideal speech. The first three principles of good governance articulated in the European White Paper (CEC 2001a), participation, openness, and accountability, are all rooted in the demands of ideal communication. Meunier's (2003) 'representative, accountable and placed under public scrutiny' criteria are as well. 'Participation' is a restatement of the requirement that no person or argument be excluded, while 'representativeness' is about trying to find a fair way to do this when the scale of the discussion does not allow everyone to speak. 'Accountability' is the demand that people account for themselves, that they explain what they think and why they think it. 'Public scrutiny', 'openness' or 'transparency' is in many ways the critical technique for guarding the standards of ideal speech. Transparency makes accountability possible and is the main safeguard against manipulation.

What science does that is special, and this is I think the essence of the scientific method, is that it takes these general rules for rationally legitimate decision-making and turns them into a rigorous procedure. The ideal of the scientific method can only be approached, not achieved in practice, in the same way that the standards of ideal speech are never completely met. The scientific method rejects dogma, in principle, no claim is excluded on a priori grounds, and all claims must pass the same universal set of procedures to be verified. In practice, of course, the majority of claims are 'black boxed' in the sense that they are assumed to have been already established, and notions of plausibility in the tradition of the scientific community play the role of gatekeeper in deciding what claims will be examined. This process is carried out, however, in a way that recognises the application of the same set of procedures to all claims.

The scientific method takes accountability and transparency to their logical extreme. The statement of a falsifiable hypothesis sets up the clearest accountability one can imagine. Quantification and experimental replication require the scientists to explain how they know something with such precision that someone else can go out and repeat the experience. This makes quantification and replication the ultimate ideal of procedural transparency. The precision of language that mathematics brings to descriptions of natural phenomena provides the most transparent accounting possible. Ultimately, this radical commitment to the standards of ideal speech is why we turn to science when we want to resolve questions about nature. Science is cast in the role of the arbiter of truth because of the transparency of its philosophically clear, if practically obscure, ability to say how it knows what it knows. The irony is that these procedures for radical accountability and transparency, and the tools of precision they have engendered, are what has made science so opaque to the non-specialist.

The question behind the eyes of the beholder under the rational category of legitimation of science, when the beholder focuses his or her judgement on procedures, is the question of scientific credibility. This does not mean, however, that scientific process legitimacy is the *same thing* as scientific credibility. Scientific process legitimacy places the question of scientific credibility in a wider context; it examines the entire gamut of standards of ideal speech when asking the question of scientific credibility.

Process legitimacy demands freedom from manipulation. When a drug company pays for research on the effectiveness of its own drugs, a question of procedural legitimacy is raised – were the results manipulated in order to maintain funding? The response to these questions of manipulation, however, is to seek to demonstrate in a transparent way that established scientific methodologies were adhered to.

Process legitimacy demands freedom from the suppression of relevant claims. Science that is selective about its evidence can be very damaging, especially when the results have an influence on policy. A very famous and far-reaching example in fisheries management is the 'tragedy of the commons'. This is a theory made popular by Hardin (1968) that has had a tremendous influence on natural resource policies. Hardin took a basic result in resource economics showing that when a natural resource is managed under open access – i.e. when anyone can use it – it will be overused to the point where the economic benefits will be wasted and the resource may even be destroyed. He gave this result the catchy title 'tragedy of the commons'. What Hardin and other adherents to this theory did, however, was to conflate 'open access' with 'commons' and conclude that only private property or government control can avoid the tragedy. This 'tragedy of the commons' parable became common sense in natural resource management for decades, ignoring mountains of evidence that property owned in common by small groups was very often not open access, but rather managed in ways that avoided resource degradation. The real tragedy that emerged from all of this was decades of ineffective top-down management that assumed that when it is not feasible to establish private property rights, only government agencies could manage resources.

Finally, outcome legitimacy also points to a kind of scientific legitimacy, although it has little to do with issues of the common good and everything to do with the potential policy impact of the new information. Policy debates are not made up of discussions naming long lists of independent facts. Instead, they are made up of discursive themes. A discursive theme is a way of linking facts, values and interests together into a story line that makes sense. It is these linkages that compete in the discussion. The story lines do change, but only slowly. When a new fact is presented that fits into an existing story line without problem, it is quickly taken up – i.e. it is treated as legitimate. When a new fact does not fit easily into a story line, it is resisted. Such resistance does not necessarily include an attack on the credibility of the process that created it. It can also mean simply ignoring the fact or treating it as unimportant. This kind of rational legitimacy is similar to the saliency of the result, but it is not about how well it fits into a policy process as much as how well it fits into the constellations of facts, values and interests that different stakeholders are bringing to the discussion.

3.1.4 Saliency, legitimacy and research on risk perception

A number of studies have been made of the impact of the source of scientific information on its saliency, credibility and legitimacy. Risk studies have shown that people care more about how decisions are made and their fairness than they do about the magnitude of a risk (Chess and Lynn 1996). The perceived saliency of an issue, and related scientific communications, also emerge as important in research on perceptions of environmental risk.

Furthermore, credibility and legitimacy are both closely tied to the source of the information. People perceive both themselves and others as having or not having a right to talk about something (Michael 1996). This is not surprising when we consider what might be the most basic observation from the sociology of knowledge: the social location of facts is what determines their effective validity. For example, as Collins and Pinch (1998) point out, you and I base our personal knowledge about nature, e.g. that it is not possible to travel faster than light, entirely on our personal knowledge about society that such information resides with physicists rather than Star Trek scriptwriters.

One of the corollaries of this observation, which has been well documented in research, is that in the event people do have personal experience of a phenomenon, they weight that experience much more highly in comparison to information that is merely communicated. Perceptions of risk in general are higher when they are more salient to the perceiver, for example if they know somebody who personally suffered the adverse event (Kolker and Burke 1993). The inverse, however, is not true; higher risks are not necessarily the more salient ones. In fact, people see risks with middle-level probabilities as the most salient, and high-probability dangers are overlooked because they are more accepted, for example traffic accidents (Douglas 1985).

In another example of exaggerated trust in one's own experience, people overestimate their ability to detect risk themselves. Many non-specialists believe that they should be able to detect risks with their own senses, for example one study of risk perception found that non-scientists thought they could taste pollution (Johnson and Griffith 1996). Judgements about whether scientists and others are aware of risks are not correlated with concern about the seriousness of the risks (Fife-Schaw and Rowe 1996).

Whether or not the risk is seen as under one's own control or as forced on one by another party is also important, not only in perceptions of fairness and legitimacy but in respect to saliency as well. A risk that becomes well-known is generally connected to some issue of legitimacy; therefore, moral concerns are involved not just in the response to risk but in its perception (Douglas 1985).

I do not know of any research that directly links these findings about saliency and risk to the question of how stakeholders will perceive the saliency of scientific advice for policy. It is a reasonable hypothesis, however, that advice based on findings linked to experience and previous knowledge of an issue will not only be seen as more credible, they will also be seen as more salient.

Legitimacy and source are linked in ways that are filtered through local experience. People dealing directly with the public have completely different ideas than scientists about what makes a communication credible. Chess et al. (1995) interviewed 145 risk communication researchers and practitioners about what scientists and people dealing with policy thought was important in the presentation of risk information. The technical people saw the question of how to express probabilities and communicate evidence to support their assertions as the heart of the problem. The practitioners were much more concerned with how to integrate political and value-based concerns into the policy responses.

Legitimacy is tied to the source of the information most often by questions about the interests of the researcher. Scientific findings are often seen as purchased. In fact, the notion that science is an expression of political interests is a form of 'common sense' among much of the general population; claims of factuality and objectivity, rather than increasing credibility, are often seen as just one more marketing tool (Irwin 1995). McKechnie (1996) in field work in a rural area found that sources are seen as credible only if their self-presentation is consistent with local values and that assertions about having esoteric knowledge actually undercut credibility as the source is seen to lack 'common sense'.

This common sense has a factual basis. It is becoming increasingly common for scientists to have direct financial stakes in outcomes. Comparative, statistical studies of the outcomes of drug trials have found that drug-company funding has a strong influence on results: 89% of company-funded studies showed new drugs to be more effective than older ones compared with 61% of those not company-funded. Moreover, the authorship of articles in major medical journals is routinely hidden; one study found that 29% of articles used such devises as guest authors or ghost authors (Guston 2001b).

Frewer et al. (1996) did a series of semi-structured interviews related to the source of information about environmental risks. They found that trusted sources are characterised by multiple positive attributes. Interestingly, while observers of policy process often assign a high weight to 'independent' sources of information, the people interviewed in this study found that sources which operate under moderate accountability, rather than complete freedom, are trusted more. Television current affairs programmes and quality newspapers are among the most trusted, with the tabloids and government the least trusted.

These potential linkages between risk perception, saliency and legitimacy take us close to the central issue of uncertainty. Jasanoff (1986) points out that officially sanctioned risk assessments focus mainly on technical questions, and their guidelines emphasise using multiple sources of uncertainty to make numerical assessments. This in itself has a political dimension as environmentalists feel that risk assessment creates a false impression of certainty. Shackley and Wynne (1996) are concerned that scientists are motivated to demonstrate control over uncertainty, for example by its quantification as risk, because they see uncertainty as a challenge to the authority and use of science in policy-making.

The question of legitimacy and source is an important one in the science underlying the Common Fisheries Policy. One question in our survey asked the extent to which scientists in expert groups suspected other scientists of arguing in a way that was consciously biased by that scientist's national interest (Table 3.1). The overall mean is just below the middle point of 4. Mean answers from those scientists whose last group was directly involved in stock assessment were slightly higher than from the others.

Table 3.1

When participating in working or study groups how often have you suspected that a scientist was arguing in a way that was consciously biased by his or her national interests?
1 = never, 7 = very frequently

Last group was not a stock assessment group	Mean	3.51
	N	161
Last group was a stock assessment group	Mean	3.89
	N	123
Total	Mean	3.68
	N	284

Relationship is significant at .06. Excluded to reach N of 284: 148 respondents who indicated not having participated in an expert group in the last five years; 20 who did indicate participating but did not make clear which kind of group and therefore did not fit in any of the two categories; six who failed to answer whether they had been in an expert group or not; and seven who did not answer the question on bias.

Scientists have also expressed feeling on several occasions that particular countries were withholding contributions of information that they should be making. Perceptions of national perspectives that reflect local fisheries but are not seen as full-blown biases are also found. One Scottish scientist told us that 'you could never get a Danish scientist to say that industrial fishing is a bad thing'. If other scientists have suspicions of national biases, it is reasonable to suppose that non-scientists have the same suspicions at an even higher level.

3.2 Boundary organisations and objects

The science boundary is the line between which claims are or are not science, between which procedures do or do not produce such claims, and between who is and who is not qualified to carry out such procedures. Science in support of policy needs to be careful about where it places the science boundary when trying to increase its credibility, saliency and legitimacy. As we have seen, in situations of high uncertainty, there is a greater need for expanding participation in scientific activities. In addition to the needs of addressing uncertainty, pressures to inflate the science boundary also emerge through policy processes seeking 'scientific' answers to more and more kinds of questions. In this section I briefly review the research that has been done on placing and maintaining the science boundary.

This constant focus on maintaining boundaries around science is critical to the functioning of both science and policy. It allows scientists and policymakers to bring a clear accounting of knowledge, including an accounting of uncertainty, into policy decisions, and this accounting, and the status distinctions that support it, should not be discounted because they cannot be perfect (Collins and Evans 2002; van Zwaneberg and Millstone 2000). The demand for objective standards for decisions, and the conferral of a special status on particular forms of knowledge to protect these standards, is not a denial of democracy; it is a product of it (Porter 1995; Ozawa 1991). One of the most basic insights from science and policy studies is that boundary work must be taken seriously. It cannot be treated naively; neither by assuming that the distinction between what is science and what is policy, advocacy or values is easily made in concrete situations, nor by assuming that it does not really exist. Boundary work as it emerges within policy debates is not an all or nothing affair (Guston 2001b). In more participatory decision-making processes, it is called upon to support specific judgements in circumstances where there are checks and balances on its use.

In fisheries management, what might be called the mainstream view of the role of science within the policy process is articulated as follows by DG MARE:

> Q 3.7 Visibly free of political influence: Scientists in most national administrations are generally placed at a distance from administrative and political

> pressures by the national fisheries laboratories. Those who are not so distanced quickly lose credibility and influence (CEC 2003a, p. 15).

Science must stand off to the side so that the other players in the policy debate have a common set of facts to debate about. Otherwise, following this logic, there is no possibility of a rational conclusion. The most concrete expression of this role is when DG MARE is able to point to scientific conclusions to justify particular management measures. This need is only intensified by the move towards the use of harvest control rules which, in the final analysis, uses the scientific finding as a mechanism to shortcut the policy debate, hence adding predictability and continuity to the polices to everyone's benefit.

On the surface, this mainstream view is an expression of communicative rationality in the way it seeks to separate facts from values and interests. However, as the research described in this section demonstrates, the view laid out in Q 3.7 is an utopian construct with a number of problems. The focus is on the objective assertions made by people assigned the role of being the objective people. In communicative rationality, assertions of fact are unique because they are carried out in discussions that presuppose consensus as a goal, a presupposition that makes transparency about the reasons for the assertion a central concern. Here the scientists' expertise in the methods of transparency is what is critical, not some privileged access to objectivity. The mainstream view expressed in Q 3.7 is not, in fact, communicatively rational at all because it prejudges assertions based on their source.

Boundary work is difficult. Scientists, of course, have the primary stake in boundary work. Within STS the early work on 'boundary maintenance', particularly Gieryn (1983), has now achieved a seminal and classic status. The basic argument was that within policy deliberations, scientists work to enhance their authority by defining what knowledge is really science, who is really a scientist (Jasanoff 1990; Gieryn 1983), and which questions are to be considered scientific (Dietz et al. 1989). When something is labelled as science, those who are not scientists are *de facto* barred from having anything to say about its substance (Jasanoff 1990). When this labelling is successful, knowledge becomes a 'black box' for the non-scientist until another expert challenges it.

Constant pressure is seen in policy contexts to make the political appear objective (Ozawa 1991). The constant pressures to inflate the science boundary to treat more and more things as appropriate for a scientific approach was discussed in Section 2.1.1. Seeking to make political decisions seem objective is not necessarily a product of fraud or opportunism – it can arise simply from the nature of the problem to be solved. In science in support of fisheries management, for example, one critical issue is the need for the operational units reflected in the advice to match those of the actual fishing activities that are being managed. Fisheries scientists traditionally worry about units defined by nature. These are mainly single fish stocks, or increasingly biological and ecological interactions between stocks and between the fish and their environment. These are all things that exist independently in nature. They are not, in principle, created by people, and they operate under natural laws. I say 'in principle' here because attempts to describe and measure these things are created by people, and actions by people have a powerful impact on their condition. Managers, however, manage fisheries which are complexes of fishing ports, fishing boats, and fishing gears that are hybrids of natural, technical and social phenomena, some of which are entirely human inventions (Wilson and Delaney 2005). Fisheries scientists are being pulled more and more into hybrid domains that they share with social scientists and stakeholders. Fishing fleets can, and do, read scientific advice based on their behaviour and then change their behaviour, often in ways to avoid the implications of the scientific findings. Fish stocks do not. When scientists are dealing with such slippery units of analysis as these, it is very hard to separate the 'political' and the 'objective'.

Tensions on the science boundary have many other sources. Gibbons (1999) argues that there is nothing really new about the boundary problem; reliable knowledge has always been reliable within certain boundaries. What is happening in policy contexts with the risk society is an increase in contestation. Knowledge that is incomplete now is not just knowledge that is waiting for better science; it is knowledge that will be contested. This contestation then puts further strain on the science boundary. Evans (2005) observes one case of scientific controversy where the more one side tried to show themselves as more scientific than the other, the more they broke down the appearance of science, and each began to appear as 'scientific' as the other. A number of studies have shown that boundaries that are too rigidly maintained can lead to scientific elites being shielded from accountability until a crisis is required to expose the problems (Waller 1994). Increasing competition is also exposing scientific boundaries to increasing strain. Waterton (2005) carried out a large set of interviews in which she asked scientists to reflect on science. She asked about the impacts of the rise of contract-driven science, and many of her examples were from scientists involved in policy support. Scientists invoked scientific norms as something that has been lost fairly recently as science has become more and more contract-driven.

Boundary work is a response to the repeated failure of methodological definitions to determine what is and is not science in practical contexts (Evans 2005). Science in practice can no longer be easily reduced to a single methodology or ascribed to a privileged subculture (Gibbons 1999). Halffman (2003) argues that neither the 'cage model' nor the 'seamless network' model seem to accurately describe the policy science interface. The 'cage model' is based on the idea that it is possible *a priori* to establish criteria about what is 'science' and what is 'politics'. It is the model supporting the mainstream view displayed in Q 3.7. In the 'seamless network' model, for which Halffman (2003) cites Actor Network Theory as the most extreme example, the attempt to make a distinction between science and politics is abandoned. Halffman (2003) argues that both approaches fail to describe the actual division of labour of regulatory regimes, and they both reduce the real efforts to negotiate the boundary to 'misrepresentations', i.e. independent science on the one hand or false dichotomies on the other. These difficulties have led to contradictory messages. Irwin (2006) offers an interesting analysis of current statements about science and policy being made by responsible institutions in the UK. The statements being made still remain rooted in a traditional understanding of science as purely objective information that can still play the role of an objective 'other' standing apart from the policy process, while the statements are simultaneously promising a more democratic approach to science. The two narratives of inclusion and otherness go on at the same time from the same sources without resolution.

One way of enforcing the mainstream view is to discourage scientists from articulating policy opinions. One role of fisheries scientists in Europe is to provide support to international negotiators, and one of our respondents related that he was present at the negotiations when the North Sea cod closure was decided on in 2001. Afterwards he told the press 'well, it's a waste of time, it won't work', for which he got his 'wrist slapped'. He felt a role contradiction because on the one hand he was a part of the negotiating process and he should support it, indeed he was expected to by his employers, but scientifically he could not.

Another respondent articulated this tension this way:

> Q 3.8 Scientists shouldn't be stakeholders. We should be objective outsiders and advisers and we shouldn't be seen as stakeholders. But as I see it ... catch advice would/should be accompanied by much wider input from, you know, other stakeholders ... and it would be a mistake for us to run a whole series of projections and more bigger and complex models and just give a whole set of numbers. That would seem to me to be pointless and repeating the mistakes of the past ... of just saying: 'Us scientists know the answers and here are your numbers'.

Both of these scientists seem to be reaching for a new way to articulate the role of science within the policy process. This is based both on their own beliefs about the policy process as it is currently structured, not seeming able or willing to respond to scientific opinion, and by the ways they are seeing other stakeholders looking at them. The scientist in Q 3.8 mentioned in another part of his interview:

> Q 3.9 Interviewer: When you're doing the assessment, you think 'this will be scrutinised' ... is that something that is in the back of your mind? Respondent: Yes, very much so. I always like to think that the difference between the guys in the Mediterranean, for example, and the way that we work in the North Sea, they almost still have the [environment where if] a

> scientist has an opinion therefore this is true. And one gets the impression that they're actually listened to, in their own countries. Whereas, we seem to have the philosophy here whereby if you have an opinion, you have to have the data to back it up.

Here we almost feel a hankering towards a perhaps imagined situation where the scientist's word will be accepted as science, but a recognition that in the environment where he is working, he will only be believed up to the point where he can back up his words with evidence that he brings to the public arena. The scientist is like any other stakeholder and what he brings to the table is scrutinised, with little of the 'distance' that brings 'credibility and influence' in the words of DG MARE (Q 3.7; CEC 2003a, p. 15).

Boundary analysis was a central tool that Cash et al. (2002) used to describe the trade-offs and synergies among credibility, legitimacy and saliency. Maintaining strong boundaries runs the risk of decreasing saliency by removing decision-makers from helping to define questions. But mechanisms that limit participation of decision-makers, as well as other stakeholders, sceptics and/or non-mainstream experts, to maintain credibility decrease legitimacy. Increasing legitimacy by greater inclusiveness can decrease saliency by re-framing the issue in a way that is irrelevant to policy decisions, as well as leading to accusations that the science can be seen as 'tainted' by politics.

> Q 3.10 If boundary spanning is too strong in coupling producers and users of assessments, suspicions arise that policymakers are not only asking the questions but also determining the answers, thus reducing the credibility of the assessment and thus its influence ... Effective institutional arrangements for boundary spanning facilitate agreement between scientists and decision-makers over what questions an assessment will address, what kinds of evidence and expertise it will employ, and what processes it will follow (Clark et al. 2002, p. 9).

Structuring the science boundary for maximum credibility, legitimacy and saliency requires a consideration of the timing of processes, in particular the timing of the use of different disciplinary perspectives and/or stakeholders. Inclusion of a wide group in order to increase legitimacy can fail on all three dimensions if the inclusion comes so late in the process that it cannot influence the questions being investigated. But there is stress between this consideration and the roles of various disciplines in establishing credibility. Lenhard et al. (2006) suggest that the transparency derived from using established disciplinary approaches early in the process can be used to establish scientific credibility that will support the later development of scientific legitimacy. They relate this to the incident mentioned above from the negotiations over the content of the Intergovernmental Panel on Climate Change's Second Assessment Report in 1995, where sceptics from the political side of OPEC withdrew their veto with the decisive factor in their decision being a frank appeal by the climatologists to the accepted methods and standards of their field.

Several observers of the evolution of science-based policies argue that the patterns they see are nothing like the traditional idea of a scientific result becoming the basis of a policy. They argue that the interactions between the two domains are so close that they should be thought of not only as intersecting, but even as co-evolving domains of human activity (van der Hove 2007). Jasanoff (2005) takes the 'co-production' argument even further, arguing that thinking of natural and social understandings being produced together provides explanatory power in many domains. Policy is so dependent on science, and the science used to support that policy is so dependent on direction and support from the policy process, that the two are developing a common product. Clark et al.'s (2002) results certainly suggest such joint activities have been a feature in the successful uptake of scientific knowledge into policy. Some even argue that the long-observed cultural distinctions between the two communities are weakening, and that scientists working in support of policy and policymakers are becoming part of a single culture creating projects that support the legitimacy of both (Lidskog and Sundqvist 2002).

Scientists working in these hybrid science-policy communities find themselves having to negotiate their credibility and legitimacy not only among policymakers but also among their fellow scientists (Shackley and Wynne 1996). The legitimacy problem raised by these patterns of co-production of science and policy, Shackley and Wynne (1996) argue, arises not so much from what might be the commonsense idea that the scientists' intellectual integrity has been 'bought' as it does from the ways that uncertainty is handled. Within such tight communities scientists learn to represent uncertainty in ways that never challenge the assumption found in the policy world that risks are tractable and that current institutions are capable of managing them.

Rothstein et al. (1999), for example, argue that co-production is a good term for describing the agrochemical review process in the UK, where a network of environmental toxicologists in both government and industry has emerged. In reaction to a number of costly regulatory decisions, the industry developed a substantial in-house scientific staff and gave them a lot of decision-making power over the development of new products. This network is relatively small and informal, and scientists move between government and industry positions. While this network has a positive impact on the saliency of the scientific information for policy development, there has been a substantial cost in credibility. Critics, especially elsewhere in Europe, have assailed the system for being too quick to accept new chemicals. The problem here may not be simply that the networking between the government and the industry is too tight, rather it is the exclusion of other stakeholders, most pointedly environmental NGOs, that creates the real costs in credibility.

One area where questions of legitimacy and saliency are often raised in respect to the science boundary is the question of scientists playing advocacy roles. Scientists have a right to participate in political interest groups just like anyone else, but there is a clear tension between this role and making a claim of producing a disinterested analysis that would have the highest legitimacy. Young (1989) argues that the kinds of claims scientists should be making and in what context forms a boundary issue. Is it possible for them to take off and put on their 'scientist hat' in different situations? Will they be given permission to do so? Scott et al. (1990) document three examples of people who tried to be neutral investigators in scientific controversies. In all three cases they were pulled into the controversy in spite of trying very hard not to be. Most often their attempts at even-handedness were interpreted by others in the controversy as an attack on the orthodox position. In most situations where scientists are acting as advocates, they are confronting opposing interests that are marshalling their own science and scientists. The result is an undermining of the credibility of the original scientists' expertise and that of science in general (Gill 2001). In the United States the emphasis on making decisions through confrontations between experts in legal fora has led to both an overblown idea of the importance of the 'expert' and unlimited scepticism towards particular expert judgements (Jasanoff 2002).

The discussion of scientists as advocates leads to real boundary difficulties even at a more abstract level of definition. Minnia and McPeake (2001) are two scientists struggling with the advocacy question. Their main concern seems to be that advocacy will lead to accusations and perceptions of bias. 'When the object is consonant with the personal values of the advocate, the appearance of bias enters; in other words the advocate appears to have a personal agenda' (Minnia and McPeake 2001, p. 6). They do not believe that in and of itself advocacy implies bias. Indeed, they differentiate between advocacy and 'extremism or irrationality', suggesting that it is in the latter case that bias is introduced while mere advocacy, paradoxically, can remain objective. In making this argument, they also suggest a dichotomy between a 'personal' agenda and a 'conservation' agenda, with the implication that a conservation agenda is more objective and scientific than a personal one would be. Doing science rather than advocacy, in their imagination, seems at least partly a question of maintaining a reasonable style of presentation and advocating for things that are generally accepted to be good rather than simply seen as good in someone's personal opinion. Indeed, they justify this by pointing to the mission statements of the wildlife agencies that employ scientists. This sort of reasoning about the science boundary is not uncommon, nor should it be rejected because it is so incoherent from a philosophical perspective. Within the context in which these scientists are operating, these distinctions may well be practical and serviceable.

In the lessons they compiled from their comparative analysis of the uptake of global assessments into the policy process, Cash et al. (2002)

placed the structuring of the science boundary in the centre. They found that science-based policy is most effective when use is made of 'boundary objects'. Examples of such objects would be models, indicators, collaborative research designs and data collection efforts that are used to provide a way to structure discussions between different perspectives. The most effective science policy efforts were found where boundaries were recognised and respected but not used as a rigid barrier to interaction. Participation was allowed across a recognised and selectively permeable boundary. The effective structuring of science-policy boundaries is delicate and complex. It depends on the nature of the problem to be addressed and the degree of consensus over policies and values as well as scientific facts (Engles et al. 2006).

Often, individual scientists find themselves trying to negotiate boundaries without the aid of any organisation or clear guidance on which standards should apply to what kinds of decisions (Waterton 2005). The need is for well-designed 'boundary organisations', i.e. institutions set up to straddle the boundary, facilitate communication and provide important mediating functions (Clark et al. 2002; Guston 1999, 2001c). These arrangements can be very simple; one example of an effective boundary organisation was simply a set of two-day sessions to familiarise policymakers with acid rain transfer and deposition models developed by scientists (Haas 2004). They can also be very complex; Dalrymple (2006), for example, argues that the CGIAR network of agricultural research institutions has evolved into a boundary organisation mediating between agricultural scientists and international development donors. Research suggests that the ways these organisations are set up is important. There are identifiable national styles in how governments seek to structure these interactions with consequential outcomes (Halffman 2005; Jasanoff 2002, 2005).

Niederberger (2005) reviews the policy successes and failures of the Advisory Group on Climate Change Research and Policy. This is a Swiss institution that was created to serve as an interface between science, government agencies and the public. She makes a distinction between a boundary organisation and an independent intermediary organisation, arguing that while the latter is basically about facilitating communication, the former should be understood as playing a deeper role. Where simply facilitating communications leaves intact the two separate sets of standards and forms of accountability found in science and policy institutions, co-production involves more fusion between the two. She argues that if the Advisory Group had been a true boundary organisation involved in co-production, it would have had a stronger policy impact. Niederberger's (2005) distinction seems to be more theoretical and philosophical than empirical; one could well imagine that the changes in policy themselves would be taken as evidence of the existence of the kind of co-production she is looking for.

Other researchers have not found boundary organisations emerging where they might be expected to. Waterton (2005) was surprised by the lack of reference to institutions resembling boundary organisations in spite of the fact that the scientists she was interviewing were often working at the policy-science interface. She argues that the scientists often find themselves having to negotiate the boundaries alone, and she found that they are sometimes doing this self-consciously and sometimes finding this painful and difficult. She suggests that ways of maintaining the science-policy boundaries are diverse and complex, and we should not allow the idea of boundary organisations to obscure this complexity.

Following Guston (2001c), the trick seems to be how to blur boundaries without making them too blurry. He argues that successful boundary organisations meet three criteria: they provide opportunities for jointly creating boundary objects or standards; they involve people from both policy and science, including professionals who can mediate between these groups; and, they exist on the boundary of the two worlds while having strong lines of accountability into both. This emphasis on accountability points us towards the critical question of how to achieve transparency at the science boundary.

Cash and Clark (2001) suggest that it is not effective to try to maintain a large distance between the science and policy based on some *a priori* definition of roles nor to place them so close together that there is no credible independence. What is required are structures that allow more complex interactions to evolve. They summarise their review of international scientific assessments with a concept of integration expressed in the dimensions of science, decision-making and scale. They further suggest that movement in these dimensions has been promoted on the technical side, while the social processes and institutional structures required have remained understudied. They conclude (2001, p. 11) that assessments that work have three common institutional features:

> Q 3.11 Multiple connections between researchers and decision-makers which cut across various political and organisational levels (polycentric networks);
> Boundary organisations – institutions that act as mediators between science and policy and across levels; and,
> Sustained and adaptive organisations that allow for iterated interactions between scientists and decision-makers.

The emphasis on cross-scale operations is a basic one that emerges frequently in ICES's case. The concept of polycentric networks that Cash and Clark (2001) introduce parenthetically is also an important idea that I have not yet touched on. The concept of polycentrism comes from political science and is most closely associated with the work of the Workshop in Political Theory and Policy Analysis at Indiana University, a group which enjoys considerable influence on natural resource social science (McGinnis 1999).

Ostrom (2001), who is trying to explain the sustainable management of forests, defines polycentric systems as

> Q 3.12 the organisation of small-, medium-, and large-scale democratic units that each may exercise considerable independence to make and enforce rules within a circumscribed scope of authority for a specified geographical area (Ostrom 2001, p. 2).

For Ostrom the strength of a polycentric governance mechanism is its ability to experiment with diverse approaches and provide for a range of responses to external shocks. While her focus is on sustainable governance, there is a clear analogy to science-based policy-making in conditions of uncertainty. Information processing, including the access of the smaller units to various knowledge sources, their ability to process feedback from policy changes and communicate these experiences to the parallel units, is a key part of the strength of polycentric polities. Polycentric approaches are also complex, redundant and hence expensive, and resistant to top-down steering (Ostrom 2001).

The polycentric networks Cash and Clark (2001) see in science for policy contribute for similar reasons. They are able to provide methodological coherence across scale levels while still allowing local specialisation; the redundancy of the system provides for multiple pathways to encourage innovation and flexibility. A polycentric network also facilitates stakeholder capacity building and involvement (Cash and Clark 2001). It is an institutional design that gives form to the continuously operating process of communicative rationality. It is also the appropriate institutional design for an adaptive system. A polycentric network has multiple sources of knowledge and capacities for responding to problems as they arise on the scale level they arise at. They are, as Cash and Clark (2001) tell us, structures that allow more complex interactions to respond to needs.

Polycentric networks also reflect current thinking about effective fisheries management in general that I would argue is broadly applicable to the EAFM. This is the idea of placing the burden of proof on those who wish to engage in economic activities and within a 'results-based' management structure. A belief that this is the appropriate framework for marine management strongly influences my assumptions about the kinds of institutions that can benefit from the reflections I am offering on the nature of transparency and the production of scientific advice. The basic idea is that someone who wants to become involved in some activity in the marine ecosystem, such as fishing, is licensed to do so through a results-based agreement. This means that, in exchange for an otherwise secure and high-quality right to a stream of benefits, they must demonstrate that they honour a series of limits on their ecosystem impacts, for example limits on the catch, bycatch, impacts on habitat, etc. This requires scientific processes both at the level of limit-setting and at the level of particular activity staying within those limits. The economic actors will demand transparency in the limit-setting process, and managers and other interested parties will demand transparency in the demonstration that limits have been honoured. This would require the development of science in an interactive way for a wide range of possible activities at different geographical scales. If this is the institutional path towards EAFM that is chosen, and I believe it reflects our best lessons from fisheries management, a polycentric network of boundary organisations is bound to emerge. When I am discussing science and negotiation processes in these pages, this is the kind of framework I have in mind.

The institutional form for effective adaptation seems to be polycentric networks of boundary organisations. These networks facilitate complex interactions that address emergent boundary issues at appropriate scales in a way that allows us to hold them accountable for both good science and good policy. Guston (2001c) tells us that successful boundary organisations have lines of accountability into both the science and policy communities, while current thinking on effective management institutions tells us that accountability to line managers and interested stakeholders is also critical. To be effective, such accountability requires transparency. So what we are after is a flexible, polycentric network of science-policy boundary organisations that are transparent both among themselves and to society at large. The critical institutional challenge that must be managed to achieve this goal I call the paradoxes of transparency.

3.3 The paradoxes of transparency

Chapter 1 argued that the basic social function of science is to achieve the maximum possible transparency of knowledge so that knowledge can be used to guide collective action. Transparency may seem like a fairly simple idea. It is certainly often treated like a simple idea, easily achieved through passing 'sunshine laws' or inviting someone to observe something. But achieving what we want to achieve with 'increased transparency' is rarely straightforward. Transparency is not just about observing something going on, it is about understanding what is going on. Understanding a complex situation requires that it be articulated or described clearly and honestly, yet the very techniques used for clarity and openness often seem to lead to transparency-induced opacity.

As I went about analyzing the case presented here, I found myself coming back frequently to this idea of the 'paradoxes of transparency'. Transparency presents itself as a paradox in many ways and situations because the techniques used to achieve it often undermine themselves. This self-negation is a common pattern. Because of this I thought of calling the idea the singular 'paradox of transparency'. I chose 'paradoxes' because I kept coming across so many different ways the self-negation happens, and they did not seem to share a general characteristic, save that they all made transparency more difficult. As this idea began to gel, I tried to give names to the various paradoxes of transparency. I offer four of them in this section. Many of the ideas behind them appear fairly frequently in the literature although, with the exception of the 'paradox of surveillance', they are not used as concepts for understanding transparency as such. In this section I describe each of these paradoxes in turn. It is quite possible that, if others find this idea of the paradoxes of transparency useful, they will be able to sketch out others and perhaps develop a more rational scheme than mine.

3.3.1 The paradox of precision and expertise

For the problems of creating a knowledge base for the EAFM, quantification is perhaps the main culprit behind transparency-induced opacity. The importance of quantification for science has long been recognised. Kant (1950/1783) argued that judgements of experience are always synthesised out of individual observations, while mathematical judgements are *a priori* and not based on experience. Quantitative methods are the paradigmatic form of science because they tap into the power of these *a priori* judgements. They begin with the accurate and consistent measurement of comparable units. When meaningful comparability and accurate measurement can be achieved, mathematical laws are mapped onto the phenomenon under study. Disputes must focus on either the comparability or the measurement of the units, and these two questions must be outlined with the greatest possible clarity if a result is to be acceptable as science. When this is achieved, this mapping of mathematical laws attains the greatest possible transparency of argument because it makes precise replication of results possible, in principle, if often not in practice (Collins and Pinch 1998).

Beyond science, the clarity of quantification is also critical for democratic governance. Porter (1995) links the rising importance of quantification to democracy. Decisions made by bureaucrats and politicians, who are open to public criticism, need to be objectively justified. Decisions made by aristocrats do not. The rule of law can no longer rely on aristocratic judgement and is forced to rely on accountancy, statistical and often scientific analysis to provide that justification, usually in the form of some numbers (Porter 1995).

The paradox itself arises from the fact that mathematical expertise is a difficult skill to acquire, as well as the fact that even those with the skills do not have the access and time required to assess the comparability or measurement of the relevant units. Finding mechanisms for collaboration and communication that are able to facilitate interaction across levels of mathematical skill is becoming very important in scientific activities and is taking up more and more scientists' time. This is certainly true among European fisheries scientists and is a problem that will be returned to frequently in this book.

3.3.2 The paradox of quantification and reification

The idea that quantification is about transparency is not paradoxical simply because mathematics require so much skill. To reify is to treat something abstract with an inappropriate level of concreteness. Reification is a constant danger when using numbers because the aura of objectivity that quantification lends to the measured often obscures aspects of its nature. Arithmetical language actually has a great deal in common with ordinary, informal language (Funtowicz and Ravetz 1990). The formality of the mathematical language is lost when expressed in normal speech, and numbers are used in ordinary speech to mean very different things. Funtowicz and Ravetz (1990) offer an analysis of numbers as a language based on the observation that a discussion about a formal language cannot take place in the language itself. They tell us a joke. A museum guide was showing some schoolchildren a collection of dinosaur skeletons. A little boy asked how old one particular dinosaur was and the guide said, '50 million and 12 years old'. The boy frowned 'That's a funny age?' 'Well yeah, but they told me it was 50 million years old when I started to work here 12 years ago, so it is just simple arithmetic!' Numbers are used in real speech to mean quite different things, and we laugh at the guard because in this situation that is obvious.

This informality inherent in discussing mathematics, however, is often obscured, and the aura of formality remains. Measuring something can change the way it is seen so much that it becomes a different thing. An obvious example is 'teaching to the test' in educational measurement, but this phenomenon does not require awareness of the measurement process. Porter (1995) argues that the concept of 'society' itself was in part a statistical construct and that people didn't really talk about 'society' before there were statistical handles to do so. Crime rates and unemployment rates made it possible to talk about a society involving collective responsibility instead of just the condition of individuals. The measured items often evolve into standardised categories where individual variation is obscured. Porter (1995, p. 28) quotes an official of the US Bureau of Standards: 'We have now reached the stage where there is a federally mandated method for measuring almost every physical, chemical or biological phenomenon.' Accommodating variation in measurement practices is impossible for a bureaucracy; even improvements are unhelpful unless made universal. But measurement is hard, and such standards also obscure this truth when reports contain spurious levels of precision. The transformation of landing figures into catch figures and thence into stock assessments is an example anyone in fisheries is familiar with. Measurement problems are easy to forget and ignore. In the American television crime drama 'Numbers', applied mathematics is transformed into an initially plausible form of magic by pretending that measurement is never a problem.

This combination of the hidden informality of numbers in speech and the ways that measurement transforms and invents phenomena has problematic consequences. A large part of the literature on local knowledge points to communication breakdowns that emerge when the day-to-day experiences of user groups are presented to them in the form of unfamiliar numerical categories. Roepstorff (2000) in his study of fisheries science in Greenland suggests that fishers 'focus on fish as a living being' and think of them as 'mass nouns', while the scientist sees the fish as a 'count noun', meaning that the individual fish is a representative of the stock in the sense that the stock is the arithmetic sum of the single fish.

Just as in daily speech numbers lose their precision while maintaining their aura, the quantitative presentation of material may also obscure underlying uncertainty. Mathematical models have been called a 'technology of hubris' when they imply that managed use is feasible where precaution might be the wiser choice. They overstate the known and downplay ignorance, uncertainty and conflict (Harremoes et al. 2001; Jasanoff 2002). In many policy contexts some stakeholders will be distrustful of the models at the same time that others are praising them (Jasanoff 1986). The greatest danger in the application of quantification to policy is when a goal is given special importance, simply because its attainment is easily measured.

Quantification creates the possibility for great transparency in demonstrating that something is true, while simultaneously undercutting that transparency with an aura of formality and objectivity, and a precision that is often not justified.

3.3.3 The paradox of surveillance

The surveillance paradox is well recognised in respect to negotiations and other processes seeking compromise and understanding among diverse parties. The world-famous Chatham House Rule, 'When a meeting, or part thereof, is held under the Chatham House Rule, participants are free to use the information received, but neither the identity nor the affiliation of the speaker(s), nor that of any other participant, may be revealed', is based on this understanding. When it is invoked, participants are much more open to information-sharing and search for compromise than when they have to account to their home constituency for every word they say. Especially the very formal and comprehensive transparency rules such as those found in policy-making in the United States hide as well as disclose important information; openness rules alone do not provide missing information, expose all the hidden assumptions and may even dampen novel interpretations of facts (Jasanoff 2002).

Degnbol and Wilson (2008) suggest that it is helpful to distinguish internal and external transparency. Internal transparency addresses the need for stakeholders to be transparent to each other so that areas for potential compromise and common ground can be found. This is not always easy when people involved in negotiations are having everything they say scrutinised by their constituencies. External transparency allows decision-making processes to be accountable to outsiders and finally to the public. Because of this, internal transparency must be limited, and attention must be paid to balancing the two kinds. The paradox is simply that in negotiations, transparency to others – i.e. to the public – reduces the internal transparency that allows negotiators to understand each other and search for possible compromise. The paradox of surveillance leads to complex dances in structuring complexity. One example is the discussion in Section 7.3.5 by the ICES delegate who was willing to have stakeholder observers at physical meetings but not video meetings, because it would be impossible to observe which stakeholders were present and how much they were influencing the scientists.

This is not something, however, that applies only to negotiations looking for compromise. It also applies to discussions of nature that are seeking consensus. Pictures of nature can be self-reinforcing. The facts that create the picture may not be wrong, but especially when they fit a particular political narrative, they can make it difficult for contradictory facts to be raised. This becomes a particular problem when facts take on a symbolic importance to particular groups. Groups have unifying symbols, and such symbols take many forms, including particular assumptions about natural facts. In fisheries, as elsewhere, belief in certain facts can act as an ideology that maintains and tests loyalty. This symbolic status shields these facts from criticism. The North Sea experience with illegal or 'black' landings for fish in the 1990s and early 2000s fits this pattern. Individual fishers were quite open about the extent of the problem privately – in confidential interviews, for example – but it was a long time before it could be honestly addressed in public fora. In fact, this did not really happen until stronger enforcement mechanisms were put in place, making it possible for fishers to comply without simply hurting their own finances while helping no one.

3.3.4 The paradox of scale

Often when one hears discussions of transparency, the speaker seems to be referring to making a process transparent to a very wide number of people, or even to the public as a whole. Transparency involving greater numbers of observers leads to greater accountability with respect to a wider range of values. The problem is that the larger the number of people, the greater the distortion of information introduced by the systems meant to make transparency possible. A good example would be the way the activities of one member of a polycentric network of boundary organisations are transparent to the rest of the network. The paradox creates substantial difficulties for both the larger group wishing to observe and the smaller group being observed. In a perhaps confusing way, this large and small distinction applies both up and down a hierarchy of institutions; it can mean a group of high-level decision-makers being held accountable to the entire interested public, as well as one particular fishing fleet being held accountable for their activities.

Discussing the paradox of scale with the detail I think it deserves means taking my last dive into explaining the basic concepts of the Communicative Systems Theory (CST) that has guided my research. As I discussed in Section 1.2, institutions are not only arenas of competition and contention, they also require the coordination of behaviour if they are to function. CST focuses on the communicative mechanisms that make such coordination possible. Up until now I have mainly discussed only one of these mechanisms, rational communication. Table 3.2 lists all the communication mechanisms used in institutions, and any institution will use a mixture of some of these mechanisms to coordinate action. The reader should be warned that while the basic ideas behind these mechanisms were inspired by Habermas's (1984, 1987) concepts, especially that of 'steering media', I have taken his ideas in a direction that he would likely not recognise. This means that any incoherence is my fault rather than his.[7]

The main difference between the mechanisms is the degree to which they are embedded in what Habermas (1984) calls the 'lifeworld', which is the rich mixture of shared background meanings that make communication possible. The mechanisms towards the bottom of Table 3.2 are based on simple mutual understandings that require little or no discussion. A can buy a candy from B, or drive through his country, without even sharing a language. If A has authority over B, A can get compliance from B with very little discussion. The mechanisms closer to the top of the table draw much more heavily on the shared lifeworld. The more embedded mechanisms motivate behaviour by convincing people that something is true or right, and these behaviours are much less predictable than the less embedded mechanisms which rely more on coercion. This is a fairly broad use of the word coercion, in that I apply it to choices in which people are faced with a take-it-or-leave-it decision, as well as situations in which direct sanctions are invoked to win compliance. The model in Table 3.2 is very similar to the state – market – civil society model, the first use of which is often credited to Polanyi (1957), and that is still widely used today (Jentoft and McCay 2003). The main difference is that it focuses on the mechanisms that handle the communication needed to coordinate action, not on the institutions themselves.

On lower scale levels, i.e. among fewer people, coordination is handled mainly by coming to a mutual understanding through rational communication.

On broader levels, the communications that allow coordination must be streamlined and made more predictable. This means that the orientation of the discussion towards convincing participants that something is true or right begins to break down. As the level on which the institution operates grows larger, coordination requires a presupposition in communications that the opinions and values of individual participants do not matter; hence the use of the word 'coercion'. This is the reality of what must be in place if behaviour is to be coordinated across broad-scale extents. The information-processing problem it presents, however, is a severe loss of richness and nuance in the information that the institution can identify and respond to and that rational communication makes most abundantly available. The result is a high potential for a systematic distortion of communication.

Table 3.2 Four continua characterising the CST communications mechanism

Communication mechanism	Embeddedness in lifeworld of communicative resources	How the mechanism elicits behaviour	Predictability of outcomes when mechanism is used	Scale	Examples of institutions that rely heavily on this mechanism for coordination
Rational communications Prestige Influence	More embedded in the lifeworld	Relies more on convincing	Less predictable outcomes	More effective on smaller scales	Science Social movements Local communities Social networks
Authority Money Right-of-way	Less embedded in the lifeworld	Relies more on constraining	More predictable outcomes	More effective on higher scales	Governments Markets Traffic

A similar table was originally published in Wilson (2003)

Adaptive ecosystem-based management requires above all a capacity for institutional learning, and the paradox of scale has direct implications for how institutions learn. Institutional learning across scales requires that knowledge flows both up and down, and this requires the translation of information into forms useful at another scale level. Such translation often distorts the information in the eyes of its producers, as is often seen when trying to use fishers' local ecological knowledge in management (Holm 2003). The information required can range from data about natural or socio-economic processes, to scientific findings of expert groups, to information about the basis and results of decisions made by lower-level management institutions. A process of diffusion from higher levels is necessary to establish and maintain a system for transparency and accountability. Then a process of concentration of information is necessary to gather and condense the information. Concentration involves packaging information into a form that will be useable on the higher scale level. The information that is the product of these packaging processes is not merely raw data and particular indicators; it includes findings, behaviours, scientific findings and group decisions. One of the interesting lessons for me from this case study is that this whole process requires many kinds of consistency (Section 7.3.1).

The packaging process involves four processes of the transformation of knowledge:

- Simplification means the filtering of the knowledge to leave only the knowledge that will be useful on the scale level receiving the knowledge. This might be thought of as increasing the ratio of signal to noise in the information;
- Abstraction means formatting the knowledge for applicability to wider situations than the one that produced the knowledge;
- Codification means categorising the knowledge within a symbol system that will be used to generate further knowledge through comparisons;
- Standardisation means simplifying, abstracting and codifying knowledge according to a common system.

The heart of the problem of institutional learning across scales is the distortion of communication (Habermas 1987). By communicative distortions I do not mean factual inaccuracies or poor interpretations of information. Nor do I mean the information-packaging processes as such, though it is in these packaging processes that the communicative distortions do their damage. Communicative distortions are severe deviations from the requirements of rational communication: that 1) people can effectively raise any claim without manipulation and 2) discussions of fact are separated from discussions of values and interests. In the diffusion process, claims are often blocked by heavy reliance on coercion-based communication mechanisms. This means, for example, that instructions about how to package information are formalised and even made into legal requirements, which in turn must be very strictly defined in order to be enforced. Enforcement institutions are not geared to take into account questions about how well these packaging instructions handle the information they are meant to communicate. The packaging can become routinised to the point of being ritualised, resulting in everyone being forced to take a cynical distance from the 'knowledge' being created. This formalisation and specificity of communicative content reduce the richness of the information being gathered, while at the same time allowing the people on the level doing the packaging to be selective about the information they are sending, because all that is really required is formal packaging. They cannot raise claims, and they cannot be effectively questioned. For example, the impacts of this blockage of questions and explanations accompanying required findings on scientists working in ICES expert groups is a central topic in Chapter 5.

Wilson and Degnbol (2002) outline an example of this kind of communicative distortion. A group of scientists shared a belief about the condition of a fish stock. The key assumption of the model they chose to define formally as the 'best available science' for this fish stock, however, was the precise opposite of what they believed to be true. The issue here from the perspective of institutional analysis is not how well the model they selected reflected the real situation in the ocean, as their original belief could also have been wrong. The issue is that a group of scientists was forced by a set of institutional imperatives to say the opposite of what they believed; they were effectively blocked from raising their claim about what they actually thought was going on. What blocked them was a combination of legal requirements for particular model parameters and the characteristics of the institutional peer review process they were operating under. These requirements were put in place precisely to help a large-scale science-policy community ensure the transparency of scientific decisions.

These distortions of communication within marine management institutions can be hypothesised to operate in particular directions. I would suggest that they would tend to make management problems appear more tractable than they are. This is because during the simplification and standardisation processes, information about local nuances will be excluded, and this will hide problems. They would also tend to make their management seem more effective than it actually is, because the same information that is formulated through institutional imperatives is used to evaluate the effectiveness of the institution. Furthermore, they may tend to hide linkages between scale levels because the packaging of knowledge from the lower level is given form by questions that are driven by the level and resolution at which the questions are posed. They may also be subject to a positive feedback loop. The responses by higher-level institutions to the results of communicative distortions are to create even more stringent packaging requirements that then increase the distortions.

The paradox of scale is a particularly dangerous challenge when trying to figure out how to structure transparent institutions.

These four paradoxes mean that transparency can never be taken for granted. It must always be fine-tuned, and as a result transparency can almost never be brought about simply by setting up rules. Too often a simplistic approach to transparency will simply end up making matters worse.

3.4 Summary of the theoretical discussion

These first three chapters trace a theoretical path towards understanding science for an ecosystem approach to environmental management. My broadest starting point has been an approach to human ecology that sees the social system as a communicative system governed by shared meanings functioning within a set of environmental systems governed by natural laws. Given this orientation, CST is an informative way to think about how the social system works. The ecosystem approach is thought of as the problem of the social system adapting through communicative processes to the changing realities of its physical environment.

Such adaptation requires gathering information about the natural systems and making that information the basis of collective action. Looked at through the lens of CST, science is the best kind of information for supporting collective action, not so much because it can produce objective knowledge, but rather because science is rooted in a radical form of transparency. Coming to a mutual understanding of nature on which we can base effective collective action requires us to explain to each other how we know what we say we know, and science is the institution that is the most dedicated to such explanations. Science makes heavy use of the rules of 'rational communication', the basic rules that communications must follow to make mutual understanding possible. These rules are that no argument or person is excluded ahead of time from the discussion, that there is no manipulation in the communication, and that the discussions of fact, values and interests are treated differently because they presuppose different goals. A discussion of fact is oriented towards reaching a consensus about what is true, while a discussion of values and interests is oriented towards reaching a fair compromise.

Chapter 2 explored some of the recent literature on science as an institution within a changing world. Science is under constant pressure. Many interests seek to 'inflate the science boundary' by defining more and more things as technical problems demanding scientific solutions. Inflating the boundary reduces science's ability to fulfil its role of facilitating transparent claims about nature by mixing questions of fact with questions of values and interests. The driver of this inflation is the common desire to have one's own goals treated as technical necessities, a pattern found frequently within decision-making bureaucracies that need to point to something external and objective to justify their decisions. The use of science within legal processes forces it to answer questions defined by the law rather than by natural processes to deal with alien standards of proof. Within research institutions and universities the ever greater demand that science serve primarily social needs, rather than the generation of new knowledge for its own sake, has become pervasive enough that some observers are calling the phenomena 'Mode Two' science. More science is being done under client-driven, consulting-type arrangements than in the past. This has limited the effectiveness of traditional mechanisms for quality control such as peer review.

Within an EAFM, as well as many other areas of social life, the greatest challenge for science is how to respond to the need for clear descriptions of nature under conditions of high uncertainty. An emerging response to this uncertainty has been an increased reliance on partnerships with experts and even lay people outside of the traditional scientific disciplines. Some have seen this movement as undermining, or even making redundant or inappropriate, the separation of discussions of scientific fact from discussions of values and interests. I suggest, to the contrary, that these linkages make this need more urgent than ever. The error is rather trying to make scientists, with their skills as transparency experts, into an objective 'other' somehow sitting outside the policy discussions. Scientists can play an important role in upholding communicative rationality in policy processes. This cannot be achieved if we simply decide that irrationality is inevitable.

Chapter 3 examined studies that have focussed on the practice of creating scientific advice for policy support. This research has found that three things seem to have the highest influence on the degree to which science will be taken up into a policy process. The first is credibility, meaning the basic scientific quality and believability of the result. The second is saliency, meaning the relevance of the result to the questions that the policy process needs and is ready to have answered. The third is legitimacy, meaning the believability that the result derives from the social context surrounding the scientific process. This can involve many factors. Examples include how plausible the result is in light of expectations, the prestige of the scientists involved, or the source of the money that paid for the science.

The studies addressing advice generation echo the discussion of uncertainty in their focus on the relationship between the scientists and the consumers of science. While the science boundary must be protected from 'inflationary' pressures in order to preserve credibility, it cannot be made too rigid in order to preserve saliency and legitimacy. The emerging institutional form, identified in many studies, is the 'boundary organisation' in which scientists work together with policymakers, managers and stakeholders to develop mutually useful joint products. In an adaptive approach to ecosystem management, flexible 'polycentric networks' of boundary organisations are emerging that are able to respond to issues that arise, operating on scales appropriate to the problem. This is a phenomenon that is already noticeable in European marine management.

The overall framework I envision when discussing potential improvements in scientific institutions throughout the book is results-based management. Under such a framework, permission for a particular activity depends on demonstrating that the impact on the ecosystem stays within scientifically identified limits. This requires accountability and transparency through interactive science on both the level of limit-setting and limit-honouring. Such processes will need to be carried out on multiple scales, for a wide range of activities and potential impacts.

How is accountability possible within polycentric networks facilitating a results-based approach to the EAFM? How can managers and the broader interested public know what is going on within all these complex marine management challenges? As I have examined this question within the ICES case, I have come to think of this challenge in terms of the paradoxes of transparency: the idea that attempts to establish transparency often undermine themselves. I have sketched out four such paradoxical patterns:

the way that the precision needed for clear explanation leads to a need for expertise and background knowledge that excludes most observers; the way that the quantification needed for clear accountability of findings and achievements can obscure and even change the substantive meaning of what is being measured; the way that demands that interactions be open to the public make people unable to be transparent to negotiating partners in ways that discover potential compromises; and the way that attempts to make things transparent to larger numbers of people can distort the information they need to grasp what is going on. The idea of the paradoxes of transparency, I believe, helps explain quite a bit about the challenges that ICES has been grappling with when providing science for the Common Fisheries Policy.

This briefly summarises the theoretical questions I have brought to my examination of ICES. Now I turn to the meat of the matter – the efforts that are going on to produce science for the CFP as it moves towards an EAFM. I will return briefly to these general theoretical reflections in the conclusion.

4. The Science Assembly System for European fisheries management

With contributions from Rikke Becker Jacobsen

When carrying out a case study, it is always an interesting question what the borders of the case actually are. During the course of this study, the way I was thinking about the object of my research would subtly shift. In the beginning it was the institutions involved in the production and use of scientific advice in support of the Common Fisheries Policy (CFP). That way of thinking about the case eventually became both too broad and too narrow: too broad because there are so many such institutions that merely describing them in a balanced fashion would be an entire, rather boring, book; too narrow because of all the influences from factors outside of the EU. I finally settled on ICES as the focus of the case study. Formally speaking, ICES is a single organisation, and although it is also a network without distinct borders, its extent is at least clear enough for discussion. More importantly, ICES must respond to demands for formulating advice by drawing on this broad and varied network. This affords opportunities for observing the interface between the various understandings of science, from pure academic science through legalistically conceived mandated science. As a result of the initial focus of the research, however, most of the discussion of ICES's external relationships relates to the CFP. Limitations in my own knowledge force me to lump all the non-EU users of ICES's advice together as the 'other clients'; they play a role in the analysis, but it is a secondary one.

The case study is covered in four chapters. This chapter describes the stage on which ICES performs. It begins with an overview of the advice system for marine and fisheries management in Europe (Figure 4.1). ICES provides advice for management in the Northeast Atlantic Ocean, including the Barents, Baltic, North and Norwegian Seas, and their advice is used by the governments of the EU, Iceland, Norway and Russia, as well as several international commissions. Marine and fisheries management on the Mediterranean Sea involves the European Union, but under a completely different system rarely involving ICES. The other important line around this stage is the one between fisheries management and broader marine management. The case study focusses on fisheries management, and a good deal of the discussion is confined to Holm and Nielsen's (2004) TAC Machine because the advice system mainly responds to the imperative of setting and distributing quotas of fish. However, the central question the case study asks is how the system can move beyond these narrow confines to the EAFM, which does incorporate the broader marine management. At the moment, adding DG Environment to Figure 4.1 would add more clutter than substance. That will likely not be true five years from now. The second half of this chapter is a discussion of the problem of scientific advice as it arises within the European fisheries management system and some of the ways people are seeking to change the advice provision system.

The case study then continues with Chapter 5 focussing on the situation and attitudes of individual scientists working within this system. It draws mainly on survey data comparing fisheries scientists heavily engaged in fisheries advice production with their peers. Chapters 6 and 7 focus on ICES as an organisation. Chapter 6 looks at the question of the challenges ICES faces in mobilising science in support of an EAFM. Chapter 7 reviews a recent reorganisation of the ICES Advisory Programme, examining how the effort to develop an EAFM influenced this restructuring, among other things.

4.1 An overview of the advice system

The advice production system for European fisheries management is part of a larger, self-reinforcing system of political necessity, scientific technique and infrastructure development. It is a hard thing to change. Holm and Nielsen (2004) identify three institutional aspects of this 'TAC Machine' that make it resistant to change, even in directions that many managers, scientists, conservationist and fishers would agree upon.

First, the TAC Machine is based on a single-species rather than a multi-species approach and creates incentives to both continue to use a single-species approach year after year and to use a similar approach for all the species within the system. Institutional forces reinforcing single species by single-species management impede two current reform priorities that seek to come at management from a broader perspective. At the simpler level, they present a challenge to long-term, mixed-fisheries management, although one could see a single-species approach as a building block that can contribute to mixed fisheries management. At a more complex level, it is difficult to reconcile species-by-species management with an ecosystem-based approach to management (Holm and Nielsen 2004).

Second, both the biology of most fish and the organisation of political life lend themselves well to a yearly rhythm. Age-based models producing a TAC (total allowable catch) for each species, which then can be easily divided into quotas, are a natural approach to producing science to support the system. The institutional forces that reinforce one particular modelling approach have channelled the handling of uncertainty into a narrow path that has proven inadequate in the long term. This inadequacy is especially evident in the way that the precautionary approach has been implemented in European fisheries management (Holm and Nielsen 2004).

Third, the TAC Machine system allows and indeed requires making scientific practice routine with standardised data as input and a standardised product as output. It is based on an illusion of a clear division of labour between science and management that obscures the many difficulties with this division discussed in Section 3.2. This routine-based system, in turn, depends on a very extensive, expensive and difficult to manage data collection system. Institutional forces reinforcing a huge data infrastructure are not in and of themselves problematic; fisheries management is always going to require a lot of data. However, both the rigid division of labour between science and management and the huge data-gathering system make it difficult to mobilise appropriate expertise and data to address complex management problems that exist on multiple scales (Holm and Nielsen 2004).

Others have noted this institutional inertia. In the EASE project report, fisheries scientists noted that 'resources have been deployed to service the annual advice system, rather than developing or refining methods to match data, knowledge and advice needs' (2007, p. 14). They estimated the cost of scientific advisory work and related research activities in Europe in 2002 at about 78 million euros, with 59 million euros spent on data collection and directly advice-related work and 19 million euros on research conducted by fisheries research institutes. Just the international meetings that provide science in support of the CFP entail 4500 person-days annually (EASE 2007).

This section begins with a brief overall picture of the institutions that make up this overall system. Then I turn to more detailed discussions of some of the more important nexuses within the system: the data-gathering system; the National Fisheries Institutes (NFIs); ICES; the Scientific Technical and Economic Committee for Fisheries (STECF); and the Directorate General for Maritime Affairs and Fisheries (DG MARE).

4.1.1 The main parts of the system

The outline of the system is shown in Figure 4.1. On the left-hand side, symbolised by circles, are the only parts of the system that actually touch the sea, the fishing vessels and the research survey vessels. Both of these feed data into the NFIs. Survey vessels do so through scientific protocols, the fishing fleets do so through mandated data collection procedures outlined by the EU's Data Collection Regulation. This system is carried out by the member states and operates on national and regional levels. The advice provision system moves up to the European Level as the NFIs supply their experts, armed with their country's data, to the ICES expert groups (see Figure 4.2). These expert groups are the entry level for the ICES system, which takes the results of these groups through a series of stages until the official ICES advice emerges. As the NFIs employ these people and pay for their participation in the expert groups, they exercise an important indirect control over these central ICES activities. They also hold considerable formal control over ICES because of their membership on the ICES Council.

What exactly they are controlling, and how much they are really controlling it, are open questions. ICES is a lot of things. It is an intergovernmental scientific organisation which is charged with producing scientific advice to be used in official decision-making. It is a primary professional organisation for marine and fisheries scientists in Europe and an important secondary organisation for North American scientists. It is a building in Copenhagen where a professional staff processes data, edits kilograms of scientific advice, and constantly hosts meetings on many subjects. It is also a loose network where approximately 1600 scientists work together to address scientific questions, some of which they are eager to examine and some of which they wish had never been asked.

Formally, ICES is an intergovernmental organisation with 20 member countries. Each member country has two delegates on the Council, and the Council is the highest decision-making level in ICES. It is chaired by the ICES President. The two delegates from each country are usually the Director of the country's NFI and someone from the relevant central ministry. The Directors are the more active of these two groups, and all current members of the Bureau, an executive committee elected by and from the delegates, are NFI Directors.

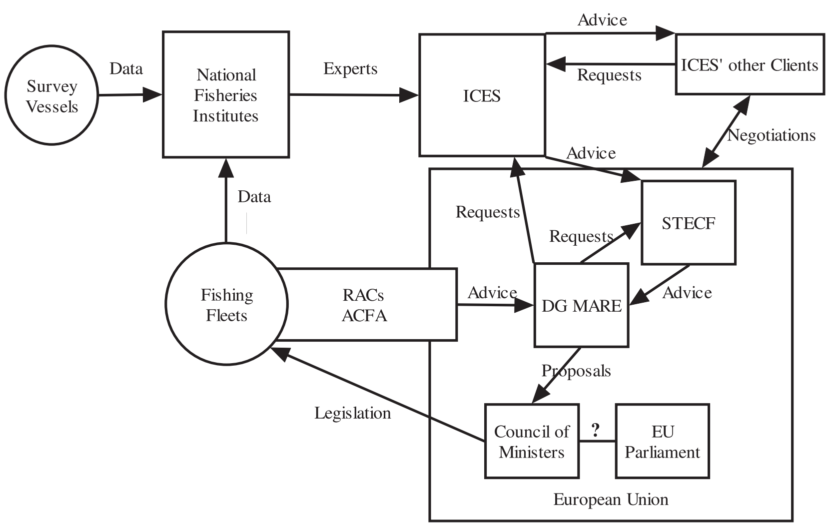

Figure 4.1 Overview of the advice system for marine and fisheries management in Europe

ICES must also respond to its clients. The biggest of them is the European Commission, mainly in respect to the Directorate General of Maritime Affairs and Fisheries (DG MARE), but ICES also receives some advice requests from DG Environment. The regular clients include the government of Norway, the OSPAR and HELCOM Conventions, the North East Atlantic Fisheries Commission (NEAFC), and the North Atlantic Salmon Conservation Organization (NASCO).

ICES is very dependent on the willingness of scientists to make their expertise available. It is true that many scientists, even while sometimes referring to themselves as volunteers, participate in ICES expert groups because they are assigned to do so by their laboratories. However, the degree of enthusiasm with which they do so is very much under their own control. Furthermore, many other scientists participate in expert groups solely because of their own interest in the issue or the scientific subject matter. As explored in some detail in the coming chapters, mobilising these people's expertise is a real challenge, and the attitudes of the constituent scientists make up an important part of the political environment of ICES.

The next section of Figure 4.1 is the box marked European Union that contains a number of sub-sections. The central actor, although not the final decision-maker, is DG MARE. Like other clients, DG MARE directs funds and requests to ICES. The amount of money involved is not huge; the current Memorandum of Understanding between ICES and DG MARE puts the figure at approximately 750,000 euros for the recurring advice. The group at DG MARE that actually uses the advice, however, does not formally receive the advice directly from ICES. Through a fairly recent change in procedures, official advice must pass through the Scientific, Technical and Economic Committee for Fisheries (STECF). Their job is to receive the ICES advice, review it, add economic information as appropriate, and pass it on to DG MARE. An important difference between STECF and ICES is that STECF is DG MARE's own body, and its budget is under the direct control of DG MARE. An important similarity between STECF and ICES is that these are very often the same scientists, who are getting their *per diems* paid by a different public body. DG MARE also has the option to send requests directly to STECF, an option that it exercises frequently for certain types of short-term and very specific requests.

In addition to the scientific advice from STECF, DG MARE also considers political and social advice from two stakeholder structures. The Regional Advisory Council (RAC) system was set up by the European Council in 2002 (CEC 2002) and given practical form in 2004 (CEC 2004). They are meant to be stakeholder fora bringing together divergent groups to try to produce a consensus approach to fisheries management problems to help meet management goals. There are seven such RACs; five correspond to the geographical level of a shared sea, the other two relate to particularly far-ranging fisheries. A RAC is required by the Council Decision (CEC 2004) to allocate two-thirds of the seats on its executive council to the harvesting sector and one-third to 'other interest groups affected by the Common Fisheries Policy' (256/17). The other group, the Advisory Committee on Fisheries and Aquaculture (ACFA), is similar in purpose to the RACs, but operates at a European level representing mainly cross-European groups. ACFA is even more strongly weighted towards economic interests than the RACs are.

DG MARE's output is proposals to the Council of Ministers for decision-making. The decision-making power has recently been expanded by the Lisbon Treaty to include a 'co-decision' by the European Parliament, but how this will influence the process is unclear, and the Commissioner's current position is that it will not influence the settings of TACs. Up until now their role in fisheries has been advisory. The Council of Ministers, in hectic end-of-the-year sessions, decides on the fisheries legislation for the coming year. While they do issue a number of 'technical measures', i.e. rules about how to catch fish and some restrictions on how much fishing will be allowed, the most important decisions are related to setting the TACs for each species and their associated member state quotas. The quotas are governed by the relative-stability rule, under which the quota allocation is based on the proportion of the catch which the member states enjoyed before joining the CFP.

Under the CFP the member states are to receive an allocation, and it is up to them how they distribute that allocation to their fishers. Member states are also responsible for passing the relevant laws and regulations, monitoring and enforcing compliance. The EU role in this area is growing rapidly. This includes not just the fisheries management measures but the data-gathering activities as well. Monitoring and enforcement at the member-state level has been the weakest part of the CFP. But data-gathering, behaviour-monitoring and enforcement have improved significantly in the past few years, due to the tightening of standards by the European Union as well as a growing influence of conservation groups at the member-state level in response to the CFP's poor conservation record.

4.1.2 Gathering the data for fisheries advice

The various scientific survey vessels owned and operated by the NFIs produce the valuable 'fisheries independent data' that constitute the most important input, at times even the only empirical input, to the stock assessment models. The cost of scientific surveys added up to 34 million euros, making surveys the largest single expenditure in the entire scientific advice system. 'Fisheries dependent data' that comes mainly from sampling landings constitutes the second largest expenditure of 19 million euros. As discussed below, the quality of these data has been the single most prominent issue in European fisheries management. These data-gathering expenditures can be compared to the 7 million euros that pays for all the analysis, stock assessments and advisory work (EASE 2007). These resources have been invested somewhat unevenly among the different fish stocks, with the northern area, including the North Sea and Baltic demersal stocks, attracting the most resources. Some 40% of the resources was spent on these stocks, while the rest of the demersal stocks received 20%, pelagic stocks 30%, and deep sea stocks 5% (EASE 2007).

In 2000 the European Union established a common regulation for the collection and management of fisheries data for all member states. This regulation is known as the Data Collection Regulation (DCR). The DCR is the answer to a need for harmonised procedures for data collection that remain the same from one year to the next. It is meant to provide an overview of the activities of the fishing fleets, total catches, price trends and the economic situation in the sector. The DCR has undergone some developments since its first adoption in 2000. The reason for this is especially the 2002 reform of the Common Fisheries Policy through which the EU management of fisheries has been moved from a concern with how to manage individual stocks to a concern with how to manage the social and economic units of European fisheries and fleets. The main trend within the development of the DCR has been towards meeting the need for multi-species fleet- and area-based advice. It is anticipated that from 2009, data will be collected by fleet and fishery, rather than by stock.

In March 2008 a new version of the DCR was adopted that is intended to prepare the system for the EAFM. The new DCR introduces satellite monitoring of vessels. It expands the EU-funded data collection programmes from one to three years. It places more emphasis on the gathering of social and economic data so as to provide a basis for impact assessment of new legislation and to allow monitoring of the performance of the European fleet. Paragraph 8 in the proposal for the new version states as follows:

> Q 4.1 Data collected for the purposes of scientific evaluation should include information on fleets and their activities, biological data covering catches, including discards, survey information on fish stocks and the environmental impact that may be caused by fisheries on the marine ecosystem. It should also include data explaining price formation and other data which may facilitate an assessment of the economic situation of fishing enterprises, aquaculture and the processing industry, and of employment trends in these sectors (CEC 2007, p. 9).

To many fisheries scientists, the data directive guidelines of 2002 actually implied a decrease in international coordination. The data directive had established sampling schemes for all of the EU member states to adhere to, but by doing that, it had also de-emphasised former cooperative sampling programmes. Since that time a new system has been created under more direct EU auspices. New internal mechanisms were needed to ensure the international coordination of sampling. The sampling of discards was one of the areas which proved to be a challenge. While all member states should engage in a national sampling programme, it was left to the individual member states to decide how to estimate the discards. There was no international coordination in place to ensure that sampling occurred from all the fleets fishing any particular stock, and this in turn prevented the full utilisation of discard estimates into the stock assessments. The international coordination of landing samplings was complicated further by the very heterogeneity of the authorities responsible for enforcement and inspection (e.g. the navy, the police, customs, etc.). The scientific methodology relating to the length and ageing of landings was also in need of international coordination as some laboratories had no established expertise. Tools were required for analyzing and storing international sampling results, and the division of responsibilities for the management and development of the international sampling system needed to be clarified.

These issues are being addressed to some degree. The concerns in the previous paragraph were themes in interviews done in 2003 and 2004. International coordination is increasing, especially through programmes designed at the regional (e.g. North Sea) level. Here is a respondent from the data programme at DG MARE speaking in 2007:

> Q 4.2 Interviewer: How much of your time do you spend talking to people in member state ministries? Respondent: Rarely, I would say. With the data collection system we have regular meetings with member states, normally when we are evaluating their national programmes. We are not in the process of evaluating their national programmes in 2008. We have bilateral meetings, and my unit is participating in all this regional coordination work. This is a good occasion where you meet not the higher level decision-makers but the people involved in implementation. These are observers and people coordinating observers, people dealing with data collection issues.

Data for fisheries advice are not available in either the quality or quantity DG MARE would like to see. According to one of our respondents from DG MARE, they had the sense that processing data for advice is not attractive to the employees of the national fisheries institutes. They thought this true even to the extent that those who do it are not seen as scientists, and there is little money available for these activities in comparison with research projects. One point of the new initiatives was to make some investments to change this situation. The impact of stock assessment work on scientists' careers and working conditions is the central theme of Chapter 5.

A respondent at DG MARE:

> Q 4.3 The data collection framework has been running for five years now. If you listen to an end user like ICES, they say that the programme is functioning well. It has stabilised the flow of data to the assessment work. That does not always apply to the quality of the data. That is a problem, and most of the problems are with missing landings, misreporting, black landings or whatever you want to call it.

Fisheries data also raise questions of confidentiality and control. Because of the concerns of member state governments, reflecting in turn the concerns of their fishers, the current legislation does not allow DG MARE to have its own databases, and they are only allowed to use data for 20 days. Getting data is actually easier for scientists when they request it wearing an ICES hat than if they request it wearing an STECF hat. If they want information for STECF, the request has to go from DG MARE to the Ministry, but if they want it for ICES, they can just make a request directly to the NFIs. DG MARE can also make the data from fisheries inspections available to ICES, although with a number of restrictions.

An agreement between DG MARE and ICES about the use of community fisheries inspections data for scientific purposes was described by an ICES official. It is a long and complex document. It specifies exactly what DG MARE is willing to provide, requires that ICES not publish the data and restricts access to members of relevant expert groups. Furthermore, ICES must only analyse the data for purposes of assessing catch statistics for assessment and advisory purposes, and in ways that are restricted in terms of geographical resolution. ICES is also restricted from making comments about individual member states, let alone individual fishing vessels. Some member states also specifically bar scientists from access to data from the vessel monitoring system (VMS) satellite tracking.

While efforts at standardisation have been critical, problems with fisheries data are extensive and often politically charged. The most basic issue is the cooperation of fishers with the data programmes and the related problem that the fish catch is not the same thing as the recorded fish landings. This happens because of discarding of fish at sea or 'black' landing of the fish. There are many possible reasons for both problems (Wilson 2000). Very briefly, discarding may be because of 'high-grading', throwing out fish caught earlier to accommodate more valuable fish caught later, or 'regulatory discards' where fish are caught that are illegal to land for quota or other reasons. 'Black' landings may be a way to avoid either fishing regulations or taxes. At the ACFM meeting in September 2004, the issue of Eastern Baltic cod was discussed as a particularly difficult example. Landings from this stock were estimated to be underreported by as much as 35%. Yet the scientists have been concerned that if they pushed this issue, they would get even less cooperation from the industry. These concerns are reflected on the member-state level as well. Some countries in the area have a rule that scientists can only use official data, so they created a category called 'unallocated' data to detach the estimated misreporting from national data. When this decision was reviewed, these scientists received comments back that this category was not acceptable. This kind of restriction means that the data cannot be evaluated. It creates 'black boxes' in the system and undermines scientific credibility of the scientists involved, even up to the ICES level. One scientist complained that if one names a specific country in trying to address misreporting, this will just make the situation worse. Indeed, this person suggested that this worsening may even extend to relationships with the scientists from that country. Another scientist from the region said that for these reasons, they never mention the countries where misreporting is undermining the value of the data. Very similar problems have been found in the North Sea and elsewhere.

Data on discards and bycatch are both particularly sensitive. One scientist we interviewed was charged with putting together discard information so that it would be commonly available to scientists throughout ICES. This proved to be a problem because various countries did not submit their information while other countries submitted all that they had. The database they created was very patchy, although he argues that the new EU regulations have improved this situation. Three countries had already collected the discard data as part of a research project, but when these data were desired by an ICES study group, one country would not provide it because of a disagreement with their fishing industry about whether the results were representative.

A good example of the problems around data and fishers' cooperation is Scotland's experience. Scotland has been a particular place of contention with respect to discard information. Scotland was the earliest place to begin a regular programme of hosting observers on board fishing vessels to gather such data. Once the data were made available, they drew attention from both the EU and local conservation NGOs that increased pressure on the industry. This led in turn to a feeling among the industry that the data that they were helping to provide were being used against them in a way that penalised them more than other fishers who had not provided the data. In at least one case this led one of the fishers' organisations to withdraw from providing more data. Interestingly, Scottish scientists felt some kinship with the industry on this issue. On two separate occasions Scottish scientists in interviews indicated that Scotland had made more than their fair contribution to data-gathering, though their reaction was that it was high time other countries started picking up their slack rather than that Scotland should pull back.

A scientist we interviewed in 2004 who works directly with the Scottish observer programme reported that it is becoming more difficult to solicit cooperation. This is something that has built up gradually, but the quota setting for 2003 seemed to have been a watershed event. At that point it became clear that fishers' organisations no longer wanted to provide cooperation at the previous levels. It was not the discards issue *per se*, but a general attitude towards government establishments that contribute towards quotas and management. The data went through the labs, and the fishers felt that they were only providing information that would be used against them. The observer programme still gets cooperation; our respondent argued that the personal approach was still the most important. But this withdrawal of cooperation has had an impact on the observers' morale, and these are not easy positions to fill or train people for. It has also, on rare occasions, meant a hole in the sampling scheme; he estimated roughly that five trips failed in 2003 out of a total of 70. The most extreme expression of industry refusal to cooperate with data collection took place in Northern Ireland where fisheries officers were kept from sampling landings through threats of violence. A scientist involved in the expert group says that this activity has had an impact on the Irish Sea stock assessments.

The increased emphasis on data-gathering and especially investments in general fisheries enforcement should begin to make a difference. For one thing, video surveillance on fishing boats is beginning to appear rapidly. Enforcement is a critical support in bringing about any kind of cooperation. Even fishers with the best of intentions are forced to cheat in a system where there is no enforcement; otherwise they hurt their own families while benefiting no one. Earlier in this decade, when the enforcement and data problems were finally beginning to get considerable attention, a pattern in interviews with fishers was for them to begin by talking about how much they had been misreporting their landings, and how much they wanted to see a system where they did not have to. Hopefully, such a system is beginning to be put in place.

4.1.3 The National Fisheries Institutes

In this chapter and throughout this book I refer to what may be the most important institutions in the whole advisory process using the single term 'National Fisheries Institutes' or more often NFIs. I thought it was important here to offer some very short descriptions of a few of these organisations. What follows is not an in-depth history or analysis, indeed the information comes almost entirely from the institutes' own websites. Such an analysis is not necessary for adequately describing the case at hand. What I hope this section does do is give the reader an idea of both the similarities and diversity of these entities that I for the most part treat almost as a unit. They have diverse cultures, quite proud scientific histories, and different organisational structures, while also having both similar missions and a need to respond to similar stakeholder groups. They are the employers of most of the scientists interviewed for the book, and hence their primary institutional identity. Some are government entities fully encompassed by the ministry in charge of fisheries, others are parts of universities. Both DTU Aqua and IMARES recently changed from independent research institutes to university research centres. After the short descriptions some general remarks about the NFIs complete the section.

The Institute of Marine Research in Bergen, Norway

The Institute of Marine Research (IMR) is the largest centre for marine science in Norway. The scientific focus of IMR is on aquaculture and ecosystems. The main task is to advise Norwegian authorities on ecosystems and aquaculture, and half of IMR's revenue comes from the Norwegian Ministry of Fisheries and Coastal Affairs through earmarked advisory contracts. IMR organises its marine and ecological research into 19 research groups, and together they cover a wide range of research on e.g. different maritime species, health, animal welfare, genetics, oceanography and data maintenance. The subjects of fisheries dynamics and fish capture constitute two fields of their own within this structure of 19 research groups. The group of fisheries dynamics studies capture rates, effort and fleet development, collects fishery-dependent data and performs monitoring services. The fish capture group, for its part, seeks to develop capture methods that are at once environment- and resource-friendly and energy-efficient. IMR has recently started to work more closely with the industry in order to improve the data collection and information flow between fishermen and scientists. This is being done through their 'Reference Fleet' programme where IMR pays a small group of fishermen to provide detailed information on their fishing activity and catches.

IMR's history as an institution goes back to 1860 with a parliamentary proposal to fund practical-scientific fishery research in 1859 (Nordstrand 2000; Schwach 2000). This led to the first investigations of herring and cod fisheries in the early 1860s. In 1900 the Norwegian parliament established its first fishing directorate and divided it into a practical-administrative and a scientific branch. The scientific branch was reorganised in 1947 and developed into the institute named IMR. IMR remained part of the Norwegian fishery directory until 1989, when the former scientific branch, now an independent institution, split from the administrative one. Today, the two old branches continue to cooperate, and they share the same physical facilities.

Due to its national advisory function, the main geographical areas of research interest are the Barents Sea, the Norwegian Sea, the North Sea and the Norwegian coastline. In addition to the many activities near home, IMR also supports fisheries research and management in developing countries. Through their 'Centre for Development Cooperation in Fisheries' IMR has worked with Asian, African and Latin-American countries and describes itself as heavily engaged in development aid activities. IMR also engages in international research activities which are not attached to development aid. It advises international organisations and commissions and works with international sister institutions including 50 years of cooperation with the Russian PINRO Institute.

The Centre for Environment, Fisheries and Aquaculture Science in Lowestoft, England

For England and Wales, CEFAS acts as the main aquatic scientific research and consultancy centre. It provides the UK government and other clients with research, advisory, monitoring, consultancy and training services. The goal of CEFAS's scientific endeavour is to help preserve the aquatic environment, develop sustainable management and protect the public from aquatic contaminants. CEFAS is engaged in a broad range of research and advisory activities within both fisheries and the broader field of marine science.

CEFAS's research area is divided into the three categories of ecosystem interactions, organism health and resource management. Fisheries research is a subfield of its own under the latter and refers primarily to the exploration and assessment of the impacts of human activity and management on aquatic resources. Back in 2003 CEFAS began to work directly together with British fishermen due to a funded initiative by the UK government. Working together with fishermen organisations, CEFAS is building up relationships between fishermen and scientists in scientific cooperation.

What is today known as the 'Centre for Environment, Fisheries and Aquaculture Science' started out as a fisheries laboratory in the town of Lowestoft in 1902. It was established as the UK's contribution to the then newly created ICES. Being an executive agency of the UK government's Department for Environment, Food and Rural Advice, CEFAS is strongly connected to a UK national political agenda and has to provide the UK government with a range of services. CEFAS is mandated by the central and local government to licence deposits at sea, undertake visits and recommend enforcement actions. Their history is an extremely proud one. It was at the Lowestoft laboratory that Raymond Beverton and Sidney Holt developed the basic concepts and equations on which modern fisheries management is based.

From a research and funding perspective, CEFAS's profile is national, European and global. Most of CEFAS's current projects are ordered and funded by the European Union and have either the North Sea or Europe as their area of research. The UK government remains CEFAS's other large customer. Having international aid agencies and national government bodies from outside Europe on its list of customers, CEFAS also engages in global-scale research and consultancy.

Fisheries Research Services in Aberdeen, Scotland

In Scotland, the Fisheries Research Services (FRS) acts as an advisor for the Scottish and UK governments. The primary task of FRS is to advise these governments on marine fisheries and aquaculture and the protection of the aquatic environment and its wildlife. FRS became a government agency in 1997. It is an agency under the Scottish Government Marine Directorate and is a purely government-owned institution. Its research activities are to a large extent confined to the Scottish context and defined according to government policy interests.

FRS divides its work into research, monitoring and advice. The scientific research of FRS is conducted in order to understand the aquatic ecosystems, its human impacts and the options for sustainable fishing and aquaculture. Monitoring is related to the state of the aquatic environment and involves the maintenance of long time-series data sets, sampling and analysis. The advisory function is of central importance to FRS. Advice is produced to guide policy development and support the obligations of the government in relation to the management and conservation of fisheries and the protection of the aquatic environment and wildlife. The control of fish diseases and the protection of the consumer are FRS areas of advice as well. FRS divides its scientific work into the different categories of aquaculture and aquatic animal health, aquatic environment, fisheries management, freshwater laboratory and marine ecosystems. In relation to fisheries management FRS primarily works with analysis and advice on stock assessment and conservation measures.

FRS engages in two kinds of direct contact with the industry. The first is primarily of a one-way character as FRS holds briefing sessions for the industry where the industry is advised about science developments. The second kind of contact can be characterised more as a partnership. Since 2006, the Scottish government has earmarked funding for joint industry/science research projects, and FRS plays a central role in the administration and development of these projects. It participated in several research projects which were defined as being of interest to industry. These projects related to the effect of management measures and concerned issues such as the effectiveness of survey trawls, area closures, technical measures and the identification of spawning areas. Fishers participated either individually or through the fishermens' different producers' organisations. The government-funded industry/science project is to continue in 2008-2009, and the aim is to generate research ideas of common interest to industry and the government, to speed up or broaden current research work and to improve the relations between science and industry.

The Wageningen Institute for Marine Resources and Ecosystem Studies in Ijmuiden, the Netherlands

Wageningen Institute for Marine Resources and Ecosystem Studies (IMARES) in the Netherlands offers strategic and applied marine research. IMARES addresses the sustainable protection of the open sea and coastal area. Fisheries is but one area within the wider field of marine and ecological research. IMARES is the result of a recent merger. RIVO (the Netherlands Institute for Fisheries Research), the forerunner of IMARES, belonged to the Department of Ecological Risk within the Netherlands Organisation for Applied Scientific Research, a Dutch organisation that promotes applied knowledge. In 2006 IMARES was established as a joint activity of the NTO and Wageningen University.

IMARES's main work is related to the waters of the Netherlands, meaning the North Sea together with Dutch waters and river deltas. IMARES's history and funding situation does not, however, link IMARES as strongly to the national government as most of the other fisheries laboratories in northwest Europe. Until 2006, IMARES was an independent organisation under a public law that promoted the application of knowledge by businesses and the government. Today, after the merger with Wageningen University, IMARES still emphasises its function as a 'knowledge partner' for government authorities, business and social organisations who work with marine living resources.

The research fields of IMARES are ecology, environment, aquaculture and marine fisheries. In regard to fisheries, the organisation's overall emphasis is on sustainability in both economic and ecological terms. IMARES offers its expertise in the various areas of inland, coastal, shrimp, flatfish and pelagic fisheries and the effects of fishing.

DTU Aqua in Charlottenlund, Denmark

DTU Aqua is a Danish fisheries research centre. It was formerly one of a number of sector research centres in Denmark, with this one being under the Danish Ministry of Food, Agriculture and Fisheries. DTU Aqua can be traced back to early forerunners who performed biological investigations in the flora and fauna of the Danish seabed in 1889. In 2006 the organisational make-up of the institute changed as it merged with the Danish Technical University and took on the status of an university institute. This is similar to what happened with IMARES, but it took place as part of a general merger between the Danish sector research institutes and Danish universities that resulted from a high-level government initiative. At that time, the institute also changed its name to DTU Aqua. As the institute became part of the Danish Technical University, it started to expand its postgraduate educational programme. DTU Aqua depends on state funding to the extent that the government provided 45% of its 2007 budget.

Unlike other research centres, to which fishery science is but one area within a broader range of marine sciences, this centre defines all its research in terms of fisheries-related issues. Having fisheries-related issues as its dedicated area, however, does not seem to limit its variety of research interests.

DTU Aqua represents its field of research as embracing all the different aspects relating to the exploration of the sea and the freshwater together with their living resources. It is occupied with all the links 'from water to table' as it provides research on aquatic ecosystems as well as fishery technology, fishery management and the production of fish consumer products. The Institute's research projects often concern themselves with the fishery-related issues connected to the waters bordering Denmark. The institute also participates in European research projects.

The institute advises the Danish Ministry of Food, Agriculture and Fisheries as well as public authorities, international commissions and the industry. The institute has started to work in closer and closer cooperation with the fishing industry in pursuit of more nuanced data and the advancement of scientific methodology. As the institute works to assess the fish stocks and their developments, knowledge about the stocks and the fishermen's planning is considered necessary.

This concludes the short description of a few selected NFIs. These institutes are the heart of the system for assembling fisheries science advice. They operate at times under considerable strain, especially with increasing special requests for scientific advice. A scientist at a NFI related in an interview that in a period of two weeks his lab received 78 written questions from their ministry originating from members of the national legislature. This was an unanticipated task which alone took up four person-weeks and for which there was no particular budget. As discussed in the following sections, ICES and DG MARE also feel the strain of an increased number of special requests.

The NFIs are the main employers of fisheries scientists engaged in advice production, and the heads of these institutes make up the most engaged portion of the ICES delegates. The NFIs' budgets are a critical source of support for ICES in that they pay for the salaries and participation costs of most of the experts in most of the ICES expert groups. Some of DG MARE's frustration in trying to get the fisheries science advice it needs, both through ICES and more directly though STECF, is related to the fact that so much control in the system is located in the member state ministries and particularly in the NFIs.

Scientists often view themselves as doing 'volunteer' work when they staff ICES expert groups. Indeed, a scientist in a leadership position at ICES described this 'we are volunteers' interpretation of their status as quite widespread among ICES scientists. The degree of commitment the NFIs actually have for the ICES system is perceived in various ways. One scientist interviewed during a meeting observation reported that the expert-group work is seen by his institute as being 'in brackets', by which he meant that he is supposed to do it without reducing his other work. His institute director sees this work as a side issue, and the relevant ministry does not provide sufficient funds to enable participation in the group. A scientist from another country at the same meeting had a very different experience. His institute saw expert-group participation as part of their central mission.

In our survey of fisheries scientists, respondents were asked to rate on a scale from one to seven the degree to which their employer encouraged their participation in expert groups. The average among 169 employees of national fisheries institutes who had attended expert groups of all types in the past 5 years was 5.8 out of 7. So the experience of the second scientist in the previous paragraph is closer to the average. For all scientists who attended expert groups, those whose most recent expert group had been directly involved in producing stock assessments for management advice (including STECF) rated this encouragement higher. For NFI employees, however, their institutes' support for expert-group participation was not significantly influenced by the type of expert group (Table 4.1).

Table 4.1

Your employer considers working/study groups to be: 1 = totally immaterial, 7 = absolutely central to your job by type of expert group			
All scientists who attended expert groups			
Type of last expert group attended	Mean	N	p
Not directly related to stock assessment	5.06	160	
Directly related to stock assessment	5.72	123	0
Total	5.35	283	
NFI employees who attended expert groups			
Not directly related to stock assessment	5.65	81	
Directly related to stock assessment	5.92	88	0.11
Total	5.79	169	

Excluded from N of 283: 148 respondents who indicated not having participated in an expert group in the last five years, 20 who did indicate participating, but who did not make clear which kind of expert group, and six who failed to answer whether they had been in an expert group or not. Furthermore, eight did not answer the question on support from employer. In the second analysis, 114 non-NFI employees are excluded, giving an N of 169; of the 114 excluded, 12 were excluded because they did not identify their employer.

4.1.4 The International Council for the Exploration of the Sea

ICES is one of the oldest intergovernmental scientific organisations. It has a storied and prestigious past and there exists a very comprehensive recent history of the organisation (Rozwadowski 2002). From a STS perspective ICES is something of a history-maker as well. The production of science for policy support was an explicit part of ICES's formation in the late 19th century when scientific organisations tended to be disciplinary and focused on pure science. In his ICES centenary lecture in 1999, Griffith (2003) offers the following quote from the Sixth International Geographical Congress in 1895:

> Q 4.4 A resolution was passed that the Congress 'recognises the scientific and economic importance of the results of recent research in the Baltic, the North Sea and the North Atlantic especially with regard to fishing interests and records its opinion that the survey of the areas should be continued and

> extended by the co-operation of the different nationalities concerned on the lines of the Scheme presented to the Congress by Prof. Pettersson'.

This makes ICES one of the earliest precursors of Mode Two science.

As detailed in Chapter 7 (see especially Figure 7.1), Griffith (2003) goes on to outline how the structure of ICES is constantly in flux, seen from the perspective of a century of history. Figure 4.2 shows the current structure, which dates from 1 January 2008. The discussions that led to the adoption of this structure are the subject of Chapter 7. In fact, as I write, ICES is in the midst of similar discussions about the reorganisation of the Science Programme to be decided upon in late 2008.

I described the Council and Bureau at the head of the chapter. At the ICES Secretariat in Copenhagen sit the 48 people who actually draw a salary from ICES. They are directed by the Secretary-General who reports to the Council and Bureau and has the direct formal responsibility for certifying on behalf of the Council that a piece of science or scientific advice is the official advice of ICES. The Secretariat has extensive and growing responsibilities for data management through the ICES Data Centre. In addition, the Secretariat has managerial responsibility for the two main divisions of ICES: the Advisory Programme and the Science Programme. Within the Secretariat, most employees are assigned to the Data Centre or one of these two programmes. When referring to these two activities with respect to ICES as a network, I and many of the scientists I quote usually refer to the 'science side' and the 'advice side' of ICES rather than to the two programmes.

Both the formal and informal aspects are important. Within the network, as described in some detail in the next chapter, whether an ICES scientist works mainly on the advisory or the science side to some degree reflects their status and has implications for their activities and carriers. The formal programmes have their respective staff in the Secretariat, and are governed by different committees (Figure 4.2). The Advisory Committee (ACOM) governs the Advisory Programme with its counterpart being the Consultative Committee[8] (ConC). All of ICES's expert groups report, directly or indirectly, to one of these two governing committees. Under ConC, the Science Programme has eight committees under which are found two forms of expert groups. 'Working groups' designates a relatively permanent group, while 'study groups' meet a specific number of times. Under ACOM are found those working groups and study groups that contribute directly to the advice, as well as types of expert groups not found in the Science Programme. These last are review groups that, as one might expect, review the scientific production of the expert groups in the Advisory Programme, and advise drafting groups that take the outcomes of the review groups and draft the advice for consideration by ACOM. The Advisory Programme also includes Benchmark Workshops that periodically take an in-depth look at the methods being used for analysis and advice for a particular group of species.

National representation is much more important in the Advisory Programme. ACOM is made up of two representatives of each of the member countries. Expert groups that report to the ACOM operate under a different set of rules than do expert groups reporting to ConC. Advisory group membership is determined by the delegates, while the chairs of expert groups in the Science Programme can allow membership on their own initiative. Several related issues are involved here. One is simply funding for participation as mentioned in the last section. The second is that participation in advisory expert groups requires the permission of the national delegates of the participant's country. An advisory expert-group chair can nominate but cannot officially permit nor fund someone's participation. While this is the official procedure, it is not always followed in practice because expert group chairs are concerned primarily with addressing their group's Terms of Reference (ToRs) and are interested in identifying and recruiting this expertise. They may not always find time for following official channels, especially as there is no particular system in place – beyond the attention of the chair – for making sure that no one is present who is not supposed to be.

ICES also has a concern that limiting participation will lead them to be accused of excluding people for political rather than scientific reasons. Of particular concern, again, are potential sensitivities with respect with national representation, but there are also concerns with the possible attendance of scientists whose views are not entirely orthodox. This second concern comes up in particular on the science side where the chairs can allow participation at their own discretion, but where results will still be understood as coming from ICES. Scientists feel that there are roles to be played by both delegates and chairs in the control of expert-group participation, but agreement on these matters is not easy:

> Q 4.5 Scientist One: The scenario **Scientist Two is pointing to [the accusation of political exclusion] is foreseeable**. For working groups that are not in the advisory process, then the Delegates should also have a hand in it. Scientist Three: It has to be at the invitation of a chair, and chairs have to be able to say no. Scientist Two: I want no part of that. Scientist One: Why? Scientist Two: **I am thinking of the boundary between advice and science, which is the path we are on if we go with ecosystem and integrated advice. There are areas with a wide range of scientific opinion … Experts with quite good credentials could see a terms of reference and consider it a refusal to invite them**. Scientist One: We get that anyway. Scientist Four: We should make honest a practice that goes on. We know that individuals find their way into working groups. At this stage let's just make the system honest. **The simplest thing is to just acknowledge that people can self-nominate with the agreement of the chair**, adding a clause that they should be able to contribute. *(Observer's notes at the Consultative Committee meeting, September 2004)*[9]

Thus, a practical solution is reached that will likely last until someone decides to make an issue out of someone else's participation in an expert group. It is interesting to note here the reference to the EAFM. This is the first mention of the related issues of the science/advice boundaries and the mobilisation of the many different kinds of expertise that the EAFM will require. These issues will be dealt with much more in Chapter 6.

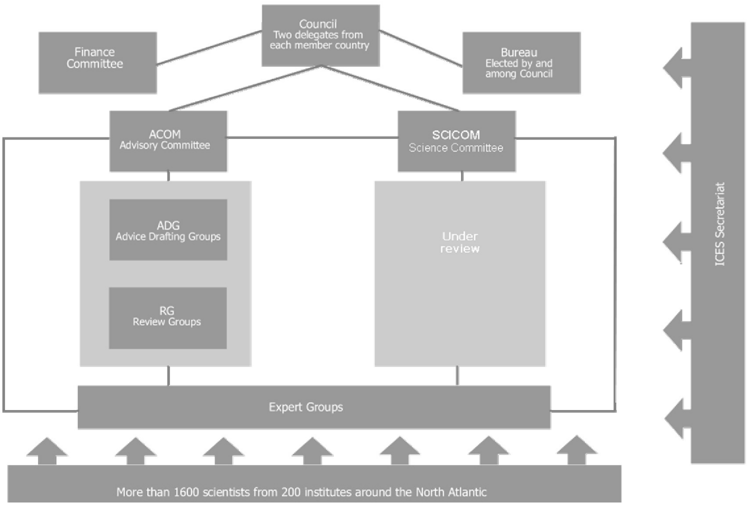

Figure 4.2 The current structure of ICES (www.ices.dk)

Figure 4.3 outlines the ICES structure before the changes that began in January 2008. The major difference is the creation of ACOM in place of what had been the Management Committee for the Advisory Programme (MCAP) and the three previous advisory committees: the Advisory Committee for Fisheries Management (ACFM), the Advisory Committee for Ecosystems (ACE) and the Advisory Committee for the Marine Environment (ACME).

According to its current Memorandum of Understanding with DG MARE, which is available on www.ices.dk, ICES provides recurring advice for 38 species or species groups. Recurring usually means annual, but particularly poor data or special characteristics in the biology of the animal may require a different time interval. The '38 species' also implies a much higher number of fish stocks, and the annual fisheries advice is of the order of 1600 pages long. In addition, ICES provides non-recurring advice through special requests that are negotiated on a case by case basis.

Table 4.2 is provided to give some examples of the kinds of advice requested. Some of these requests may be so-called 'fast track' requests (examples in Table 4.3) that ICES must organise and respond to under exceptional time pressure.

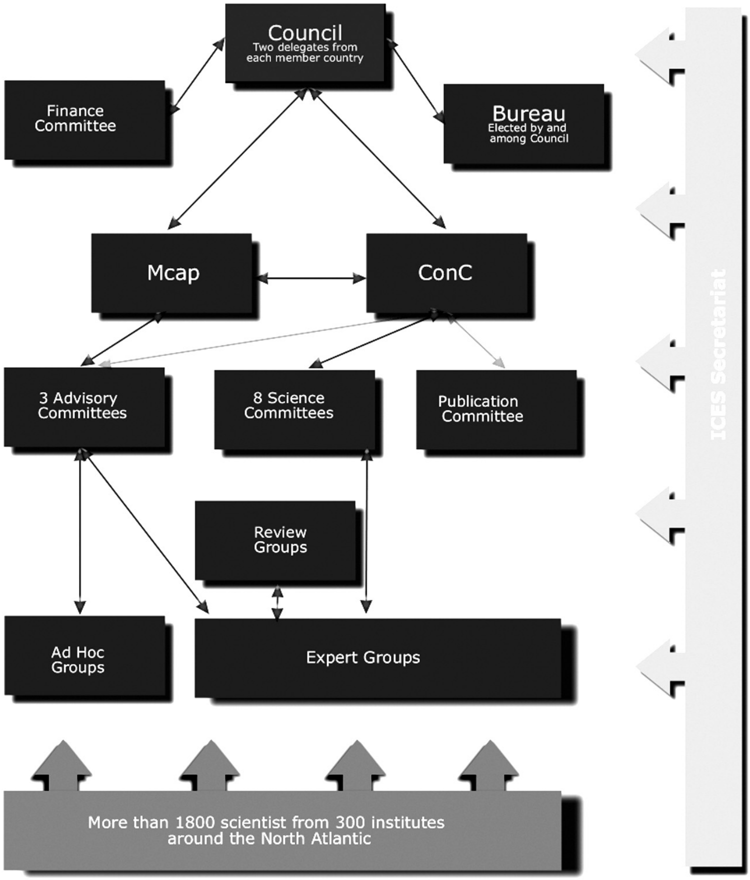

Figure 4.3 The structure of ICES before the reorganisation (ICES 2007c)

Nevertheless from a DG MARE perspective, in spite of the fast track system, the advisory system is not designed to respond swiftly enough to urgent requests (CEC 2003a). DG MARE is considering making greater use of short-term contracts and more stringent prioritisation procedures in order to address issues which arise in the EU political arena or in international negotiations, for example, that need quick scientific assessments. Because ICES is geared towards addressing the ongoing annual rhythm of stock assessments and generating advice for TACs, it is difficult for them to organise resources to address such requests from DG MARE.

Table 4.2 ICES special requests pending in May 2008

Client	Topic	Date received	ICES response	ICES groups	Client deadline
EC-DG MARE First 3 as examples of a total of 10 special requests	Long-term management of the NEA mackerel stock and fishery	25/01/2007	Progress report ACFM Oct. 2007 Final response 25 April 2008	Ad hoc group	As ICES response
	Management plans for NS saithe, NS herring and herring IIIa	27/03/2007	Saithe 25 April 2008 Herring stocks 27 June 2008	WGHMP, WGNSSK Ad hoc group	As ICES response
	Revision of Salmon Action Plan – Baltic	09/08/2007	26/06/2008	Ad hoc group and WGBAST (13-16 May)	As ICES response
NASCO	Advice for 2008	12/07/2007	09/05/2008	WGNAS	15/05/2008
NEAFC First 3 of 10	Blue ling spawning aggregations	30/11/2007	23/05/2008	WGDEEP	As ICES response
	Stock structure of *Sebastes mentella*	30/11/2007	*Early 2009*	SGRS + attached workshop	Not settled yet
	Advice along the lines of that given recently regarding deep-sea species on the appropriateness of the introduction of potential management units for redfish in the Irminger Sea and adjacent waters	30/11/2007	23/05/2008	WGDEEP	As ICES response
Norway	IUCN criteria for red-listing marine fish species	20/09/2007	*To be decided*	*To be decided*	*Not settled yet*
Norway	Management goals for the harp seal stock in the northeast Atlantic	27/02/2008	*To be decided*	*To be decided*	;*Not settled yet*

Norway, EC and the Faroes	Mackerel, unreported landings	25/02/2008	*To be decided*	To be decided	Not settled yet
OSPAR First 3 of 14	An assessment of the changes in the distribution and abundance of marine species in the OSPAR maritime area in relation to changes in hydrodynamics and sea temperature	01/07/2006	ACE June 2007 9 June 2008: final response	Many SGs and WGECO involved	09/06/2008
	Peer review of further nominations for threatened and/or declining species and habitats	01/07/2007	01/02/2008	Ad hoc group	01/02/2008
	Development of proposals for Environmental Assessment Criteria	01/07/2007	23/05/2008	WGBEC	02/06/2008

Thanks to ICES for providing this information.

Table 4.3 Fast track requests to ICES in 2005

Client	Fast track request
DG MARE	1. Compile status list of EU fish stocks 2. Sole in IIIa B new information to be included in re-assessment of stock 3. Bycatch of common dolphin 4. Advice on deep-sea stocks 5. Long-term management of Baltic cod 6. Request on restocking of glass eel 7. DNA analysis of Baltic salmon
DG MARE and Norway	1. Long-term management advice
DG ENV	1. Influence of sonar on marine mammals and fish
NEAFC I	1. Information on stock identity of *Sebastes mentella* and quantitative information to allow spatial and temporal limitations in catches 2. Advice regarding the proposal for the protection of vulnerable deep-water habitats 3. Stock assessment methods for Atlanto-Scandian herring and blue whiting stocks 4. NEA mackerel stock assessment methodology
BSFC	1. Advise on areas with the Gotland Deep and Gdansk Deep where the hydrological condition allow for a successful cod spawning in 2005
OSPAR	1. The design of one-off surveys to provide new information for a number of OSPAR Chemicals for Priority Action 2. Quality Assurance of Biological Measurements in the northeast Atlantic
HELCOM	1. To coordinate quality assurance activities on biological and chemical measurements in the Baltic marine area and report routinely on planned and ongoing ICES inter-comparison exercises, and to provide a full report on the results
Norway	1. Catch of NEA cod and haddock for 2006

Thanks to ICES for providing this information.

4.1.5 Scientific, Technical and Economic Committee for Fisheries

The STECF has been producing reports since at least 1995, but the current STECF was legally created by the same European Council legislation that reauthorised the CFP in 2003. Article 33 of that document reads as follows:

Q 4.6 – 1. A Scientific, Technical and Economic Committee for Fisheries (STECF) shall be established. The STECF shall be consulted at regular intervals on matters pertaining to the conservation and management of living aquatic resources, including biological, economic, environmental, social and technical considerations.
2. DG MARE shall take into account the advice from the STECF when presenting proposals on fisheries management under this regulation.

Members of STECF are appointed for three-year terms by DG MARE. There are currently 32 members and 39 reserve members. The STECF holds a plenary meeting twice a year. The fisheries advice is reviewed, and an Annual Economic Report is produced. Part of this is an estimate of the economic impact of the current ACFM advice. When scientists attend STECF meetings, DG MARE pays for travel expenses, but they remain employed by their home organisations even though they are working on assignments put to them directly by DG MARE. Member states can be more willing to send people to STECF than ICES because they see it as closer to the decision, and work at ICES is paid for by the NFI.

As mentioned above, STECF and ICES expert groups draw on the same pool of people. In recent years STECF has begun to have similar recruitment problems to those found in ICES. The Joint Research Centre, a Commission entity which is acting as the STECF Secretariat, is having increasing problems recruiting scientists for expert groups. According to a respondent at DG MARE, the problem is much more finding people than it is having the money to put them to work.

Some in DG MARE do not find this continued dependence on the employees of other institutions adequate because it prevents the expansion of STECF tasks beyond what the goodwill of the NFIs and other employers allows. The need for a true in-house science capacity at DG MARE is an ongoing debate. Some scientists working at DG MARE strongly believe that DG MARE needs its own in-house fisheries science capacity. While the ICES system with STECF review may provide independence, and hence increased legitimacy, it is inefficient. A Commission employee describes their position this way:

Q 4.7 This is quite a strong opinion, actually, from those who are writing these regulation proposals. They have good reasons to have that opinion; I understand perfectly where it is coming from. All these concerns about whether it is legitimate outside the house and whether it is scientifically credible in terms of being properly peer-reviewed and based on models that have been scrutinised becomes less of a concern. It is not really that the in-house advice may have less standing in civil society generally, in the public debate that is not an issue. If you are under pressure and you have to say so many days for a hundred different fleets, you have to come up with a number for the next regulation, you just need that number to come from somewhere, and as long as it is on the best possible technical basis you

> could just consider it to be engineering rather than science, and it may be perfectly valid without having all these features that you would need to have legitimate and credible science.

The relationship between ICES and STECF can be tense. One issue is that, in spite of the fact that they are competing for the same people, it has been difficult to avoid overlap and duplication. Another is feedback when changes in the advice are recommended. A respondent who works in the Advisory Programme at ICES:

> Q 4.8 Interviewer: Do you find DG MARE helpful partners in terms of eliminating ICES STECF overlap? Advisory Programme Leader: Yes and no. Yes, when we have face-to-face talks and we resolve issues, but not in terms of planning. I find it very difficult to understand the logic of DG MARE about when they use groups in ICES and when they set up different groups in STECF. And in general there is very little communication from STECF to ICES. So if there's a group evaluating North Sea flatfish management plan, we don't see it, and there's no formal exchange of reports.

One of the most fascinating things in this entire system is how essentially the same fisheries scientists seem to adapt to quite a different scientific culture when working in STECF or working in ICES. There is a story that has been repeated many times now about STECF and ACFM. It is outlined in detail in Section 6.1 because it illuminates a great deal about the workings of the science boundary in the advisory system. The reason for mentioning it here is that the way it is so often repeated, and even has become part of the cultural mythology of the advisory community, illuminates the tension that exists between STECF and ICES, even though they are usually the same people. The story is that the ACFM had refused to do mixed fishery assessments because of a lack of data on discards that they felt made the analysis impossible. After this happened, DG MARE brought several of the same scientists who had been at that ACFM meeting to a meeting of the STECF and asked them to do the analysis that ACFM had refused to do. Those scientists did provide the requested results to DG MARE. When the story was retold at ICES, it was also mentioned that some DG MARE employees had said that this outcome demonstrated that ICES should have been able to do the analysis in the first place. The ACFM members who related this anecdote were quite offended by this. One scientist got a laugh at an ACFM meeting by joking that 'DG MARE is better than ICES because they are able to do more work with less data'.

The following is from an interview with a scientist active in STECF. He describes the relationship between STECF and ICES advice in terms of the actual practice of how they receive the ICES advice and then pass it on to DG MARE. He also mentions this same story about the mixed fishery assessments:

Q 4.9 Interviewer: So you think, that's how STECF is working, as a sort of an extra quality control? Respondent: Well, not so much quality control. It just adds, you know, if you look at our annual stock review that we do in STECF, in 99%, or at least 95%, we just say 'No comment', or 'We agree this, that or the other, but we note that'. And the only reason that there is an STECF comment is that somebody has thought something was questionable with the ACFM report and that always comes back to the member state, to the scientists on the ACFM from the member state that put in the request. I would suggest. I mean we don't sit around and read the whole of the ACFM report in that STECF subgroup and start asking questions. We say 'All right, do we have any questions about this advice? And if we have, let's look at it. Interviewer: But that's only one part of the STECF role, and the other, it has a wider mandate than that? Respondent: Yeah, but it's actually an important part because it's written in the STECF regulations that you have to provide an annual review of stocks. Um, it has a wider role, and it's a consultation role, it's a consultation committee for DG MARE. So DG MARE can ask it any issue that it wants, really, and sometimes there's actually a bit of naughtiness on DG MARE's part, if you like. They try to ask questions that support the answer they first thought of, and if that answer isn't supported then they often come down and say, well, why can't you do this? You know, a couple of years ago there was an example ... all the heavyweights came into the room and said, 'look we need a table in your report of what comes out of this mixed fishery forecast', and of course we all smelled a rat. And reluctantly we agreed that we would put a table in as an example, and of course as soon as it was in the report, it was used as the proposal to start negotiations. So that was a bit err, we weren't very happy about that.

The importance of these references to the table being 'an example' that they were not 'happy' about should not be exaggerated. STECF made some very specific distinctions about the questions they were and were not willing to answer (Section 6.1). Previously in the interview, he also said, however, that sometimes ICES is simply not pleased to have another committee second-guess its advice. He believes that STECF should be seen as another opportunity for DG MARE to get needed work done, even if this closer association sometimes provides 'scope for people to be rather naughty' as he later puts it. Members of ACFM have been known to go back to the members of their home institutions and report that they do not agree with the result in the ACFM report, and that they intend to try to get the advice changed at STECF. Another stock assessment scientist told us that DG MARE organises STECF and related meetings on short notice 'because they need something that is called 'science' to be used as the basis of a decision'. He sees the less structured STECF as almost a way 'DG MARE exploits scientists'. These are classic tensions between gaining some legitimacy while losing some saliency through increasing the distance between managers and scientists.

The importance of the formal distinction between in-house and independent science is debated. In the private sector in-house science has actually declined because of costs and because high uncertainty means that firms are not sure what knowledge they need (Gibbons et al. 1994). However, some have suggested that in policy matters, this is a very important variable. Clark et al. (2002) report that how much a scientific assessment is carried out by or under the control of the subsequent users is one of the top three features in determining the future use of that scientific assessment. This is a function of increased saliency. Alcock (2004) makes a similar claim in a study of the organisation of fisheries management, while simultaneously pointing out that fisheries science under control of the management agencies also raises suspicion among stakeholders. His analysis, however, conflates in-house (in his terms 'embedded') science with top-down management. He characterises the pre-cod collapse Canadian system as embedded, while seeing the US system as disembedded because of the existence of the Regional Management Councils. This characterisation underestimates both the relative degree to which scientists have influence over NOAA Fisheries and the degree to which NOAA Fisheries has influence over the Regional Management Councils. His suggestion that the more independent science has a higher legitimacy among stakeholders would also fail in a comparison between North America and Europe, where ICES is formally entirely independent of the Commission and where the legitimacy crisis in fisheries is particularly focussed on science (Schwach et al. 2007). The bottom line seems to be that what is really important with respect to saliency is the ease of communication between policymakers and scientists and, with respect to legitimacy, the perceptions of independence, rather than in either case the legal relationship between the two groups.

4.1.6 DG MARE

DG MARE uses two main types of biological advice from ICES. These are the annual advice on TACs and the more specific special requests described above. Many of the special requests derive from derogation requests, meaning requests by member states for the special application of regulations, but many also derive from ideas about technical measures for specific fisheries. DG MARE will also sometimes request ICES advice with respect to strategic fisheries management directions.

Needs for advice are identified in DG MARE, usually from someone charged with writing the regulations or with handling the annual negotiations both among member states and between the EU and its fishing neighbours. The annual advice is delineated in the Memorandum of Understanding between the DG MARE and ICES. Until recently, ICES produced advice in June and October on management of a range of different stocks. This timeline is now being moved up in a process called 'front-loading' in order to accommodate more input from stakeholders.

When the advice arrives from ICES, DG MARE immediately asks STECF for an opinion, and they create a study group of approximately ten people and write a report commenting on the advice. A respondent at DG MARE described this process as 'quite extensive, they do a thorough job, not a rubber stamp, but basically they do end up agreeing. They comment on all issues relevant to DG MARE'. Simultaneously, DG MARE is already beginning to draft measures because of time pressure. If STECF does recommend something different than what ICES did, they then make the required modifications. The final proposal is sent to the Council of Ministers. Once this proposal is made, the negotiations start.

The derogation requests can become quite extensive and are usually dealt with by STECF. DG MARE is, in fact, required to respond to any citizen, so of course any communication from a member state ministry asking for a derogation must be responded to. A Commission respondent said that at any such request, 'the machinery starts and a group in STECF is formed'. However, this is not quite as *ad hoc* as it sounds. The requests from each member state are bundled, and most come in the beginning of the year right after the Council of Ministers has made its final decisions. When this system was set up in 2002, the member states sent many requests that got no response from STECF beyond 'we don't have the data needed to evaluate this'. Without an analysis a derogation will not be granted, so more and more often the member states now provide some sort of backing for the request including a scientific rationale, which gives STECF something to review. The final decision is made by the Council of Ministers, and the decision about what will be sent on to them remains with the full-time DG MARE staff.

The final political decision-making process for the CFP sits in this relationship between the Council of Ministers and DG MARE. The Council of Ministers reflects the desires of the member states, while DG MARE represents the European perspective. The Council makes its decisions by 'majority vote', meaning majority in the EU's weighting system in which larger countries have more votes. The Council has the final decision, but DG MARE is not at all powerless because only DG MARE has the power to propose. The Council can only approve their proposal. So when DG MARE makes a proposal to the Council, its role has not ended. Once the proposal is made, a negotiation starts in the form of 'if you propose this, we will agree to it'.

With uncertain advice, everything becomes even more negotiable, and this creates a dilemma. The scientists at DG MARE say that they struggle with the extent to which they should try to foresee the reaction of the Council and be more precautionary. In the end, a respondent said, they usually try to give the advice based on what is seen as the most appropriate reaction to the kind of uncertainty involved. The Council generally does not consider itself bound by the precautionary approach in the sense that having less information means less fishing. One attempt by DG MARE to introduce this directly was rejected (a rule that when data were poor, the TAC would be reduced by 25%). The proposal did, however, convince certain fisheries where there had been some obstruction in data-gathering to start being more forthcoming. A more common reaction of the Council to uncertainty is to just carry over the existing TAC from the year before.

Once the negotiation process starts, the role of scientific advice is formally ended. While DG MARE is required to consider the best available science when making its proposal, the Council is not required to do so. The same is true of the impact assessments that are required for Commission proposals but not for Council decisions. As a Commission respondent phrased it:

> Q 4.10 The requirement is that the proposals from DG MARE must be based on this [best available science], but it is interesting that when the Council decides something else without having any scientific opinion on the impacts, that is not considered an issue. That is a little bit of a paradox … the initial Commission proposal must be backed by science and all this, but it is true that DG MARE is responsible because it says we are not going to do this, and DG MARE agrees to a compromise, then the Council takes it. This compromise does not have to be validated according to any principle of governance, impact assessment, scientific justification or stakeholder consultations. The principles apply only to the first proposal.

Another possibility would be that the Council has the power to simply allow DG MARE to manage a species as they see fit. DG MARE is trying to move in this direction, as no one is satisfied with the current system in which, at the end of the day, the cabinet ministers of world powers are sitting in a room haggling over numbers of monk fish. They hope to move towards a system where the Council focuses on principles, not on tonnes of fish. The main strategy for this is developing long-term management strategies based on harvest control rules.

4.1.7 Priorities for reform

From the point of view of the managers in DG MARE, there is currently a gap between the form of advice that the scientific system is geared to deliver and the form of advice that is increasingly needed. Under the current reform process launched in 2002, the Council of Ministers identified a number of priorities with direct implications for how scientific advice should be provided. The major priority was moving from short-term year-by-year management to a long-term framework based on harvest control rules (HCRs). HCRs are rules for imposing catch and effort limits that are developed before the need for them arises, and so they are, in principle, able to avoid arguments over the immediate imposition of limits on fishing. This is the critical reform needed to move the system away from the bottleneck around the decisions in the Council of Ministers. Another important driver is the desire of the fishing industry for a stable business based on predictable catches. Accompanying this shift to long-term management is a movement towards management based on fleets and fisheries rather than fish stocks, because these are the units that are actually regulated. DG MARE needs scientific advice that they can apply to the management of fisheries, i.e. fisheries-based, multi-species advice. These changes have important implications for how scientific advice is to be formulated and communicated.

Table 4.4 Prioritisation of problems with respect to the process of producing scientific advice for management

Last expert group attended Problem	No expert group	Non-assessment expert group	Assessment expert group	Total	
				Mean	P
Difficulties with producing mixed-fisheries advice	3.9	3.91	4	3.94	0.77
Difficulties with producing fishery-based advice	2.8	3.18	3.33	3.11	0
Difficulties tailoring advice to harvest rules	2.66	2.6	2.26	2.51	0.1
Inconsistencies between different stocks in how advice is determined	2.85	2.54	2.82	2.72	0.11
The length of time between data gathering and the generation of advice based on that data	2.79	2.77	2.59	2.72	0.58

Respondents were asked to prioritise these problems with respect to one another, resulting in scores from 1 (lowest priority) to 5 (highest priority). N = 341. Excluded: 20 who did indicate participating in an expert group but did not make clear which kind of group and therefore did not fit in any of the three categories; six who failed to answer whether they had been in an expert group or not; 69 who chose not to answer the question; and 29 who failed to rank according to instructions.

Table 4.4 indicates the priorities that the fisheries scientists give these various reform items. The only point of significant disagreement is that advisory scientists give a higher priority to fisheries-based advice than their colleagues do. It is striking, however, that the scientists are very much in agreement that the time lag between data-gathering and basing advice on that data has a very low priority. This is likely the problem that fishers would cite before the rest. The other low priority, the question of inconsistencies between stocks with respect to advice, is something that is very much of concern to clients and the leaders of the ICES Advisory Programme. This issue is discussed extensively in Section 7.3.1.

Another priority for reform among some, but not all, clients is formal transparency. Transparency is called for directly in ICES's MoU with DG MARE, and it was a topic that got some attention in the recent restructuring debate as discussed in Chapter 7. ICES is opening up meetings to observers much more than in the past. In 2004 they began to invite representatives from both industry and conservation NGOs to sit in on the meetings of ACFM. This was in response to the desire of both DG MARE and the stakeholder groups who have been working in various ways for such access.

I did short interviews with all of the observers who attended the October 2004 and the June 2005 meetings and solicited email reflections from the ACFM membership about their reactions to being observed. Of the 12 scientists who offered such input, seven wrote that they rarely or never found themselves aware of the observers' presence during the meeting; four reported that they were aware some of the time; one reported that he was aware of them most of the time. This last scientist was playing a facilitation role in the meeting that required such awareness. Nine of the 12 scientists said that they did not believe the presence of observers had any influence on their statements or behaviour. One scientist said that he believed that it made him more circumspect in the way he discussed the issue of misreporting and that he may have 'pulled his punches' a little in these statements. Another scientist said that he believed that the observers influenced behaviour in a 'not necessarily negative' way in that they provided an incentive to behave more 'properly'. This was in reference to the fact that scientists commonly read email or news on their computers when the meeting is discussing stocks that do not interest them. The observers made them a bit more self-conscious about this. A third scientist said that he was aware of trying to articulate things in a less technical fashion, but suggested that this might also have positive aspects.

The observer programme, however, hardly introduced concerns about outside inspection. In working sessions scientists are very conscious that their work will undergo scrutiny, and there is an increased appreciation that this examination will go beyond the ACFM and even beyond the client groups such as DG MARE. This awareness is reinforced by these clients, as in the following short exchange:

> Q 4.11 Scientist One: [representing a client] Don't write anything, leave it, too complicated, just say they have been updated ... Scientist Two: a couple of well crafted sentences about changing age ranges and rescaling the reference points to make it clear what we have done in the introductory pages, otherwise I agree with Scientist One. *(Observer's notes at the Advisory Committee for Fisheries Management meeting, October 2003)*

One impact of this expectation of scrutiny is that the ability to clearly explain becomes a criterion for the validity of results. This is evident in Q 6.1 where the scientists agree that if they cannot explain the model under discussion, they should not put it forward. What this actually means is a bit controversial as can be seen from the quote from Scientist Three. On the one hand, he recognises that laypeople are going to read the report, but at the same time he feels that they should be writing their report for other experts, i.e. ACFM, and not be constrained to write with laypeople, even managers, in mind. Nevertheless, towards the end of the meeting as the report is being finalised, the following exchange took place that showed considerable awareness of the possibility of a politically oriented lay audience.

> Q 4.12 Scientist One likes the run number to come after the description of what it is doing, and Scientist Two agrees. They say **this is easier to understand because you start your thinking with the run description. Scientist Three does not want on line 16, page 5 to have someone with a 40% cut described as suffering little, suffering less would be better**. Scientist Two: I have difficulty interpreting the conclusion because of not understanding q. Scientist Three agrees. **Scientist Two: If you speak to a manager how would you explain q and p?** Scientist Four: I have some text on that. Scientist Two: Lift it in. *(Observer's notes at the Working Group on the Assessment of Demersal Stocks in the North Sea and Skagerrak meeting, September 2003)*

Transparency depends on the ability of fisheries stakeholders, including scientists, to be able to explain how they know what they say they know. A number of forces are pushing this requirement deeper into the workings of the scientific deliberations than it was before. These demands to increase transparency, while often enjoying very strong support, begin to quickly run into the paradoxes of transparency as soon as they begin to be systematically applied.

4.2 Numbers, words and people: Uncertainty and the science boundary

The limits of the advice assembly system are most clearly revealed when it encounters the scientific uncertainty that is such an important factor in marine science and the management based on it. The quota allocation problem that characterises the European fisheries management system requires an answer to one main question: 'How much fish can we take this year and still have enough left over for long-term exploitation?' The most useful answers to this question are generated by quantitative forecasts of the future state of fish stocks under various conditions, most importantly levels of exploitation. These forecasts are based in turn on stock assessments that characterise the present state of the stock based mainly on data gathered by survey vessels. Fisheries-dependent data gathered in fish landing ports is also used, to a greater or lesser extent depending on the species, to increase the accuracy of the assessments. Previous assessments also play an important role in current assessments. The length of the time series of data available, the number of fish that die as a result of fishing as opposed to other factors, the accuracy of estimates of fish ages and how much they weigh when they are at particular ages, and the relationship between the number of fish in a stock in one year and the number of fish that will be 'recruited' to that stock in the next year are all important for most stock assessments. These factors are all more or less uncertain, again depending on the species. Uncertainty in these factors, as well as in others that may be important, has a powerful influence on scientists' confidence in their forecasts of future numbers of fish. It is difficult for scientists to finesse this uncertainty verbally. Splitting hairs is a constant temptation, but the reality of the need for the numbers means that fine distinctions have little meaning for the overall system:

> Q 4.13 Scientist One: We don't reject the assessment, but we reject it as a basis for a forecast. Scientist Two: Different words, same meanings. *(Observer's notes at the Advisory Committee for Fisheries Management meeting, October 2003)*

To be salient, the advice needs to provide a clear basis for deciding how many fish can be caught. To be legitimate, it must pass through the ICES process, but in the end it cannot be open to various interpretations by the different stakeholders. To be credible, it must account for the underlying uncertainty. These demands arise in very practical ways within the political milieu surrounding the implementation of the advice. One illustrative exchange was related to us by an ICES official about discussions with DG MARE on how the advice should be communicated. DG MARE wanted ICES to account for uncertainty when providing its advice, but not in a way that made it unclear what the advice actually consisted of. ICES agreed that the advice should be clear and said that they would continue to work on the problem 'along with DG MARE observers at the ACFM meetings'. For ICES the particular problem in deciding how to articulate official scientific advice is something that the scientists and the managers need to work out together. DG MARE sometimes sees the problem as that of ICES presenting results in a way that understates the uncertainty and allows the fishing industry to seize on a particular result that they like and ignore the uncertainties underlying the result. This is, of course, a common accusation that the fishing industry makes of the managers as well. At other times, of course, the parties would like their preferred choice to be presented with less emphasis on uncertainties.

One example emerged around the 2002 North Sea cod assessment. The assessment had indicated an increasing biomass and decreasing fishing mortality over the two most recent years. This had been taken by the industry as evidence that the regulatory measures were working and that stronger measures, which both DG MARE and ICES thought necessary, were in fact not necessary. DG MARE believed that the uncertainty underlying these results had not been effectively communicated. They wanted the advice to be placed in the form of HCRs with expression of uncertainties around the assessment numbers. They did not specify what these expressions of uncertainties should consist of, and our respondent felt that this decision was, somewhat unfairly, being left up to ICES. The scientists did not have an agreed method for calculating the precision of assessments. Even more important, the real uncertainties for the scientists arose from the fact that the population dynamics of the stock at such historically low levels were not understood. There is good reason to think that methods that work fine in assessing and making forecasts about healthy stocks are much less reliable when stocks become very small. They also noted that the recent assessments had been overly optimistic about the rebuilding of the cod stock, which they attributed to data problems arising from discarding or from the swift changes in fishing patterns that are common in mixed fisheries governed by a number of separate quotas. This is a complex mix of sources of uncertainty that required judgement calls in place of fully transparent scientific methods, and therefore required a degree of trust in the scientists' ability to make these calls in a balanced, well-informed fashion.

The underlying issue in this disagreement between the scientists and the managers is the complementary and contrasting roles of quantitative tabular data as opposed to qualitative textual information in the scientific advice for fisheries. On the one hand, we have the one main question mentioned above, 'How much fish can we take this year and still have enough left over for long-term exploitation?' DG MARE, at the end of the day, wants scientific advice that will allow them to choose a number that they can justify. On the other hand, as the concerns in the last paragraph reflect, the credibility of results requires that uncertainties be explained with phrases such as 'stock dynamics at low levels are not understood', 'data problems from discards' or 'changing fishing patterns'. Whatever DG MARE means by 'expression of uncertainties around the assessment numbers', and they likely do not have a clear, operational idea themselves, it is closer to a quantity such as a confidence interval than it is to a long digression about the real sources of uncertainty that ICES is facing.

What is being wrestled with here is the line between what Funtowicz and Ravetz (1990) would call the spread and assessment of the number and its pedigree. The fisheries professionals are comfortable thinking about spread, somewhat comfortable thinking about assessment, but they do not have the language to talk about pedigree. Dealing with uncertainty requires the development of trust because the procedures of transparency cannot be brought to bear. This means an alternative social process involving the development of trust in judgements based on both trust in the judgement of others and a degree of participation, and hence ownership, in the judgement itself. These are the requirements of an effective 'extended peer review'. This also implies that institutional forms are needed that are able to limit the size of the group where these trust-based mechanisms for addressing uncertainty have to take place – i.e. nested systems and the separation of different levels and types of transparency mechanisms. This is a central question in the case to which I return in the final chapter.

The following exchange took place at the ACFM meeting after the discussion related above between DG MARE and ICES. It is a long excerpt, but very illustrative of the problems that they are facing and of how both quantitative advice and its qualitative context come together in trying to formulate the advice that is really relevant for how they see the actual condition of the fisheries. The discussion is about how to formulate the advice for North Sea cod in a mixed-fisheries context. The text under discussion says that the cod catch should be 'as close to 0 as possible', recognising that there will be discards of cod from fishing boats targeting other species.

> Q 4.14 Scientist One: Last year we gave strong advice because the stock was in desperate and dire state, and we wanted to *prevent* its commercial extinction, this kind of wording takes us back to wording that got us criticism in the past. '**Close to 0 as possible' what is that? Now we give unequivocal advice, and we get attacked. I don't want to be unhelpful to managers but ... I am unhappy with this kind of phrasing.** We had a long discussion from last year and Scientist Two's text is a reaction to manager feedback, but my view is that the state of the stock has not changed. Do we just bend? We need to be helpful, but do we bend to every wind? Scientist Two: No and this should not be seen as a retraction from last year or the seriousness of the situation, **I agree to anything that says it is as bad as it was, but in mixed fisheries, if we say the catch should be zero, then we are saying 'Close all demersal fisheries'**. There is no way of getting around saying that mixed fisheries should prioritise clean [i.e. little bycatch of species of concern] fisheries, but there will not be 0 catches, unless you want to say close all demersal fisheries. Scientist Three: Yeah, how much do we read into changing 'reduced catch of cod' to 'no catch of cod'. What do we want to advise? Scientist One: **If we are really serious then we say this is the advice and the caveat comes with it, if there are other reasons they decide to go ahead, fine, but we should make sure they recognise what they are doing.** Scientist Two: We could have an opening statement saying the catch should be zero and all fisheries closed, then continue with this text. Scientist Four: **I agree to a large extent, but it should be made conditional on the implementation of the recovery plan that would take account of the mixed fisheries**. *(Observer's notes at the Advisory Committee for Fisheries Management meeting, October 2003)*

Not only must the advice for cod within the mixed-fisheries context be communicated using text,[10] how the advice will be phrased is directly affected by both the ongoing discussions with the managers and their speculations about how the advice will be taken up and used. Scientist Four's contribution is perhaps the most interesting as it indicates a desire to make the advice conditional on a specific set of management measures. In this particular comment, such reflexivity is perhaps especially ironic because this scientist is referring to taking 'account of the mixed fisheries' when DG MARE is asking for tools to do just that. Tools that, in a few days, the ACFM is going to decline to provide because of discomfort with the units of analysis required and the levels of uncertainty attached to them (Section 6.1). While this is a particularly dramatic example, comments indicating that scientists need feedback from managers are very common at both the ACFM (now ACOM) and assessment expert group levels. The following exchange addresses this question even more directly, also in terms of how to handle mixed-fishery advice and this time in the context of how to apply the precautionary approach to which ICES is publically committed. It took place at the committee in charge of the Advisory Programme:

> Q 4.15 Scientist One: This is how ACFM must deal with this; when they give multispecies advice, it must be consistent with the precautionary approach. I thought the interpretation is that below Blim they had to go to 0 catch ... Scientist Two: No, they have to have a recovery plan. Scientist One: But as long as there is no recovery plan in place, they have to advise 0 catch. The recovery plan is not in force now. **Scientist Three: It is not ACFM that has to recommend a certain recovery plan. Scientist One: No they have to recommend the catch level and that must in some way be consistent with the precautionary approach**. Scientist Four: No. Scientist One: I'm surprised you don't think so. Scientist Three: We can ask Scientist Five about that. **Scientist Four: You are right in that the reference points are from ACFM, they take the precautionary approach into account, but they don't check against it afterwards. Scientist One: We should check how close we are to these goals.** *(Observer's notes at the Management Committee for the Advisory Programme meeting, September 2004)*

It is in these kinds of comments that we can see how the line between the science and management has become truly reflexive. MCAP actually wants to follow up and see how the advice is actually being used in terms of ICES's precautionary goals, presumably to inform how future advice will be set. A lack of follow-up on what happens to ICES's advice is a systemic problem, as discussed in Chapter 7.

At one point in this October 2003 ACFM meeting, a document was presented suggesting guidelines on the use of language in the report text. The word sustainable and its relationship to the fisheries management targets, for example, were discussed at length. There was some fear that they should not be tied too closely as ICES may be changing the way it defines these reference points. Another concern was that after they had gone through all this trouble to define what they mean, readers may not even bother to turn to these definitions. It was suggested, albeit factiously, that instead of using a word like 'sustainable' in the text that a code such as 'Concept One' be inserted instead so that people would be required to look up exactly what they meant.

It is not just the ACFM that takes the text of the advice very seriously in terms of seeking to limit how the advice will be used. This is also true of scientists in the expert groups that feed into the ACFM. Consider the following exchange:

> Q 4.16 Scientist One: Yes, you are adding another rinkydink. We should stop pretending we know how many fish there are. Scientist Two: That is where we are going. The trend is there, but the scale is wrong. Scientist One: The system will use it at the Council of Ministers. Scientist Two: That is why I want all these caveats. *(Observer's notes at the Working Group on the Assessment of Demersal Stocks in the North Sea and Skagerrak meeting, September 2003)*

The text here is the mechanism that allows the scientists to continue to see themselves as doing science while having to articulate results in the midst of great uncertainty. The scientific norm of organised scepticism (Merton 1968b) retains a good deal of force. Scientists want to be either sure or silent, with the text they can at least show where they are sure about what they do not know.

Not all scientists are comfortable with this use of text, and this depends on the kinds of advice being developed. Consider the following point made by a scientist at a meeting in reference to the development of advice for EAFM.

> Q 4.17 If you give loose, non-quantitative advice to managers, then that is what they want because it gives them permission to do what they want, so you shouldn't do it because they get used to a casually written narrative essay. *(Observer's notes at the Consultative Committee meeting, September 2004)*

Text is important to others as well. One of our respondents, who is a negotiator for the fishing industry and who was one of the observers at the ACFM, put it this way:

> Q 4.18 Respondent: [In] the consultations between Norway and the EU for example where they use the ACFM advice, from that I know how much they look into the wording of the advice, so it was quite interesting to see how it was done. Interviewer: The wording? Do you mean the text? Respondent: Yes, the text exactly. They look into the text and say why they use this

> word, why do they use 'may' instead of 'must' and is there a hidden meaning in this, they really look into it during consultations, and it was interesting to see how aware they are of the importance of how they put the words, how they make the text.

So while the primary interest of DG MARE may be the numbers found in tables, when these numbers are thrown into a political process, the text sometimes does have an important impact on how the numbers will be used – at least according to one person who is both deeply involved and financially interested in the outcomes. The textual aspects of the advice confront the limits of its role as a scientific recommendation. This is nicely illustrated by the desire of the scientist in Q 4.4 to make the interpretation of the advice contingent on a particular management approach. In the traditional view of science and policy, science is supposed to be the objective other that provides the parties involved in the policy negotiations with an agreed basis for discussion. Yet here it is seen to be very difficult to produce the science without being already involved, at least to some degree, in that discussion. This provides an illustrative grounding for Jasanoff's (2002) argument that to 'politicise' science by making it available for public scrutiny and input can promote the interests of both science and democracy.

Struggles over the science boundary apply as much to people as they do to results. When someone has been stamped a 'scientist', the power to designate a fact is to be used at the behest of the bureaucracy, not the individual scientist. The designation is based on employment rather than on education. In the mainstream view of the role of science in fisheries policy, introduced in Chapter 3, the role of scientists is something you are hired to do rather than something you are trained to be, though obviously the training is a prerequisite to the hiring. Indeed, one scientist told us that not all members of expert groups have university degrees. One that we talked with had been hired as a technician and received a degree in statistics after he was already a regular participant in the assessment group. Biologists working for the industry are lobbyists and negotiators, even if scientists find them easier to work with than they do fishers. Administrative bureaucracies often seek to define scientists (and themselves) as non-stakeholders. They resist seeing scientists as stakeholders because if they are stakeholders, then they bring their own values and interests to the debate, and not merely facts to be used at the discretion of others. Again in the words of DG MARE (Q 3.7, CEC 2003a, p. 15), if the scientists want 'credibility and influence', they must keep their 'distance'. From the scientists' perspective, the distance may make the promise of influence look somewhat empty when, as in Q 4.4, they are trying to guess the managers' intentions.

The scientist who had his 'wrist slapped' in Chapter 3 reflects this way on the science boundary:

> Q 4.19 Interviewer: But how do you make that distinction when sort of, the boundary between what is science and when it turns into management? It's not always that clear cut. Respondent: It's not always clear and ... yeah, one can end up in a deep hole [laughs]. I'm always aware of it, but it isn't always successfully avoided. But that's also quite an exciting part of the job as well, I think. Interviewer: You like it? Respondent: Yes, I like, I like the confrontational aspects of – of science, industry, officials, and the tension that's associated with that. I find that quite interesting. In fact, it's the only interesting part of the job at the moment. There's no science left. That's my impression. Not in the role I have.

As we will see in Chapter 5 the feelings and experiences that the respondent is pointing to with his exaggerated phrase 'there's no science left' is both widespread among fisheries scientists and more problematic for many of them than this quote would indicate.

Here is another example of a Commission scientist describing the need for a feedback process in the face of the fact that a simple 'the advice is X' ignores the interdependence between advice, management measures, and fishing activities. The topic under discussion is how to develop a management strategy approach to management:

> Q 4.20 Commission Scientist: As a manager I can illustrate some points here. Baltic cod is illustrative; it is a single-species fishery. The BSFC [Baltic Sea Fisheries Commission] developed a management plan to maintain the two stocks above the Bpa, they set a harvest control rule, and an increase in biomass of 30% per year. **The advice came out that this was OK, but the unreported landings were too high to make it real. At the same time we got new advice that because of this uncertainty, they refused to give us short-term assessment**. So the advice is 'no fishing'. These are the two problems. We don't get the science to implement it, so the plan on paper does not work. It is very clear that in addition to the SSB objective, **we have to address the problem of scientific input and the availability of knowledge, a feedback process**. *(Observer's notes at the Advisory Committee for Fisheries Management meeting, September 2004)*

In the following quote, from the same Commission Scientist in response to the October 2003 ACFM decision about multi-fishery advice, the last sentence borders on lamentation.

> Q 4.21 What we have now is in some ways weak advice. We say you do this, you do that. There is no data besides one table offered about interactions. It leaves managers to decide how they can define fisheries and take bycatch considerations. We have the same situation as last year. We are on our own in Brussels. *(Observer's notes at the Advisory Committee for Fisheries Management meeting, October 2003)*

No one involved in the production of scientific advice in support of fisheries management would suggest that the drawing of the science boundary is not problematic. DG MARE sees the problem in the context of the mainstream view of the role of science in policy (Q 3.7), and this leads them to cast the problem in terms of the clarity of statements of management objectives. 'Unclear statement of objectives' is a phrase very often heard in critiques of policy by natural scientists. DG MARE describes the problem in detail as follows:

> Q 4.22 One of the difficulties with much current scientific advice is that the division of labour between the scientist and the manager is sometimes confused. Some scientific advice may be based on assumptions about policy objectives that are the responsibility of the manager, with the result that the advice becomes open to question because of its policy assumptions. It is therefore important that requests for scientific advice be formulated in a way that leaves no doubt as to what assumptions the scientists are being asked to make. At least two approaches are possible. The first is for the management authority to state clearly what its management objectives are and to 'impose' those constraints on the scientists. This approach might be followed by the Community in the case of agreement on multi annual management plans, where targets in terms as, for example, biomass, fishing mortality rate, yields or catch stability could be fixed. The second is for the management authority to request advice on different management options before deciding on which one to choose. In this case, those giving advice would be required to identify the assumptions underlying such options and to indicate the alternative strategies to be followed. Greater clarity concerning the assumptions about policy objectives will be needed (CEC 2003a, p. 13).

They want to make the process tighter and the goals clearer. The science process itself remains a linear one with requests for sets of facts being responded to with the provision of facts. This makes an interesting contrast with the scientists from both ICES (Q 4.6, Q 4.8, Q 6.1, Q 6.4) and DG MARE (Q 6.4) calling for greater reflexivity, conditional advice and ongoing interactions across the science boundary. The second choice they mention, however, is very close to the idea of scenario-based participatory modelling that is an important emerging model for new kinds of scientific practice.

Much like the mainstream view of the role of science, in the face of these actual experiences, the notion of clear statements of management objectives contains utopian elements. Management objectives are set by political negotiations in the face of changing environmental conditions. Science is done in a highly variable climate of uncertainty from both physical and social sources. Hence, in fisheries, objectives that are clearly stated are almost always very abstract and become unclear as soon as they begin to be operationalised. While it is important to keep trying to make them as long-term as possible (e.g. greater use of HCRs), they will never stay clear for very long, and their clarity cannot be relied upon as the basis of a smoothly running linear advice system.

4.3 Conclusion

Within the advisory system of the CFP, there is a marked desire for increased centralised control that is having some real impact. This can be seen in the desire for an in-house advisory system, the increased reliance on STECF, and the ongoing tightening of the European data-gathering and fisheries-monitoring systems. These changes are in response to very real needs if the CFP is going to become a more effective fisheries management system. However, the many costs involved, both financial costs and costs in legitimacy and democratic governance, simply reflect the fact that the CFP is an attempt to manage fisheries on a continental scale. To some extent this reflects the biological requirements of managing shared stocks, but this has been intensified for reasons of European politics.

For very good reasons rooted in the complexity of the required information and knowledge, the global trend in natural resource management in the past 30 years has been towards making decisions at the lowest possible level. This begins with trying to leave as much discretion in the hands of individual fishers as is feasible and then moving upwards. The creation of the RACs reflects this in Europe, but the way this creation was such a baby-step in the direction of the cooperative management systems used in the rest of the developed world is another reflection of how slow Europe has been in modernising its fisheries management. The tendencies towards increased centralisation, while necessary to make the CFP work, move in the opposite direction. Larger-scale systems have to rely much more on mechanisms that shortcut communications. They squeeze out the communicative rationality that makes systems sensitive to the need for change.

This squeezing out of communicative rationality takes various forms in this case study, but most of the important ones have to do with giving space for reflection and review of where the science boundary needs to be set. When observing ICES, I rarely heard the ‘engineering versus science’ comparison that I encountered in DG MARE (Q 4.7). In ICES science versus advice is the much more common distinction. This indicates a large jump in understanding of both science and advice between the two institutions. The engineering rhetoric is a way to side step the question of scientific legitimacy from peer review. Engineering does not require peer review; it demonstrates its credibility when the engineered product works. DG MARE is looking at this in a very similar way, but ‘works’ does not mean here, at least not in the first instance, ‘accurately characterised the state of a fish stock’. The history of the CFP indicates that ‘works’ has not meant maintaining stocks of fish. ‘Works’ means sufficient to allow the regulatory process to move forward. This is a very short-term perspective.

It is not driven by the desires that individual scientists at DG MARE have for the way the system should work, it is driven by the pressures they are under to keep the TAC Machine running. For the Advisory Programme to operate as an effective boundary organisation, even in the relatively simple problem of single-species fisheries management, it must make time and space for reflection under strong pressure from the broader system for clear and quick results. As will be discussed in Chapters 7 and 8, this is an important point where the question of social power forces its way back into what is on the whole a functionalist analysis of what is needed for an EAFM.

As we will see in later chapters, the debate within ICES between science and advice is very much a parallel of this discussion; advice there is what allows the science to work for the client. Both groups are looking for Guston's (2001b) serviceable truths. What counts as serviceable within the management bureaucracy seems to be much less dependent on the procedures of scientific legitimacy. From DG MARE's point of view, however, this is not because they do not care how credible the number is, it is because the system that produced the advice is so large and expensive that they have no realistic way of assembling a better one.

Scientific boundary work is more than simply determining what sort of facts and results will be stamped 'science' and which will not. Also included are questions about who will play the role of science, what data the scientists will have available to them, how they will present their work; and even the ways that they will behave as they play their scientific roles are in dispute. The boundary is very vulnerable to pressures, and the scientists doing the boundary work find themselves badly squeezed. This becomes acute when uncertainty rises high because then issues of transparency of argument and methodology become entangled in issues of trust and participation. The next chapter gives a glimpse of just how squeezed these scientists feel.

5. Attitudes and working conditions of ICES advisory scientists

With the co-authorship of Troels Jacob Hegland

The focus of this chapter is the experience and attitudes of individual scientists within the fisheries advisory system. Most of the information is taken from the survey of fisheries scientists, but we have added a number of quotes from meetings and in-depth interviews where this helps give a fuller picture. One important task is to compare the experience of fisheries scientists who are more involved in the advice generation system with that of their colleagues who are less involved.

Most of the tables draw comparisons between scientists who work for different kinds of employers or based on the type of the last expert group they participated in, which we consider the best single measure in the survey of participation in the advisory system. We consider this measure to be a good one, but far from perfect, as explained in Appendix 1, where the details of the survey methodology are outlined. Basic information about expert group participation is given in Table 5.1. The types of attitude scales used here are the most useful for the two tasks of comparing differences in attitude among groups and looking for correlations among the attitudes themselves. It is also reasonable to make rough statements about where on a scale a group of respondents scores, e.g. 'well above the neutral point' or 'around the neutral point', but these scales are not meant to invent and then measure precise differences among peoples' attitudes.

This chapter has three parts. The first focuses on the impact of the advisory system on scientists' careers and working conditions. The second focuses on scientist's attitudes towards the precautionary approach that frame much of how fisheries scientists see the meaning of their advisory task. The third section focuses on scientists' attitudes towards the advisory task itself.

5.1 Advice provision, career and working conditions

5.1.1 Expert group participation

The simplest way to get at working conditions through a survey question is to ask: 'Please rate your overall job satisfaction.' The results related to place of employment are reported in Table 5.2. NFI scientists have a lower overall job satisfaction than all other employment categories. The scientists employed in academic institutions place themselves close to the mean, and scientists employed in NGOs have the highest job satisfaction. One of the main differences between NFI scientists and scientists in the other employment categories is the former's greater involvement in assessment expert groups, as shown in Table 5.1.

Those whose last expert group was an assessment expert group rate their overall job satisfaction lower than other scientists (Table 5.3). Do these numbers reflect the expert group itself or simply being employed somewhere where more people are sent to assessment expert groups? To answer this question, we analysed job satisfaction for each employment category in relation to expert group participation. The results for NFI scientists are reported in Table 5.4. The table shows that lower overall job satisfaction is most closely related to the last expert group being an assessment expert group. Scientists who participate in assessment expert groups rate their job satisfaction lower than other scientists from their own employment category.

Table 5.1 Composition of survey sample – employer and expert group attendance

		NFIs	Academia	NGO	EU	Other private	Other gov.	Total
Not attended expert group meeting	Row percentages	27	39	6	8	15	5	100
	Column percentages	18	54	100	34	40	47	34
Last attended expert group meeting not directly dealing with assessment	Row percentages	53	25	0	6	11	5	100
	Column percentages	40	39	0	31	33	53	38
Last attended assessment expert group meeting	Row percentages	73	6	0	9	12	0	100
	Column percentages	42	7	0	34	27	0	29
Total		50	24 100	2 100	8 100	12 100	4 100	N = 418

Percentages are rounded and do not always add up to 100. Excluded: 20 who did indicate participating in an expert group but did not make clear which kind of group and therefore did not fit in any of the three categories; six who failed to answer whether they had been in an expert group or not; and 21 who did not identify their employer.

'I moved away from assessment and advisory groups six years ago; feeling better,' one scientist said in an interview. Another scientist explained why he rated his job satisfaction so low:

> Q 5.1 I am currently over-committed to projects in my laboratory. ICES is putting increasing demands on me to work on ad hoc fast-track advice and review groups. The quality of assessment data is deteriorating and there is still an expectation at WGs to come up with good scientifically based assessments, which cannot be done!

NFI scientists who last participated in non-assessment expert groups actually rate their overall job satisfaction somewhat higher than those who have not participated in expert group meetings at all in the last five years. While some non-assessment expert groups have had some advisory functions in response to requests from ICES clients about marine management issues outside of fisheries, the majority place greater emphasis on research questions of direct scientific interest to participants. They are attractive opportunities in many ways for the scientists, almost like long and well-focussed conferences. This distinction between the Advisory Programme and the Science Programme within ICES is a very important one. With the rise of EAFM discussed in Chapter 6, more involvement of non-stock assessment groups in advice production is needed, and this means some cultural changes are required.

Because these non-assessment group meetings are more attractive than the assessment groups, there is a tendency for very senior scientists to attend them rather than the assessment groups (Table 5.5). A third of the attendees in the non-assessment expert group are very senior scientists. Only a fifth of the attendees in assessment groups fall in this category. The data also show that the most junior scientists are a bit more likely to be assigned to attend the assessment expert groups.

The training of scientists for assessment work is an issue that ICES emphasises. Assessment work requires specific quantitative skills that are not always sufficiently covered in the education of biologists. One of our interviewees stated that 'biology is seen as the thing to do if you want to do science and can't do maths'. The same scientist indicated that those mathematicians or biologists who are involved in assessment work often get involved 'by accident' and then subsequently have to acquire the necessary skills. ICES conducts various courses related to the training of assessment scientists. Junior staff are also brought to expert groups specifically for training, which then takes place through a master-apprentice type of relationship.

Table 5.2 Means of scales of perceptions of working conditions by type of employer

		NFIs	Acad.	NGO	EU	Other private	Other gov.	Total Mean	Total N	Total P
Please rate your level of overall job satisfaction.	Mean	5.12***	5.35	6.10*	5.15	5.67**	5.65	5.29	439	0.02
1 = I am not at all satisfied, 7 = I am very well satisfied		N	220	104	10	33	55	17		
In the past three years how has the pressure you experience on the job changed? Only those with same job title for last three years are included.	Mean	5.3	5.59**	5	5.70*	5.13	5.08	5.37	345	0.08
		N	179	81	8	27	38	12		
1 = decreased a great deal, 7 = increased a great deal										
To what extent does your employer encourage your participation in externally funded research?	Mean	5.71	6.13***	4.30***	5.52	5.52	4.76***	5.7	434	0
		N	218	104	10	33	52	17		
1 = it is strongly discouraged, 7 = it is strongly encouraged										
Approximately how many days did you spend on job-related travel in 2004?	Mean	45	35***	35	46	57***	39	44	438	0
		N	220	103	10	33	55	17		
How do you feel about having to travel this much?	Mean	4.23**	3.95	3.8	4.27	3.85	3.82	4.1	433	0.25
1 = I would enjoy travelling more, 7 = this is far too much travel		N	219	100	10	33	54	17		
How much does your job encourage you or hinder you from producing the number of peer-reviewed publications you feel you would like to be producing?	Mean	3.82**	4.90***	3.22	3.61	3.71	3.5	4.03	431	0
		N	217	104	9	33	52	16		
1 = severely hinders, 7 = strongly encourages										

** indicates significance at 0.10, ** indicates significance at 0.05, *** indicates significance at 0.01. Asterisk indicating significance refers to differences between the category and all other categories combined. Excluded in all questions are 22 respondents who did not identify their employer. Furthermore, four did not rate their job satisfaction; five did not rate the changing pressure, and 93 did not confirm holding the same job title for the last three years; nine did not rate the employer's encouragement to externally funded research; five did not indicate how much travelling they did in 2004; ten did not indicate what they felt about the amount of travelling; and twelve did not rate the employer's support for peer-reviewed publications.*

5.1.2 Work pressure

Work pressure related to assessment expert groups is high and increasing, as Q 5.1 suggests. In interviews, other scientists complained about the overall amount of work in connection with assessment expert groups. One of the scientists told us that he had had to take two months off because of stress. Another told us that the meetings of one expert group, where our research project had an observer team, had become increasingly chaotic and that it was impossible to stick to the planned and already long working hours. This was emphasised by a scientist who approached our observer to say that the previous day's work had carried on to 4 a.m. This was the only time a scientist at any meeting we were observing – and all the scientists at these meetings knew why we were there – asked that something go into our report.

The uncertainty involved in assessments seems to be a major factor in not being able to stick to the planned working hours. One scientist linked this to software problems.

> Q 5.2 Interviewer: It seemed like uncertainty was a major driver in making people stay up all night. Does that sound sensible to you? Yes, you are always thinking, what if I tweak this? The problem is that a lot of the software we use is not user friendly, you can tweak quickly, but then you are cutting and pasting for a couple of hours for figures.

This scientist never feels that the job is done. This is partly due to the time constraints in an overstretched system. It also shows an entanglement between the uncertainty of the scientific tasks and the difficulties of managing these tasks. The high uncertainty about nature also means high uncertainty about when you have done enough in attempting to describe nature.

One survey question read: 'In the previous three years, how has the pressure you experience on the job changed?' To get a picture of actual changes in the work pressure, rather than changes related to advancement, we report only the results of respondents holding the same position as three years earlier (Table 5.2 and Table 5.3). The correlation between job satisfaction and experience of changes in pressure at the job is -0.23 ($p = 0.01$), suggesting that increasing pressure at work is moderately linked to lower overall job satisfaction at a general level. However, pressure can increase simply because the scientist gains more experience and thereby seniority, even though he or she remains in the same position. In fact, increased pressure is not necessarily experienced as negative. One respondent indicated that his job pressure had increased substantially but commented on it in this way:

Table 5.3 Means of scales of perceptions of working conditions by type of expert group

		Not attended expert group meeting in last five years	Last expert group did not deal with assessments	Last expert group did deal with assessments	Total		
					Mean	N	P
Please rate your level of overall job satisfaction.	Mean	5.45*	5.42	4.93***	5.29	437	0
1 = I am not at all satisfied, 7 = I am very well satisfied		N	147	166	124		
In the past three years how has the pressure you experience on the job changed? Only those with same job title for last three years included.	Mean	5.26	5.36	5.45	5.36	343	0.46
		N	105	133	105		
1 = decreased a great deal, 7 = increased a great deal							
Approximately how many days did you spend on job-related travel in 2004?	Mean	39**	41	51***	43	435	0
		N	144	167	124		
How do you feel about having to travel this much?	Mean	3.62***	4.2*	4.38***	4.06	429	0
1 = I would enjoy travelling more, 7 = this is far too much travel		N	141	164	124		
How much does your job encourage you or hinder you from producing the number of peer-reviewed publications you feel you would like to be producing?	Mean	4.54***	4.14	3.25***	4.02	429	0
		N	143	164	122		
1 = severely hinders, 7 = strongly encourages							

** indicates significance at 0.10, ** indicates significance at 0.05, *** indicates significance at 0.01. Asterisk indicating significance refers to differences between the category and the other two categories combined. Excluded in all questions are 20 who did indicate participating in an expert group but did not make clear which kind of group and therefore did not fit in any of the three categories, and six who failed to answer whether they had been in expert group or not. Furthermore, two did not rate their job satisfaction; five did not rate the changing pressure, and 91 did not confirm holding the same job title for the last three years; four did not indicate how much travelling they did in 2004; ten did not indicate what they felt about the amount of travelling; and ten did not rate their job's support for peer-reviewed publications.*

> Q 5.3 Being a young scientist the greatest changes in my work activities have been increases in responsibility, which we consider very valuable. The diversity of activities (sampling, writing proposals, analysing, publishing, giving presentations, coaching students, etc.) has also increased.

The survey data, however, do not show a relationship between assessment expert group participation and increased job pressure over the past three years (Table 5.3). The same is true if we look only at NFI scientists (Table 5.4). We know that the advisory process is making increasing demands, but because of the increased external control pointed out by the Mode Two theorists, all scientists are under more pressure from many different directions.

The fisheries scientists estimated that they travel on average 43 days per year in connection with their work (Table 5.3). Those whose last expert group was an assessment expert group travel the most (Table 5.3); on average, they estimated that they travel ten days per year more than others. Those in assessment expert groups are also more dissatisfied with the amount of travelling than others (Table 5.3). One scientist involved in assessment work said in an interview:

> Q 5.4 For the good of your health, you can't carry on like that. [I have] spent half my life away from the lab under difficult circumstances in the last three years, often having to travel at less than 24 hours notice for three to four days. This was constant.

Q 5.4 suggests that the problem is not the amount of travelling as much as the circumstances of the travel. In addition to the short notice, some British scientists felt that the implementation of the EU Working Time Directive has removed an important incentive for travelling by cutting the amount of overtime that can be saved up. This means that the travelling scientist is not able to compensate his or her family or social life by taking time off at another date to the same extent as before. Table 5.2 shows that there are large differences in the amount of travel between scientists in the different categories of employment. Scientists from academic institutions travel least, with a mean of 35 days a year. Scientists in the 'other private' group travel the most, with a mean of 57 days a year. Unlike the case of assessment scientists, however, this does not seem to translate into dissatisfaction with travel in general (Table 5.2). It is also important to point out that all of these differences in attitude towards travel revolve very closely around the neutral position (4). The level of dissatisfaction with travel may vary systematically between different groups of scientists, and we know from interviews that there are strong feelings about travel among some individuals, but the dissatisfaction is not very strong overall.

5.1.3 Research and publications

Participation in assessment expert groups negatively influences the scientists' opportunities to publish in peer-reviewed publications. One question was: 'How much does your job encourage or hinder you from producing the number of peer-reviewed publications you feel you would like to be producing?' The mean for all scientists is almost exactly the neutral four (Table 5.3). However, for the group of scientists who last attended an expert group that dealt with assessment, the mean is 3.25. Scientists who did not attend an expert group meeting in the last five years constitute the other extreme with a score of 4.54. The difference is even more pronounced if we look at NFI scientists alone (Table 5.4). It is not simply publications as such; publications represent the ability to remain focussed on valuable research. One scientist currently chairing an assessment expert group suggested in an interview that the amount of travelling involved in ICES and EU work meant that he could seldom 'sit and pursue a line of research'.

As many as one-quarter of the scientists indicated that they would *absolutely* be willing to forego career advancement in order to spend more time doing research. We say 'absolutely' because this figure is based only on those who checked 7 on the scale of 1 to 7; if 6 is also included the figure is over 50%. Ironically, in most institutions employing scientists, it is quite normal to be promoted out of research to research administration or other administrative duties. The two first questions reported in Table 5.6 relate to how important it is to the respondent to do research rather than other tasks. NFI scientists and scientists employed in academic institutions score relatively high on their desire to do research. Several scientists added as a comment to the question that they have already passed over an advancement possibility because of the administrative duties involved. NGO scientists score lower on these questions, which is not surprising as these positions usually have no research component.

In all employment categories, advancement is perceived as leading to fewer chances to do research. However, NFI scientists are more pessimistic than scientists from academic institutions and NGO scientists. This corresponds to the over-representation of NFI scientists among those mentioning administration in the open-ended question on changes (Table 5.7).

The most interesting information in Table 5.6 is, however, the distance between the scientists' emphasis on research and the possibilities of doing that later in their career – the distance between wishes and expectations. NFI scientists show the biggest difference. NGO scientists end up with a very small difference, based mainly on the lesser desire to do research. The relative match between wishes and expectations is, no doubt, part of the reason for the high job satisfaction that NGO scientists reportedly enjoy (Table 5.2).

Table 5.4 Means of scales of NFI scientists' perceptions of working conditions by type of expert group

		Not attended expert group meeting in last five years	Last expert group did not deal with assessments	Last expert group did deal with assessments	Total		
					Mean	N	P
Please rate your level of overall job satisfaction.	Mean	5.08	5.46***	4.78***	5.11	208	0.01
1 = I am not at all satisfied, 7 = I am very well satisfied		N	37	83	88		
In the past three years how has the pressure you experience on the job changed? Only those with same job title for last three years included.	Mean	5.14	5.18	5.44	5.3	169	0.31
1 = decreased a great deal, 7 = increased a great deal		N	28	61	80		
Approximately how many days did you spend on job-related travel in 2004?	Mean	41	41	50**	45	207	0.1
		N	36	83	88		
How do you feel about having to travel this much?	Mean	3.64***	4.24	4.42*	4.21	206	0.01
1 = I would enjoy travelling more, 7 = this is far too much travel		N	36	82	88		
How much does your job encourage you or hinder you from producing the number of peer-reviewed publications you feel you would like to be producing?	Mean	4.69***	4.02	3.23***	3.81	205	0
1 = severely hinders, 7 = strongly encourages		N	48	83	86		

** indicates significance at 0.10, ** indicates significance at 0.05, *** indicates significance at 0.01. Asterisk indicating significance refers to differences between the category and the other two categories combined. Excluded in all questions are 20 who did indicate participating in an expert group but did not make clear which kind of group and therefore did not fit in any of the three categories, six who failed to answer whether they had been in expert group or not, 21 who did not identify their employer, and 209 who did not work in NFIs. Furthermore, one did not rate job satisfaction; two did not rate the changing pressure, and 38 did not confirm holding the same job title for last three years; two did not indicate how much travelling they did in 2004; three did not indicate what they felt about the amount of travelling; and four did not rate the employer's support for peer-reviewed publications.*

5.1.4 Funding and administration

One of the things that the Mode Two approach emphasises is the relationship between the way science is funded and how it is carried out, especially in relationship to quality control. One of the most important changes in fisheries science in recent years has been the change towards reliance on 'soft' money that scientists or institutes have to apply for. This has affected working conditions in several ways:

> Q 5.5 When we first came here ... in those days the government used to give us a pot of money and say, 'here's your pot of money. Do what you like with it, as long as you keep us happy.' Um, internally we would then compete for shares of that money to do interesting biological studies that would actually help.

The scientist indicates that the current funding arrangements are not leading to better research, rather the contrary. This particular scientist suggests that scientists had previously been able to decide for themselves and produce better – or at least more interesting – research results instead of being caught in fulfilling contractual demands.

Table 5.5 Type of last expert group by seniority

Seniority	Definition	Type of last expert group Non-assessment group		Assessment group		Total N
		Row %	Col %	Row %	Col %	
Very senior	PhD before 1986 or	68		32		69
	MSc before 1984		33		20	
Senior	PhD 1986-2001 or	54		46		135
	MSc 1984-1999		51		58	
Junior	PhD after 2001 or	50		50		46
	MSc after 1999		16		22	
Total N		143		107		250

Relationship is significant at .09. Excluded in all questions are 148 who did not attend any expert group, 20 who did indicate participating in an expert group but did not make clear which kind of group, six who failed to answer if they had been in an expert group or not, and 41 who did not report the year of their terminal degree.

Table 5.6 Means of scales of perceptions of research and career by type of employer

	NFIs	Acad.	NGO	EU	Other private	Other gov.	Total	
							Mean	P
How important is it to you that much of your time is spent doing research rather than advocacy, administration or management? 1 = not very important, 7 = very important	5.62*	5.96***	3.30***	5.42	4.83***	4.41***	5.49	0
Would you be willing to forego career advancement in order to spend more time doing research? 1 = no never, 7 = yes, definitely	5.09	5.37**	3.78**	4.97	4.65*	5	5.06	0.05
In the job you have now, how does advancement affect your chances to do research that you think is valuable? 1 = far fewer chances, 7 = many more chances	2.74***	3.52***	3.2	2.91	3.26	2.56	3.01	0
Difference between a scale based on first two questions and the third question	-2.61***	-2.14	-0.22***	-2.24	-1.45***	-2.22	-2.27	0
Total N of difference = 418	211	103	9	31	48	16		

** indicates significance at 0.10, ** indicates significance at 0.05, *** indicates significance at 0.01. Asterisk indicating significance refers to differences between the category and all other categories combined. Excluded to reach N of 418: 22 respondents did not identify their employer; five did not rate importance of research vis-à-vis other tasks; ten did not rate willingness to forego career advancement; and ten did not rate effect of advancement in relation to research. A reliability analysis between the two first scales yielded a Cronbach's alpha of .634.*

The scientists in our survey were asked to give a short description of the most important changes they have experienced in their work activities since assuming their current position. This was an open-ended question, meaning that no answers were suggested. As it turned out, answers to this question fell into two categories. One group of scientists discussed concrete changes in fisheries research issues, for instance the increasing focus on ecosystems. The other group discussed changes in their working conditions. Two issues recur in many of the answers: funding sources and administration. The answers of several respondents included both, for example:

> Q 5.6 A lot of pressure (and time spent) to secure funding for research and less time to actually do research. More bureaucracy, meetings and management responsibilities.

To get an idea about any interesting differences between those mentioning funding and/or administration and those who did not, the answers to this question were coded so that they could be compared with other questions in the survey database. Answers were placed in two categories with respect to both funding and administration. For funding, answers that referred to changes in funding sources were placed in one category, while answers that did not mention funding or only mentioned general changes in funding levels were placed in the other. For administration, answers mentioning management, administration, bureaucracy, or meeting activity were placed in one category, and all other answers were placed in the other. Two coders were used as a check against bias (Table 5.7).

Sixty scientists mentioned changes in funding sources. This is a considerable number given that the responses were the result of a completely open-ended question about general changes. Several mentioned that the need to write applications for funding puts additional pressure on them. Some also argued, as in Q 5.5, that the changes in funding sources have had implications for the type of research carried out. One respondent argued as an example that the 'decrease in central funding and the need to seek outside funding [is] limiting the opportunity to undertake basic research'. Differences in the perspective on funding relate also to the type of employer. Table 5.7 shows that 24% of the scientists in academic organisations mentioned this change, but only 14% of the scientists in the other groups combined.

Another question asked how much the scientists' employers were encouraging them to participate in externally funded research. The answers revealed significant differences across the categories of employers. The overall mean was 5.7 on a scale from 1 to 7, which suggests fairly strong encouragement. NFI scientists were close to the overall mean, whereas scientists from academic institutions gave a significantly higher rating (Table 5.2).

Scientists employed in academic institutions are also the second highest employment group in experiencing increased pressure on the job (Table 5.2). It is likely that this feeling of increased pressure is related to the stronger emphasis on external funding. One academic scientist said with respect of funding that he felt a 'pressure to do it, but no other tasks are taken away to permit this'. The survey data also show that those who mention sources of funding as an important change also feel a greater increase in work pressure (5.82 v 5.38/7 P = 0). NGO scientists feel the least pressure from their employer to obtain external research funding. This may mean that the pressure on them is to find funding for advocacy, or that NGOs tend not to use their scientific staff for fundraising because it is carried out by others.

Table 5.7 Percentages mentioning issues of funding and administration by type of employer

Subjects mentioned without prompting in replies to the following open-ended question: 'Please give a short description of the most important changes you have experienced in your work activities since assuming your current position'		NFIs	Not NFIs	Acad.	Not acad.	N
Administration as important change	Mentions issue	39	26			117
	Does not mention issue	61	74			242
Funding sources as important change	Mentions issue			24	14	60
	Does not mention issue			76	86	299
	N	183	176	82	277	Total N 359

Rounded percentages. Excluded: 22 did not identify their employer and 84 did not describe the most important changes. NFIs against all others combined gave a Chi-Square of .01 for administration. Academia against all others combined gave a Chi-Square of .03 for funding.

The open-ended question on most important changes in work activities also generated many answers, almost one-third of the total, mentioning administration along the lines of Q 5.6. Scientists employed in NFIs mention these issues significantly more often than others do (Table 5.7). Some 39% of NFI scientists mention administration as opposed to 26% of all other groups combined. One scientist offered the observation that there is a 'much greater emphasis on administration and monitoring of work targets with no improvement in work output'.

Administrative pressures are to some extent related to pressures from the industry, the EU and other elements. In particular, assessment scientists have to be increasingly aware of how they formulate their advice, how they present uncertainties, that their assessments are consistent from species to species, etc. As discussed in Chapter 7, these issues are having a tremendous impact on how ICES is organising itself, certainly with respect to the traditional advisory structures, but increasingly in terms of changes in funding structures and administrative loads. These pressures are having a number of unintended, negative consequences on the lives of these scientists.

Table 5.8 Patterns in expert group assignments by gender

	All respondents Type of last expert group			Total	P
	No WG	Non-assessment WG	Assessment WG		
Men	76%	84%	80%	80%	0.15
Women	24%	18%	20%	20%	
Total N	148	167	123	438	
		Employees of National Fisheries Institutes			
Men	68%	86%	82%	81%	0.08
Women	32%	14%	18%	19%	
Total N	38	83	87	208	

Excluded in all questions are 20 who did indicate participating in an expert group but did not make clear which kind of group, six who failed to answer if they had been in an expert group or not, and one who did not indicate gender. The second question also excludes 21 who did not identify their employer, and 209 who did not work in NFIs.

5.1.5 Gender

Fisheries science in support of fisheries management has noticeable gender patterns, and some female fisheries scientists have related experiences of discomfort in professional situations because of their gender. Compared to many other sciences, the proportion of female scientists is low. Participants in all of the meetings we observed were largely middle-aged white men. Only 20% of our survey respondents are women, and this distribution holds fairly evenly across all employment categories. Women's numbers within fisheries science are increasing. Among the most junior third, the respondents who received their last degree within the past seven years, 32% are women. Among the most senior third, respondents who received their last degree 17 or more years ago, only 5% are women.

Women rate their job satisfaction slightly lower than men do at 5.09/7 compared with 5.36 ($p = .09$). A weak pattern is visible in the assignment of men and women to types of expert groups (Table 5.8). Women are over-represented among those who do not attend expert groups and under-represented among those attending the more desirable non-assessment expert groups. Assignments to assessment expert groups show no gender pattern. This pattern is the same, but less pronounced, among the more junior third. It is statistically significant at 0.10 only among NFI employees (Table 5.8). The overall picture seems to be a slowly changing working environment where full gender integration is being approached, but still not reached.

5.2 The precautionary approach

Most fisheries scientists understand their advisory task as using the best available science to provide advice for implementing a precautionary approach to fisheries management. The precautionary approach is not itself a scientific concept, but rather a framework for the delivery of science. It is not merely an official doctrine. In expert group and other meetings there are constant references to the precautionary approach as the guiding principle for making judgements in uncertain situations. As discussed in Chapter 3, in a way similar to the EAFM, it is a very important 'scientific ideology' in fisheries science in Europe.

Table 5.9 Responses to attitude scales about the precautionary approach by type of employer

		Type of employer							
		NFIs	Academia	NGO	EU	Other private	Other gov.	All	P
It is critical that fisheries management be risk-averse and chooses lower fishing pressure when stock condition is uncertain. Strongly disagree = 1 … 7 = strongly agree	Mean N	5.71 220	5.83 105	6.3 10	5.3 33	5.44 54	5.00* 17	5.66 439	0.12
To what degree should judgements made in preparing scientific advice be influenced by the precautionary approach? never = 1 … 7 = always	Mean N	5.94 219	6.21*** 104	6.60* 10	5.39*** 33	5.49*** 55	6 17	5.92 438	0

** indicates significance at 0.1, *** indicates significance at 0.01. Asterisk indicating significance refers to category compared to all other categories combined. Excluded in both questions are 22 who did not identify their employer. Furthermore, four did not answer the question on risk-adverse fisheries management, and five did not answer the question on judgements.*

The precautionary principle as a general norm in environmental management came to the fore through the Rio Declaration (UN 1992). The United

Nations Conference on Straddling Fish Stocks and Highly Migratory Fish Stocks (UN 1995) first articulated the principle for fisheries with the following definition:

> Q 5.7 States shall be more cautious when information is uncertain, unreliable or inadequate. The absence of adequate scientific information shall not be used as a reason for postponing or failing to take conservation and management measures (UN 1995, p. 6).

The precautionary principle is the official doctrine of the Common Fisheries Policy, and indeed of the EU. The following is from the Council Regulation dealing with the 2002 reform of the Common Fisheries Policy:

> Q 5.8 (3) Given that many fish stocks continue to decline, the Common Fisheries Policy should be improved to ensure the long-term viability of the fisheries sector through sustainable exploitation of living aquatic resources based on sound scientific advice and on the precautionary approach, which is based on the same considerations as the precautionary principle referred to in Article 174 of the Treaty (CEC 2002, p. 59).

The treaty referred to here is the treaty establishing the European Community. The precautionary approach is how the precautionary principle becomes operational. ICES mentions greater emphasis on the precautionary approach in the discussion of its basic mission in its strategic plan, which was formally adopted by the contracting parties in 2002:

> Q 5.9 The Mission statement is noteworthy in terms of the evolution of ICES. Marine ecosystems are inclusive of fisheries, but much broader and more complex. The emphasis on marine ecosystems does not diminish the priority that ICES will give to fisheries. Advice on fisheries will continue to be a prominent part of the ICES programme, with an increased application of the precautionary approach and within a wider ecosystem context (ICES 2002).

Beyond simply doctrine, it is widely supported among fisheries scientists. In the US research, 80% of the marine scientists we surveyed agreed with the statement, ‘It is critical that fisheries management be risk-averse and choose lower fishing pressure when stock condition is uncertain’, while 44% agreed strongly (Wilson et al. 2002).

Table 5.9 lays out the relationship between two statements about the precautionary approach and the type of employer. No relationship was found between these scales and type of expert group. The first statement is a restatement of the precautionary principle, and it gets high agreement across all categories, the lowest being scientists working in the ‘other government’ category. The other statement related directly to the question of the degree to which precaution should influence judgements in preparing advice. Scientists working for the European Commission still scored above the centre of the scale on this question, but below all other categories. The highest scorers were the NGO scientists, six of whom chose 7, with the other four choosing 6.

Even with this kind of general support for the precautionary approach, real disagreements and confusions exist about how it should be implemented. The following quote from ACFM is typical of many such discussions about advice and is a good illustration of both the basics that the scientists agree on and the pressures and temptations that they struggle with in the implementation of precaution:

> Q 5.10 Scientist One: Would it be better to highlight that the estimate is close to Blim, and whether it is above is to a large extent how you do the model, and we can't really tell. We can shortcut the discussion and say that apparently we are very close and it is not an either or. **Scientist Two: So we just say that Blim is very sensitive to the assumptions made in the forecast and a 40% reduction would rebuild to Blim. It would be very difficult to say we think this would do the job in accordance with the precautionary approach**. We have tried to do the best we could with the management plan and **now we are sliding on to saying yes as if these numbers are certain. The precautionary approach is exactly that, we are not in a position to say the stock will recover** [General agreement around the table]. *(Observer's notes at the Advisory Committee for Fisheries Management meeting, October 2004)*

A conceptual framework has been developed and used for the inclusion of the precautionary approach in Europe that identifies 'precautionary reference points'. The basic idea is that stock assessment models are used to identify Blim, which is the lowest level a spawning stock biomass should ever be allowed to get, and Bpa, which is the larger spawning stock biomass used as a target in order to reduce the chance that a stock is ever fished down to Blim. This conceptual framework is what Degnbol (2003) describes as stochastic predictability:

> Q 5.11 ... because [in the implementation of the precautionary approach] the basic concept of predictability was maintained, but the predictions of the effects of management measures were expanded to include an estimate of the associated uncertainty (2003, p. 40).

He argues that while the precautionary approach is about accepting the fact of uncertainty, this approach treats it as a supplementary consideration within the standard and traditional framework of stock assessment models and their associated management techniques. The fundamental question of the conditions of predictability themselves were not examined. This is an excellent example of Shackley and Wynne's (1996) transformation of uncertainty.

A constant tension in the discussions among ICES scientists in the preparation of advice is the movement between precaution, as it is expressed through stochastic predictability, and the underlying issues of uncertainty that resist being expressed as error terms in stock assessment models. The argument is not with the precautionary approach itself but in the way it was defined with respect to single-species stock assessment models in a reductionist manner. As discussed further in Chapter 7, this was seen by many scientists as a block to the development of a comprehensive approach to precaution, multispecies approaches, and finally the EAFM itself. The following quote comes from a scientist who works for the European Commission:

> Q 5.12 Respondent: I have to say that the precautionary approach killed most of the fisheries science in Europe for almost 6 years. Interviewer: What do you mean? Respondent: Simply because they stopped all initiatives. It was like everyone started from scratch in discussing precautionary [processes] and reference points, and by the end of the day what we have achieved after 6, 7, 8 years is hardly anything. It is just more or less the same. But it blocked, at the same time it blocked any initiatives in relation to mixed fisheries species interaction. I would say, basically science was better off in the beginning of the 1990s – fisheries science – than we are today.

Here we see a frustration with what the precautionary approach has done to fisheries science, not simply with respect to advice, but as a discipline. On the one hand, he explains elsewhere in the interview that when the precautionary approach started, it stalled some initial attempts in the early 1990s to refuse to give advice where the information did not warrant it. One could interpret this as saying that the way that precaution was adopted in the mid-1990s as 'stochastic predictability' deflected a deeper critique of the underlying uncertainties of the TAC Machine (Holm and Nielsen 2004) system. He goes on to argue that precaution was probably good for management as such because it forced the system to deal more seriously with what was happening with the fish stocks. But fisheries science began spending all of its energy on the sterile pursuit of precautionary reference points while issues like multi-species interactions were neglected. If this scientist is correct in his historical interpretation, the interpretation of the precautionary approach as stochastic predictability had important implications for fisheries science as a whole and not just for the advice process.

Precaution raises questions about the respective roles of managers and scientists. It is another area where the science boundary is being pushed, but this time the pushing is coming from the scientific community. Strongly but not inaccurately stated, precaution as stochastic predictability has meant the incorporation of scientists' political positions directly into stock assessment models. Precaution is an area where they have very strong prescriptive opinions, and this tension is very difficult and sometimes painful for the scientists. At the end of the day, the precautionary approach is a political rather than scientific principle. It is a judgement about who, most broadly present or future users, should bear the risk of poor fish yields that the uncertainty about the stock condition represents. It is also a judgement in which the scientists and managers have an indirect interest. When stocks collapse, they are the brunt of a great deal of criticism.

The scientists, however, particularly when discussing it in the terms given them by the 'stochastic predictability' approach, often seem to treat it as an objective outcome rather than a political judgement. Some of their observers can be very critical of the degree to which the scientists lean towards the precautionary approach. We asked one biologist, who was employed by the fishing industry and observing ACFM, what surprised him, and he replied:

> Q 5.13 I am surprised by the reluctance to increase quotas. I have always argued among my constituents that people in ACFM are not opposed to fishers, and the appearance that they are is not real. I think I have to revise that to some extent and that is surprising to me. They question any positive trend above and beyond what is reasonable.

The precautionary approach is going to continue to create difficulties for how science is done. The precautionary approach is the place where scientists most clearly take on the role of advocates. Related to this, indeed perhaps a reason for it, the precautionary approach reaches beyond the areas where quantitative models can provide clarity into the truly treacherous depths of applied uncertainty. Many of the real sources of uncertainty in fisheries, let alone an EAFM, simply cannot be reduced to probabilities, so the precautionary approach will never be fully encompassed by stochastic predictability or even the more advanced tools of risk analysis.

In the final analysis, the precautionary approach confronts scientists with a paradox that is a particularly acute one within a scientific culture: the less they know, the stronger their opinion should be.

5.3 Scientists' attitudes towards the advisory task

One of the clearest results of this research is that fisheries scientists' attitudes towards producing scientific advice are strongly negative. Pressures on the science boundary and the constant demand to produce answers for the next cycle of the TAC Machine are having an impact on the scientists' morale. A large number of scientists believe that much of their work is ignored by the management system. This belief, as argued in Chapter 1, is often exaggerated, but it is certainly an often encountered and honestly held perception. Many scientists are experiencing a real discontinuity between what they see as the science they want to do and many of the advisory activities they spend their time on. In an interview, one scientist articulated this very succinctly:

> Q 5.14 I think there is an increase in the amount of time that is directed away from doing what you might call science into simply number manipulation and giving advice.

This dissatisfaction takes two main forms. One is that advice work is simply less scientifically attractive, just 'turning the crank' on stock assessments is often seen as routine and of limited value for career development, especially when one is not one of the 'gurus' who work on developing these methods. This leads to an advice-related frustration with one's own career. The second is a feeling that the advice being produced is not only not taken sufficiently seriously by the system, it has become so divorced from what is going on in the natural world that they are 'pretending', to use the term chosen by the scientist in Q 4.16.

This leads to an even more basic conflict with the scientists' own identities as scientists. We think a good word to describe this experience, one that has a long history in sociology, is 'anomie'. It was coined by Emil Durkheim, one of the 19th-century founders of modern sociology. The American Heritage Dictionary (2000) defines anomie as 'alienation and purposelessness experienced by a person or a class as a result of a lack of standards, values, or ideals'. The anomie of the scientists arises from being asked to play a difficult role under sometimes trying conditions and then having the results of these efforts 'disembedded' from the cultural understandings that produced them. 'Disembedding' here means the separation of the results from the background meanings, practices and assumptions that allow the scientists to consider the results to be 'science'. The respondent we quote in Q 5.14 above illustrates this disembedding at another point in the interview:

> Q 5.15 It's not really science anymore. We're number engineers. We fiddle with numbers to, you know, try to add some scientific credibility to an opinion.

Again we encounter the image of the scientist becoming an engineer, a tinkerer who carries out a technical assignment without reflection, while still being expected to lend scientific credibility and legitimacy derived from more than just what works. The lack of faith in results evident in this quote arises from the way the entire system handles uncertainty. As another respondent said:

> Q 5.16 You can elaborate the methods, but if the data aren't good enough to support them, what's the point? You're just fooling yourself that you're getting a better assessment. The strength of this, the accuracy of the assessment depends on the weakest link in the chain.

Or as the first respondent put it:

> Q 5.17 There are a lot of people who are directly or indirectly involved in the advisory process. We have to rely on people to do data extraction, so we can manipulate the numbers.

A third respondent points to the cyclical growth of bias and uncertainty that he suggests is built into the TAC Machine system and for which it is hard for scientists to determine when they should say that it is impossible to come up with a scientific result.

> Q 5.18 We're giving advice for TACs although it's been clearly stated in the past that they don't work, and though we know that TACs will widely lead to non-compliance, which then screws up your basis for setting TACs in the future. So there is some extent to which we're guessing, really the extent to which boats are misreporting in order that you can estimate the TACs for them to misreport next year. So that is a sort of downward spiral … It's … But at what point would we say 'no we can't …'? I think with a lot of stocks we're quite close to that. It's hard to define exactly what would lead me to make that statement.

A fourth scientist sees a similar systemic trend towards less and less realistic stock assessments, though from a slightly different point of view that adds some technical details to the reasons for this expected decline:

> Q 5.19 Maybe TSA is not suitable, we had a lot of trouble with it at the Northern Shelf with the gadoids, and we did a number of different analyses, and the results from TSA, XSA [types of assessment models] and this survey thing we have been developing all were surprising and contradictory, and we decided to put forth four assessments. This kind of uncertainty about model choice is going to get worse because the quality of catch data is declining. And the survey data as well because they may not be as good at picking up fish when there are few of them.

These observations are not atypical and indicate a growing discomfort among fisheries scientists about the value of their work with respect to fisheries management. Several of the statements we have reported here are emblematic of this. The most striking one in some ways is the plea by the scientist in Q 4.16 that they stop pretending they know how many fish are in the sea. This demand was expressed with complete certainty at a plenary session in front of 25 colleagues (as well as 2 outside observers) who were working to assess some of the most important fish stocks managed by the CFP. The context of the quote was what was going to happen to the results in the larger world after they finished fiddling with the details.

This anomie is happening mainly to the scientists working for the national fisheries institutes directly in stock assessment processes. These are the scientists who have the largest gap between their desire to do research and their perceptions of their future chances to do so (Table 5.6), who are feeling somewhat more pressure from administrative work than other scientists (Table 5.7), are the least satisfied with their opportunities to publish, and who have the lowest job satisfaction. They still, however, rate their job satisfaction above the mean of the scale. It is important in this discussion to recognise that, while this dissatisfaction is real, they are still people with relatively good and interesting jobs who do not suffer the consequences of the problems with the fisheries management system in anything like the way that people in fishing communities do.

One question that the survey data can answer is if this discomfort about the science they are doing has a negative impact on the scientists' job satisfaction. The survey contained the following two questions:

> Q 5.20 In our interviews scientists have reported many different internal reactions to the experience of producing scientific advice for use as the basis of policy decisions. For each of the following quotes, we would like you to indicate how often you have felt this way while participating in the production of scientific advice.
> I am being asked to answer impossible questions.
> I have never felt like that = 1__ 2__ 3__ 4__ 5__ 6__ 7__ = I often feel like that
> and
> I am being asked to create certainty that is not really there.
> I have never felt like that = 1__ 2__ 3__ 4__ 5__ 6__ 7__ = I often feel like that.

Before answers to these two questions can be compared with answers to other questions, attitude measurement theory tells us that one thing has to be examined. This is the question of whether or not they are measuring the same underlying attitude or two different ones. If they are measuring the same thing, the answers should be added together to make a combined scale. A test is necessary because two mistakes are possible here. On the one hand, if we combine the two and they really are different, we run the danger of covering up important differences. But if they are really measuring the same thing, then the measurement is statistically more accurate if the two questions are added. This is especially true when evaluating the relationship between the discomfort about science and other questions because it would create statistical co-linearity. Two basic tests exist. One is simply to ask if a reasonable person would say that the two questions as phrased are basically asking about the same thing. The other is to look at the simple correlation between the two answers, with the rule-of-thumb being that correlations between survey responses of greater than 0.5 means that the questions are addressing the same underlying attitude (Nunnally 1978). We believe a 'reasonable person' would think that 'answering impossible questions' is an idea that is very close to 'creating uncertainty that is not really there', and responses to these two questions have a simple correlation of 0.47. This just misses the 0.5 rule-of-thumb, so our judgement is that it is better to add them together to make a single variable.

The survey question measuring job satisfaction looked like this:

> Q 5.21 Please rate your level of overall job satisfaction.
> I am
> not at all = 1__ 2__ 3__ 4__ 5__ 6__ 7__ = very well
> satisfied with my job overall.

Because many of these people who are involved in advice giving have other reasons for lower job satisfaction, we added the answers to some other questions to the regression analysis to control for their influence. These questions are:

> Q 5.22 My job
> severely hinders = 1__ 2__ 3__ 4__ 5__ 6__ 7__ = strongly encourages
> my production of the level of peer-reviewed publications we would like to be producing.

and

> Q 5.23 How do you feel about having to travel this much? (Referring to a previous question about the amount of travel involved in their job.)
> I would enjoy travelling more = 1__ 2__ 3__ 4__ 5__ 6__ 7__ = this is far too much travel.

and

> Q 5.24 Did you find participation in this working/study group personally enjoyable?
> I detested = 1__ 2__ 3__ 4__ 5__ 6__ 7__ = I very much enjoyed
> participating in this group.

Table 5.9 reports the results of a linear regression of the model just described. The result is quite clear. Controlling for the impact of differences in working conditions and personal enjoyment of the expert group, the more scientists feel that they are being asked to create certainty and answer impossible questions, the less satisfied they feel with their job.

A scientist who had moved from an academic to applied research environment within the last few years submitted the following reflection to us along with the survey answers:

> Q 5.25 I found that in the circles of [applied] fisheries science, there are actually a lot of clever people who are motivated to do things right, but our

scientific work is very much constrained by the inertia of the system we work in, and of course by time constraints. The system we work in is the factor with the most impact, we think. We have seen people doing clever research throughout the whole year, but in the assessment WG they just become different people, suddenly stuck to traditions. I ask myself: Why do we let ourselves be manipulated into doing bad science?

For fisheries scientists providing scientific advice to support European fisheries management, this is not an atypical experience.

Table 5.10 Regression on attitude scales

I am not at all = 1 … 7 = very well satisfied with my job overall		B	Beta	t	p
(Constant)		4.75		8.35	0
Working and career conditions	How much does your job encourage you to or hinder you from producing the number of peer-reviewed publications you feel you would like to be producing?	0.25	0.33	6.36	0
	Did you find participation in this working/study group personally enjoyable?	0.28	0.24	4.49	0
	In the past three years how has the pressure you experience on the job changed?	0.3	0.2	4.1	0
Experience of results as unreal	Scale made from experience of being asked to 'answer impossible questions' and 'create certainty that is not really there'.	0.1	0.1	2.6	0

N = 270 AR^2 = .29 Exclusions: 148 respondents who indicated not having participated in an expert group in the last five years, and six who failed to answer whether they had been in an expert group or not. Sixteen respondents did not rate their level of enjoyment; seven did not respond to the publications question; eight did not respond to the pressure question; nine did not respond to one of the questions about impossible questions or certainty; and one did not rate his or her job satisfaction.

5.4 Conclusion

A good metaphor for what stock assessment scientists are experiencing is that they are being asked to mould factual bricks from uncertain clay, stamp 'science' on them, and then hand them to someone else to use to build a bridge. This runs directly counter to the culture that scientists have built over centuries to maintain the communicative rationality that keeps science working well. These are the things that I have variously described here in terms of openness to new claims, the fair evaluation of arguments based on rigorous and prescribed methodology, freedom from manipulation, a background assumption of trust for other scientists, and disinterestedness and scepticism in respect of the results. The anomie and resistance of the scientists working within the advisory system are a good illustration of the discussion in Section 2.1.1 about the reasons the bureaucratic system has for inflating the science boundary and the ways the scientists resist that inflation. Unless, of course, as in the precautionary approach, they have their own values which they would like to see made into technical necessities.

In this respect, the production of scientific advice for European fisheries management can be seen as a process of cultural alienation. It is ironically very similar to what many researchers looking at local ecological knowledge have said about what happens to the knowledge of fishers when it falls into the hands of managers and scientists (Agrawal 1995; Holm 2003). Like that of the fishers, the knowledge of scientists is disembedded from the knowledge culture that created it and is transformed by management into something alien.

The pressures to inflate the science boundary will always be a constant factor because they are not based on a conspiracy to cheat science (or fishers), but on the real practical problems that arise in the management bureaucracy because of the way their authority functions. Beyond the management bureaucracy is the wider scientific community and the conservation values that are enshrined there.

The inflation of the science boundary is hardly the only thing that makes management advice a less attractive task for European fisheries scientists. Participation in advice reduces levels of publication, and increases undesirable travel. Perhaps the most difficult aspect is the way that the very high uncertainty not only translates into frustration about the quality of the science, it also makes it very difficult to organise activities and know when a task has been adequately accomplished.

The system described in this chapter and the previous one is what is in place now. It is a system that examines species one by one and tries to tell us how many fish can be taken from the sea. So far it has not worked well for this task, but this task is a relatively simple one compared with considering fisheries in the context of the ecosystem as a whole. As will be discussed extensively in the coming chapters, mobilising the knowledge of these scientists becomes even more difficult in the face of the EAFM.

A maxim of politics tells us that one strategy for solving a problem is to redefine it as an even larger problem. Perhaps that strategy can work in European fisheries management. The EAFM will require ICES to call more and more strongly on the 'science side', i.e. those scientists who have been much less involved in the production of scientific advice. At the same time the conditions that fisheries scientist are facing have also created a great deal of energy for reform. The scientist in Q 5.25 is not alone in asking, 'Why do we let ourselves be manipulated into doing bad science?' In the precautionary approach and the EAFM, this frustration has found an ideology and an opportunity that provide a platform for working for change. The next chapter examines how ICES scientists are beginning to meet this challenge.

6. Science for the ecosystem approach to fisheries management

The scientific advisory system for European fisheries management is in a process of rapid change. The internal pressures described in the previous chapter mean that there is a strong desire for change within the scientific community. These demands take several forms. The fishing industry is looking for advice that extends beyond one year. The environmental community, along with many scientists and backed up by international agreements, is looking for an ecosystem approach to fisheries management. The fisheries managers in Brussels are looking for advice that they can apply to mixed fisheries. ICES is looking for ways to respond to all of these demands, and in doing so they are addressing both scientific and organisational issues. While these things cannot really be separated, this chapter emphasises the first set of issues, and the next chapter emphasises the second with a discussion of a recent reorganisation of the ICES Advisory Programme.

This chapter is in some ways the heart of the case study because it focuses directly on the problem of knowledge and the adaptation of the social system to its environment. The subject is the EAFM, but the chapter leads into the EAFM through a discussion of a similar problem at a lower level of complexity: multi-species, mixed-fisheries advice.

6.1 Multi-species, mixed-fishery management advice

DG MARE need scientific advice that they can use to manage the complex mix of fleets, fisheries and fish stocks that make up European fishing as it is actually practised. There is a strongly felt need for fisheries-based advice that can be used in situations where complexes of fisheries and fleets operate in one region and catch more than one species. The driving force behind the move to fisheries-based advice is again the division of fish. A politically viable way has to be found for dividing fish among different fleets. Some of these fleets capture different species that they specifically target; others catch fish as 'bycatch', meaning that they catch them while targeting other species. One difficult example arises when there is a need to reduce the bycatch of fish from stocks that are believed to be in serious trouble, such as North Sea cod, by fleets that are targeting healthy fisheries in the same area. Different fleets with different gears fish for different healthy stocks while having different bycatch rates for the cod. If the managers can find a 'scientific' way to divide the allowable bycatch for cod, then this solves the very serious political problem of how you treat all these different fleets, which often belong to different member states.

The single-species approach is a component of the overall TAC Machine, and so it is very resistant to change (Holm and Nielsen 2004) because it is directly linked to other parts of this self-reinforcing system. Multi-species, fisheries-based advice presents problems for fisheries scientists who are trained to deal with units such as fish stocks that are defined by nature rather than units like fisheries, where natural, technical and social phenomena are mixed together (Wilson and Delaney 2005).

I offer as an illustration of these difficulties the story mentioned in Chapters 3 and 4 about the development of mixed fishery advice for the North Sea. The story begins with observing an attempt to develop a multi-fleet, multi-fishery model for 83 North Sea fleets at an ICES Working Group on the North Sea and Skagerrak (WGNSSK) in October 2003. The purpose of the model was to calculate the individual impact of these many different fleets on the North Sea cod stock. The group was working on this task in response to an urgent request from DG MARE where tremendous political heat was being felt around the issue of allowing several cod stocks to recover that were in perilous condition.

In developing the model, the scientists immediately encountered a daunting dual problem: defining the different fleets and getting comparable data for them. The problem with fleet definition is that individual fishing vessels have their own patterns of fishing in terms of location, species and gear, and these patterns change during the year. So any definition of 'fleets' is artificial, to some degree. Vessels can and do move between the various fleets, no matter how the fleets are defined. This problem is reduced by the fact that the management system has become so reliant on these artificial definitions over the years that they have been reified into actual fishing behaviour through licensing and other regulations. A critical downside of this reification is that it has become more and more difficult for fishers to adapt their fishing activities in response to changes in the resource.

Getting reliable and comparable data on fish discarded at sea was a particular problem for these scientists. In fact, they ended up with 83 fleets because those were the fleets they had comparable data for, and those data were only complete for one year. The next time this model was used, the number of fleets was no doubt different. These data problems were significant because tests showed that small changes in how fleets were defined had noticeable impacts on the results.

These scientists saw themselves as experimenting; they knew that the data problems meant that official decisions could not be based on this work. The WGNSSK scientists believed that these data problems precluded the use of the model in decision-making, and the scientists were concerned that this initial work would be misinterpreted as more solid than it actually was.

Because of the need to rely on the essentially social concepts of fishery and fleet (Wilson and Delaney 2005), the mixed-fishery approach is one place where there is considerable pressure on the science boundary. During the meeting the form that the report of the WGNSSK was going to take became controversial:

> Q 6.1 Scientist One: We are trying to do a mixed-fishery forecast. You just suggested we put forward a scenario, while I thought this was just sensitivity analysis. If you suggest options, then one may be taken up, but this sensitivity analysis shows that this model is very sensitive to how it is set up. Scientist Two: But that is a political decision. If we don't think we can explain this we should not put it forward. Scientist One: After this discussion with [another scientist] it sounds like we can't really explain this model ... I have not been in the sub-group, if they can explain it then I have no problem. Scientist Three: If they can explain this for lay people, who will read this? We are not writing for managers, we are writing for ACFM, and they can decide if they want to go forward. Scientist One: I want clear text from the sub-group that they are ready to take this forward. We should not have scenarios in there that may be picked out. Scientist Three: I just want this in the working group report, not in the ACFM report. *(Observer's notes at the Working Group on the Assessment of Demersal Stocks in the North Sea and Skagerrak meeting, September 2003)*

The results were discussed at the ACFM, however, because of the urgent requirements of DG MARE. ACFM took the challenge of producing multispecies advice with evident seriousness in their October 2003 meeting, continually coming back to discuss the implications of the new emphasis. A multi-species section was created in the report, and the advice for cod, among other key species, was reported in that section, rather than in the section describing the individual stocks. ACFM recognised that the data were weak. Still as they had to respond, they concerned themselves with less refined alternatives that depended less on precise data:

> Q 6.2 Session Leader: The problem is the linkage of stock and fisheries advice, and that is a problem. We should not say 'closure of all fisheries' but 'a zero catch of cod', then we raise the question of closing the fisheries. But we have to keep the fisheries and the stock separate things ... [further discussion]. Scientist One: We don't want to take away the strong message [about the cod, but] we are giving unclear advice that says you can have fishing and not, we cannot escape criticism. Session Leader: This is moving in the right direction, we must anticipate that criticism with some text. *(Observer's notes at the Advisory Committee for Fisheries Management meeting, October 2003)*

The scientists wanted to stick to dealing with the cod stock. They could not offer scientific results with respect to fisheries because the data they had were not strong enough to provide 'analytic forecasts' of the impact of all these fleets on the cod stock. At the same time they knew that the stock was in very bad shape, and they wanted their advice to give a clear message: the cod stock should not be fished. They also did not want to deal, in a scientific sense, with the sticky issue of how to implement this imperative among all the different fleets, many of which were, after all, fishing healthy stocks and only catching cod as bycatch. They saw their role here as 'raising the question' of closing the fisheries. The official outcome read as follows:

> Q 6.3 It is not currently possible to provide analytical forecasts for input into mixed-fishery evaluation models. The main obstacle is that ICES does not have access to discard data for most fisheries. Development of such capability furthermore requires better catch monitoring, fishery analyses, and management decisions. The lack of such mixed-fishery forecasts necessitates the development of complementary processes that do not require analytical short-term forecasts. ICES has in this report taken a first step towards the formulation of advice in a mixed-fisheries context ... ICES acknowledges that defining relevant allocation scenarios places difficult demands on managers and that mixed fishery advice in particular will require interactive communication between scientists and managers. DG Fish has indicated to ICES some scenarios that would be of interest for managers. However, mainly because discard data for most fleets are not available, ICES is unable to provide the required scenarios at this time (ICES 2003, pp. 5-6).

What ACFM was doing here was redefining the problem of how to move from single-species-based advice to multi-species, fishery-based advice in two directions. These directions were not mutually exclusive, but they did have very different emphases. The first, in keeping with the tradition of the TAC Machine, was to define the problem as a lack of data, with the solution then becoming an even more intensive and extensive data-gathering apparatus. On the other hand, they were calling for a move towards processes that 'do not require analytical short-term forecasts' and that rely on more 'interactive communication' between managers and scientists about what the alternatives really are. This can be read at least in part as a desire to adjust the science boundary and move back out of science some of the social and political questions that the development of complex models for dividing mixed fisheries among multiple fleets was trying to solve. This suggestion provides a lesson. They are suggesting increased interaction with *non-scientists* as a way to *clarify* and *reinforce* the science boundary.

The ACFM agreed with the WGNSSK that the data were insufficient to forecast the impact of various combinations of fleet effort. They did indicate that if reliable landings and discards data were available by fleet, they could ‘present forecasts based on major groupings of fleet/fisheries, and evaluate the impacts on cod and other rebuilding species of various distributions of effort among fleets’ (ICES 2003, p. 222).

This response was not good enough. It stalled the machine, something that had real consequences for a lot of people. DG MARE had a very difficult time moving the decision process forward. A Commission scientist described the issue in a public lecture:

> Q 6.4 ICES just said do not fish cod. There should be a recovery plan for plaice without cod bycatch or discards. For cod there should be a zero catch until the SSB is within safe biological limits. So this means no cod catch and would require closing the demersal fisheries. So basically the advice is no fishing [for all demersal fisheries in the North Sea]. They gave us some defined biological limits but have refused to give forecasts for cod, haddock and some others. This is the advice I have received. What will you do, I ask you? No fishing is not an option. Scientists don’t tell you the elements of a recovery plan, they don’t guide you. This is why this is such interesting work, it is really, really interesting work to do. (*Audience lecture notes, January 2004*)

The answer to the question ‘What will you do, I ask you?’ was that in November just after the ACFM meeting, the STECF met and was asked by the Commission to evaluate the ICES advice, among other things with respect to the possibility of mixed-fishery forecasts. Calculation of these mixed-fishery forecasts, using a model called MTAC, required as a ‘major input’ (CEC 2003b, p. 2) the analytical forecasts of impact on the cod that the WGNSSK and ACFM had specifically declined to make due to lack of adequate data. The STECF recognised the validity of ACFM’s reasoning but also believed that ‘despite its numerous limitations, it would be more appropriate to provide advice based on evidence for the mixed-species nature of the different fisheries than advice that completely ignores the effects of technical interactions on the implementation success of TAC-based management’ (STECF 2003, p. 56).

The rationale for their decision was the distinction between using MTAC to provide the basis for mixed-fishery advice and using it as the basis of mixed-species advice. ACFM was particularly concerned that the MTAC model should not be used as the basis of advice because 1) data on discards were insufficient (the most prominent of several reasons for their refusal to make the short-term analytical forecasts) and 2) the definitions of ‘fisheries’ were too coarse. STECF agreed that these would be fatal flaws for the *mixed-fishery* advice, but they did not believe that this would be so for the *multi-species* advice. The practical implication of this was that STECF saw themselves as able to address one of the central multi-species management problems: that of fisheries where technical interactions caused fishers to fish on more than one TAC-controlled stock while harvesting their quotas at different rates; but not the other key problem, which was allocation of the harvest of such protected species among different fisheries (CEC 2003b).

The STECF ran the multi-species analysis that ACFM had declined to do. In their report they offered a set of eight scenarios which illustrated the different political options, mentioned in Q 6.1, that were built into the model. Moving a step beyond the use of models to produce numbers demanded by political needs, the scientists were now self-consciously building political options into their models. This self-consciousness is important. In other ways, such as in the use of precautionary judgements in assessments, fisheries scientists have been building political options into models without referring to it as a political decision (Wilson and Degnbol 2002).

At the same time there was clearly no consensus on what constituted reliable and useful data. This back and forth between ICES (ACFM/WGNSSK) and the Commission (STECF) was a debate over the science boundary. ICES was unwilling to stamp any results from the MTAC models as science valid for advice, while STECF was willing to do so with respect to fish species, but not to fisheries, a social unit for which data are much harder to define and collect. This meant that none of the scientists were willing to provide a quantitative underpinning for the allocation of stocks among fisheries. The unwillingness in this case was justified by specific technical problems, not by the principled stance that allocation decisions are political and not scientific decisions as fisheries scientists have argued under other circumstances (Wilson and Degnbol 2002).

As complex and difficult as they are, these issues around moving from single-species, stock-based advice to multi-species, mixed-fisheries advice are merely a shadow of the problems we face in generating scientific advice for the Ecosystem Approach to Fisheries Management (EAFM). Most realistic approaches to the EAFM retain these same questions of managing the impacts on many fish stocks of many different fleets, while also incorporating an expanded agenda involving fisheries impacts on the marine environment and the need for fisheries to share the marine environment with many other users. The responses and positions that have emerged with respect to multi-species, mixed-fisheries advice foreshadow the strategic alternatives we face. On the one hand, we can emphasise a conceptually simple and practically challenging strategy of merely expanding the approach from management based on single-species, stock-assessment models to management based on ecosystem models. This would mean a commensurate expansion of the monitoring, data-gathering and data-management systems. On the other hand, we can emphasise a more interactive approach that to some degree substitutes negotiations about how to handle uncertainty among stakeholders for the search for the perfect model. This process could be simplified, for example, by using spatial tools.

6.2 The ecosystem approach to fisheries management

That fisheries management should move in the direction of an 'ecosystem approach to fisheries', to use the FAO's term (Garcia et al. 2003), is a broad but shallow consensus among fisheries scientists. The consensus is broad in that nearly everyone agrees that it is a good idea, and that fisheries cannot be considered apart from their interactions with the broader marine ecosystem. The consensus is shallow in that there are many different ideas about how this approach would work in practice. The term 'ecosystem approach' is often used as a way to justify the inclusion within fisheries management policy of a sub-set of important related issues, e.g. the impact of fishing on the sea bottom, bycatch of marine mammals and birds, that need to be addressed but are outside the traditional fish stock focus of management. In the last few years it has also become associated with the movement towards greater use of marine protected areas. An earlier phrase, 'ecosystem management', has largely been abandoned because of the twin realisations that we do not govern nature, we govern people, and that ecosystems are much too large and complex realities to try to manage in any direct sense.

6.2.1 The need[11]

The need for a broad approach to fisheries management is driven by a complex set of related issues. Some are very specific and immediate concerns with environmental damage from fishing. Others are longer-term and broader problems involving the overall role of fishing in the marine ecosystem and the human food system. Fishing activities have impacts on fish stocks and on the broader marine environment. Other natural and human-induced processes in the marine environment have impacts on fishing.

The ecological importance of traditional management

The discussion of fisheries in the context of the broader environment begins with two salient facts of human ecology. The first is that fisheries and aquaculture are the single largest class of human impacts on the marine environment. These impacts are continually increasing as fishers pursue new species, penetrate new fishing areas and develop new gears in search of a way out of the 'commons' dilemma in which they find themselves. They are competing with one another for a continuously declining resource in order to pay off the already far too large investments they have had to make to remain competitive. This pressure is the classical concern of fisheries management, but it is also this pressure that has resulted in the most profound impacts of fishing on the marine environment, and this is true beyond simply the condition of the target stocks.

The second salient fact is that capture fisheries are one of the most environmentally benign sources of animal protein in the food supply system with great potential for shrinking our overall environmental footprint. The human ecological importance of fish begins with the fact that they have the highest protein content in their flesh of all food animals and that no other food animal converts feed to body tissue as efficiently as fish (Smil 2000). Capture fisheries in particular make a critically important contribution because they do not generate the waste and disease problems found in both terrestrial and aquatic animal husbandry. If effective ways out of the dilemma described in the last paragraph were implemented, the result would be a slowing of the rate of fishing to allow stocks to recover and produce new fish at a much higher rate than they do in their currently depleted state. This could be done with a far lower fishing effort, energy use and other environmental impacts that such effort entails. This would mean a potentially large increase in the production of capture fisheries and a substantial lowering of the price of fish products, a situation which would hold great potential for relieving overall human pressure on the environment.

What these two human ecological facts make clear is that the EAFM begins with the traditional concern of fisheries management, i.e. balancing fishing pressure and the natural production of fish biomass to gain the largest long-term yield of protein from the sea. Most of the problems discussed below would be vastly improved simply by getting an adequate handle on controlling fishing pressure. Understanding fisheries in its ecological context means understanding that capture fisheries potentially provide a critical substitute for both terrestrial and aquatic animal husbandry practices that are less sustainable in the long run.

Incorporating ecosystem considerations in fisheries stock assessments

An accurate assessment of the condition of a fish stock relies on knowing the impacts of other environmental factors beyond fishing. Predator-prey interactions are an important part of this, leading to the importance of multi-species assessments discussed in the last section, but there are many other considerations. Changes in climate, salinity and temperature have an impact on stock recruitment relationships, species composition, natural mortalities and growth (ICES 2006b). Life histories may shift, and this might change the availability of food (ICES 2006b). These impacts run all the way from the local impacts of pollution on a spawning ground to the possibility of ecological 'regime shifts'. For example, the North Sea has undergone such major changes in its overall ecology in both 1983 and 1997 (ICES 2006a). In the North Sea these changes seemed to be linked to huge climatic rhythms like the North Atlantic Oscillation (ICES 2006b), but they are not entirely natural. Research on the Scotian Shelf in Canada has documented a regime shift that seems to have been driven in large part by the removal of a huge number of fish (Choi et al. 2004).

Environmental impacts from fishing

Fisheries generate a number of impacts on marine habitats. The most direct physical impact is that of fishing gear on the sea floor. The ICES Working Group on Ecosystem Effects of Fishing Activities (WGECO) (ICES 2000) has summarised studies of the impact of trawl nets on the sea bottom. They conclude that the evidence shows that bottom trawls can remove some physical features, and reduce the complexity of the physical and biological structures that are critical for maintaining both biodiversity in general and fish yields in particular. The other main impact from fisheries on the marine habitat is through pollution. While capture fisheries generate some pollution in the form of engine leakage and offal discards, the most critical pollution is generated by aquaculture. Pollution also has a serious impact on fisheries. The majority of pollution is from land-based sources, and as most fish spend part of their life near shore, there is a severe risk of exposure (Parrett 2001).

Fisheries threaten biodiversity in a number of ways. Depleting a stock may reduce its ecological niche, making recovery more difficult or even impossible (ICES 2006b). Biological extinction from directed fishing pressure alone is rare, if not non-existent, but these heavily depleted species may easily become bycatch in fisheries directed at other species. This is because fish populations become commercially extinct, meaning that the declining population becomes unprofitable to catch well before it reaches the level where biological extinction is threatened. Pressure from overfishing increases the vulnerability of a population to extinction from other sources, such as climate change or habitat destruction. Bycatch, the capture or entanglement of untargeted species in fishing gear is a problem in fisheries the world over. While research has focussed more on problems with marine mammals than it has on seabirds and benthic organisms (Payne 2001), all three areas are a problem. Bycatch of depleted stocks that are no longer targeted can contribute to extinction dangers, especially for long-lived, late-maturing species (Musick 1999). The once abundant common skate in the North Sea, for example, has been extirpated in some parts of its range, mainly as a result of both commercial and recreational fishing pressure and bycatch (Hilton-Taylor 2000). The barndoor skate in North America has suffered a similar fate entirely as a result of bycatch (Casey and Myers 1998).

WGECO also emphasises that fishing changes trophic relationships through the relative abundances of predators, prey and competitors, and creates changes in the genetic make-up of populations (ICES 2006c).

6.2.2 The ecosystem approach and governance

From the side of nature, the idea of an ecosystem approach emerges from ecosystems being inescapably interconnected, open systems with ambiguous boundaries and highly complex linkages. This makes a strong argument for considering the entire system before making decisions that affect its parts. This reality takes on a different aspect when looking from the viewpoint of society – i.e. when the question of governance[12] for an ecosystem approach is raised. This interconnectedness, openness and ambiguousness have made the idea quite attractive to all the various groups that have sought to turn its practical operations in specific and diverse directions while agreeing to the vague generalities (McCay and Wilson 1997). The 'ecosystem approach to environmental management' has become a unifying phrase for many different kinds of efforts to improve governance.

The idea of an 'ecosystem' is a fundamentally ambiguous concept (O'Neill et al. 1986). When introduced to policy-making, it becomes an 'essentially contested' (Gallie 1955) concept, meaning that its definition always depends on the speaker's interest in how it is defined. In Europe, in particular, there is no baseline concept of what an ecosystem is. All ecosystems, including marine ecosystems, have been 'disturbed' by centuries of human use, and all ecosystems are subject to both shorter-term natural variability and long-term evolution.

Most practical experience with 'ecosystem management' has been on land. The idea became very prominent in the late 1980s and 1990s in managing terrestrial environments in the western part of the United States. At least 18 agencies of the United States federal government, as well as many local governments, examined the implications of ecosystem management for their operations (Haeuber 1996; Yaffee 1996). Throughout this history there has been an ongoing struggle between 'biocentrism' – nature should be 'managed' for itself – and 'anthropocentrism' – nature should be managed for the sustainable production of goods (McCay and Wilson 1997). Scientists advocating managing ecosystems for production are found mainly in natural resource management agencies, while independent and academic natural scientists support the idea of 'managing nature' for its own sake (Grumbine 1994). Grumbine (1994) reviewed 34 articles on ecosystem management and found biocentrism to be widespread among academic scientists. The emphasis was much more on ecological integrity than on sustainable use. The main goals being articulated were maintaining viable populations of all native species, representing all native ecosystems, conserving evolutionary potential, and basing geographical scales on the needs of the largest carnivores. Human use, on the other hand, was to be allowed only when these other goals were met.

This same division is found in fisheries, and these two basic viewpoints explain a lot about disagreements in marine management. Many people, principally found in the commercial fishing industry and government agencies, seem to see the sea as a 'farm' and fisheries management as exclusively about maximising long-term food production. Many others seem to see the sea as a ‘park’ and management about protecting it for its own sake. In this view fishing can be allowed if it has no impact on any other marine creatures. Some even refer to the sea as the ‘last wilderness’ (e.g. Dinwiddie and Thomas 2006) as if it were some final bulwark against the planetary encroachment of humans. Responsible stakeholders do not hold to these viewpoints in any absolute way, but they are clearly present. Fisheries scientists tend towards the ‘park’ pole (Wilson et al. 2002); after all, many of them came into the business through a love of nature. Our data on fisheries scientists’ attitudes indicate the same differences between agency and non-agency scientists found by Grumbine (1994) with respect to terrestrial ecosystems. This can be seen, for example, in their attitudes towards the precautionary approach (Table 5.9).

An ecosystem approach has been called for in a range of documents from international conferences and codes relating to general environmental issues, natural resource management and specifically fisheries. The basic idea has had a long history in land and forest management agencies, going back at least to the 1930s (Haueber 1996). Turrell (2004) documents the development of the concept of an EAFM in three strands of UN agreements: the UN Convention on the Law of the Sea; the UN Conferences on Environment and Development, i.e., the Stockholm Declaration, the Rio Declaration, the Johannesburg Declaration, etc.; and the UN Food and Agriculture Organization (FAO). Each of the areas has seen a proliferation of documents, agreements and ideas about what an ‘ecosystem approach’ might be. For the marine environment, early articulations of related concepts from as early as the 1950s emphasised the interrelatedness of the ‘ocean space’ (Turrell 2004, p. 7). In the 1970s the ocean space idea began to be linked to an ecosystem approach to ‘sustainable development’. The 1972 Stockholm Declaration called for more rational management of the seas through an integrated and coordinated approach based in ‘rational planning’. The late 1970s and 1980s saw the articulation of the value of ‘biodiversity’ which was linked to the ‘precautionary approach’ (see Section 5.2) by Agenda 21 and the UN Convention on Biological Diversity processes in 1992. The actual term ‘ecosystem approach’ in the form ‘ecosystem process-oriented approach’ (Turell 2004, p. 16) first appeared in 1995 in support and advisory meetings to the UN Convention on Biological Diversity process. It was specifically linked to the concept of ‘ecosystem functions’ and the development and use of ‘models of ecosystem processes’ (Turrell 2004, p. 16).

The FAO process has been the most prominent one in fisheries, and their 2003 technical paper on the subject (Garcia et al. 2003) has become an important reference document. From an institutional perspective they emphasise the need for strong legislation with effective implementation and enforcement. They also focus on the need for coordinated decision-making among agencies.

Q 6.5 Intersectoral planning and coordination must be improved, on an ecosystem basis, particularly when resources are shared (e.g. space shared among aquaculture, transportation and fisheries) or nuisances are transboundary. This requires developing collaboration between institutions in charge of the different economic sectors as well as of research, environment, etc. Such collaboration will not be very effective without explicit allocation of natural resources and space and improved coherence between sectoral legislative frameworks. A requirement in this respect is to improve coordination between regional fishery and environmental commissions (Garcia et al. 2003, p. 37).

Under the subject of governance, they offer the following list of needed components:

Q 6.6 Decentralisation of decision-making and management responsibility to lower-than-central national level (e.g. to coastal communities), building the necessary capacity at that level.

- Higher participation of stakeholders in decision-making through opening of institutions, broader public debates, development of representation of the sector, etc. Various forms of partnership management (co-management, community-based management, etc) are available.
- Improved transparency, diffusing more information and instating oversight mechanisms.
- Establishment (or confirmation) of user rights, communal or individual, free or against payment, exchangeable or not, depending on the circumstances.
- Bilateral and international agreements are required to optimise management of shared and straddling fish stocks. This implies agreeing on resource allocation and, in the ambit of regional fisheries commissions, with the issue of new entrants and illegal fishing. Agreements may also be needed to deal with transboundary pollution.
- Cooperation between regional fishery bodies and environmental commissions ... need to be significantly upgraded in many areas.
- Oversight mechanisms by institutions and individuals independent from sectoral interested pressures would help build objectivity and public confidence in fisheries governance (2003, p. 39).

A tension can be seen in these FAO reflections on what is needed for EAFM governance that I think is a central institutional issue within the EAFM idea. It is also reflected in tensions over how science in support of an EAFM should be done. On the one hand, there is the need for strong legislation and a comprehensive, inter-agency, decision-making process that is able to make practical decisions related to the whole ecosystem. On the other hand, there is just as clear a need for more cooperation from more groups in society operating on multiple scales. This need is reflected in Garcia et al.'s (2003) call for decentralised decision-making, greater participation, and transparency. The need for more participation has a good empirical basis. Steven Yaffee and his students at the University of Michigan identified 662 sites of ecosystem management and did case studies of 77 (Yaffee 1996). Ecosystem management in practice turned out to be much more than management coordination and inter-agency collaboration; everyone interviewed talked about more effective public involvement. Both viewpoints are right. We do need increased interagency coordination and more decentralised and participatory decision-making across multiple scales. This is a major governance challenge with strong implications for science.

The EAFM increases the number of groups that would have a stake in any given decision. Consider how the simpler problem of predator-prey interactions in fisheries has made policy alignments more complex by involving more players. Two examples from the 1990s come to mind. One involves the struggles around menhaden management in the United States where the issue was the degree to which fish targeted by an industrial fishery were needed as fodder by popular recreational species. The other is conflicts around the protection of sand eel that are a critical prey for kittiwakes in the North Sea. In both cases, management based on predator-prey relationships was successfully implemented after considerable effort – and this was against fisheries operating at a long distance that had little or no local political clout. Given the increased policy complexity that developed around these relatively simple inter-specific interactions, it is clear that the social complexity implied by an EAFM creates a major challenge for any current management institution.

The term 'ecosystem approach' is routinely used by stakeholder groups precisely to legitimate their having an influence on fisheries decisions. In recent European discussions around marine ecosystems, the term 'ecosystem approach' is often used as a general way of describing responses to the problems that have traditionally been left out of fisheries management, particularly ones by environmental groups involved with birds and marine mammals. The term is often used simply to mean taking into account impacts on habitat and other species than commercial fish. 'We should be taking an ecosystem approach' means 'we should be thinking about this issue that my group is concerned about'.

The problem raised by the intersection of increasing both the number of uncertain scientific claims that must be defended and the number of stakeholders scrutinising them has also been missed by some of those responsible for designing management systems. The European Commission's Biodiversity Action Plan for Fisheries suggests that while management of biodiversity, which is how the document conceives the ecosystem approach, will initially bring higher catches, the consideration of the state of other species or habitats 'will lead to less exploitation, less fishing opportunities and lower employment in the fishing sector' (CEC 2001b, p. 6). In a continent-wide policy arena, where even mild reforms of the Common Fisheries Policy are politically difficult, to suggest that an ecosystem approach, with all its complexities and uncertainties, will be the basis of reduced fisheries employment on the general behalf of other species and habitats indicates that the Commission has not thought through the social and policy realities. Nor, apparently, has it considered the far greater ecological importance of increasing the amount of protein derived from capture fisheries through traditional fisheries management goals as discussed above.

Nor do ecosystem approaches reflect the realities of bureaucratic environmental management. Bureaucracies depend on calculable rules to trigger responses (Porter 1995), while ecosystem approaches present complex interactions of parameters that are difficult to quantify and impossible to interpret in real, decision-making time. More fundamentally, the concepts needed to make ecosystem management work do not translate into firm, legal definitions, not even ones as simple as boundaries (Haueber 1996). For management bureaucracies, changes in policies involve high transaction costs, so they seek decisions that are as permanent as possible. Ecosystem approaches depend on the flexibility to make changes in response to shifting system parameters (Wilson and Dickie 1995). Moreover, bureaucratic personnel decisions depend on the clear identification of arenas of responsibility and mechanisms of accountability, whereas ecosystem approaches see 'responsibility' for outcomes in the interaction of many arenas (McCay and Wilson 1997). Bureaucracies are not well designed to address ecosystems, and many of the institutional design characteristics that make an ecosystem approach difficult are basic requirements of coordination of large-scale policies in a democracy.

One attempt to reduce the governance problems created by the EAFM is the promotion of marine protected areas (MPAs). In fact, MPAs are a good example of where a particular management agenda is closely linked by many people to the ecosystem approach. MPAs are areas where fishing and other human activities are restricted or prohibited, and they range from highly protected nature reserves to large multi-use areas with modest limitations on specific types of activities. As a fisheries management tool MPAs have gained in popularity over the last couple of decades. MPAs are expected to reduce fishing on spawning stocks and recruits, to increase fish abundance within the protected area and to promote spillover of the increased fish abundance into neighbouring areas where it may lead to improved catches. By reducing fishing effort, MPAs can contribute to ecosystem conservation and enhance or preserve local biodiversity. Their introduction is therefore often supported by conservation organisations (Halpern and Warner 2003). Once they are established, MPAs typically require much less biological information than other management tools, and they may therefore be a more cost-effective way to conserve fish stocks than either TACs or effort control.

Despite these advantages, MPAs have been met with criticism both within and outside the discipline of ecology. Their protection is limited to relatively stationary species, and they do little to protect migratory species.

MPAs also sometimes simply force fishers to relocate and concentrate their fishing effort in adjacent areas. Expectations for MPAs are often high because of their popularity, and experience with MPAs shows that few have fulfilled expectations. In a global assessment Kelleher et al. (1995) classified only 31% of the MPAs surveyed as achieving their management objectives. Various reasons have been attributed to the lack of success with MPAs. They may be too small, poorly designed, or lack economic input or stakeholder participation in their establishment (Degnbol et al. 2006). The various forms of MPAs established in Europe have done little to settle arguments about their effectiveness because of serious design flaws. They have not been based on an experimental design with measurable objectives; indeed, there has been little agreement on identifying what one might measure to evaluate them. In many instances the established regulations have been haphazard, with little or no monitoring of their implementation (PROTECT Project 2006).

Degnbol et al. (2006) argue that MPAs are particularly popular among biologists and environmentalists because the idea is fundamentally rooted in the idea of the fishery as essentially a biological and ecological system that must have its complexity recognised and all its parts protected. It has therefore become a 'technological fix' for the fisheries management problem for many biologists and environmentalists. While small MPAs are arguably not meaningful for the management of many non-sedentary fish species, large MPAs are fixes that have some severe weaknesses from the point of view of both the economics and politics of management. They are economically inefficient because they impose a direct constraint on fishers' decisions, and they are politically difficult because they tend to have strongly disproportionate impacts on different fishing communities (Wilson 2000). One important exception to this is the temporary closure of spawning areas during spawning periods. This is a measure that is actually very popular with the fishing industry when it is linked to 'real time management', meaning that the spawning times and areas are identified by fishers who then put the closure into place for a limited period of time. One of the great fears that the industry has about MPAs is that they will always be permanent. As one industry representative put it at a stakeholder meeting:

> Q 6.7 MPAs should have measurable targets. Fishers' experience is that closures are never raised again. As a fisheries management tool we are very hesitant, we are against the idea of using MPAs as a tool for reducing fishing mortality, existing boxes are much more political than biological. And boxes are never lifted, boxes are like coffins. *(Observer's notes at the North Sea RAC Spatial Planning Working Group meeting, May 2005)*

Within MPAs the basic tension between multi-scale decentralisation and inter-agency coordination reappears in a reduced form. The permanent imposition of large MPAs with strong and inflexible restrictions on fishing and other uses would certainly reduce the governance problem while ensuring the continued health of the marine environment. One might say it would deal with the EAFM governance problem by sweeping it off the sea. The political feasibility of doing this across any really large area is at best questionable. Any attempt to make MPAs less permanent or give them a flexible rule-making structure reintroduces the decision-making problem at the new level of the MPA. Various agencies and stakeholders would have to be involved, and the science would have to be developed. This might well be a very good thing; it could allow a fresh start at a new decision-making level with new, ecosystem-oriented objectives and rules of participation. It would help the problem, but not solve it.

Table 6.1 Responses to attitude scales about the ecosystem approach by type of last expert group

1 = strongly disagree, 7 = strongly agree		Type of last expert group None	Non-stock assessment	Stock assessment	Total	P
It is critical that fisheries management retain species-by-species management as its basic approach.	Mean N	3.72 146	3.44 166	3.71 121	3.61 433	0.31
The requirements of implementing an ecosystem approach to fisheries should be a central principle when reorganising national and international fisheries management agencies.	Mean N	5.69 144	5.46 166	4.93 123	5.39 433	0
It is difficult to see how any management bureaucracy could effectively implement an ecosystem approach to fisheries management on any marine area large enough to be shared between two or more nations.	Mean N	3.14 144	3.02 165	3.63 121	3.25 430	0.01

Excluded in all questions are 20 who did indicate participating in an expert group but who did not make clear which kind of expert group, and six who failed to answer whether they had been in an expert group or not. Furthermore, six did not answer the question on species-by-species management; six did not answer the question on the ecosystem approach as a central principle; and nine did not answer the question on effective implementation of the ecosystem approach.

The results of our survey of northern European fisheries scientists confirm these bureaucratic difficulties with EAFM. We asked three questions in the form of seven-point 'strongly disagree to strongly agree' scales related to different aspects of the implementation of the ecosystem approach. The same result was found in similar research in the United States (Wilson et al. 2002). Average scores on all three of these questions were in the direction of greater support for the ecosystem approach (Table 6.1). When the scores were compared by the type of last expert group attended, no difference was found for the basic idea of moving away from species-by-species management. More complex statements, however, showed systematic differences. On both a question dealing with the ecosystem approach and bureaucratic reorganisation and a question dealing with using the ecosystem approach on large geographical scales, scientists whose last expert group had been an assessment expert group were less supportive than the other scientists (Table 6.1), while still on the ecosystem approach side of the centre of the scale (4).

Table 6.2 Responses to attitude scales about the EAFM by type of employer

1 = strongly disagree, 7 = strongly agree		Type of employer							
		NFIs	Academia	NGO	EU	Other private	Other gov.	Total	P
It is critical that fisheries management retain species-by-species management as its basic approach.	Mean	3.6	3.72	2.30**	4.27**	3.65	3.06	3.64	0
	N	219	103	10	33	54	17	436	
The requirements of implementing an ecosystem approach to fisheries should be a central principle when reorganising national and international fisheries management agencies.	Mean	5.18***	5.73***	6.50**	4.94*	5.4	6.06*	5.39	0
	N	219	103	10	32	55	17	436	
It is difficult to see how any management bureaucracy could effectively implement an ecosystem approach to fisheries management on any marine area large enough to be shared between two or more nations.	Mean	3.33	3.38	2.00**	3.84*	3.16	2.88	3.31	0.7
	N	215	104	10	32	55	17	433	

** indicates significance at 0.1, ** indicates significance at 0.05, *** indicates significance at 0.01. Asterisk indicating significance refers to category compared to all other categories combined. Excluded in all questions are 22 who did not identify their employer. Furthermore, seven did not answer the question on species-by-species management; seven did not answer the question on the ecosystem approach as a central principle; and ten did not answer the question on effective implementation of the ecosystem approach.*

When the same questions were broken down by type of employer, systematic differences were found for the first two, while the third question revealed differences between two categories and all the other categories combined (Table 6.2). For all three questions, scientists working for conservation NGOs were the most supportive of the ecosystem approach, while scientists working for the European Commission were the least supportive. For the basic question about moving away from species-by-species management, the Commission scientists were the only group among any examined whose average scores were beyond the centre of the scale, away from support for the ecosystem approach.

Table 6.1 and Table 6.2 tell almost exactly the same story that was found in a similar survey of fisheries scientists in the United States (Wilson et al. 2002). The closer a scientist is to the day-to-day implementation of fisheries management, the more cautious he or she is about the ecosystem approach. Everyone supports it, but those tasked most directly with management decision-making seem to have a greater appreciation of the practical and political problems associated with its implementation. Table 6.2 also reminds us that species-by-species management, while hard to justify on scientific grounds, is used to solve some very real political problems in the ways that fisheries are organised and managed.

The bottom line from these scientific, social and bureaucratic considerations is that ecosystems are not a unit of management that fits easily with democratic, science-driven management as it is carried out on large scales through government bureaucracies. Rather the approach that these studies seem to call for is a more Habermasian one (see Chapter 1) that relies on communicative rationality to ensure timely responses to a widely defined set of problems. This would mean resisting the temptation to define the EAFM as an essentially technical challenge where the ecosystem is seen as a big machine that needs to be properly maintained, and so the first thing we need to do is to diagnose its problems.

The alternative is to view the EAFM as a social dilemma as well. The social dilemma is finding ways for people who see problems in the ecosystem to initiate a decision-making process to effectively respond to those problems. The question then becomes how to ensure that all concerns have access to the management process. From this perspective the management system responds to individual issues as they arise and does not try to figure out how to deal with everything at the same time. The 'polycentric' or dispersed networks of groups concerned in various ways with the ecosystem discussed in Section 3.2 have an important role to play here. This way of thinking about the EAFM requires trusting that there are enough people concerned about the marine environment that all the problems will be identified. Yaffee's (1996) analysis of a large sample of attempts at an ecosystem approach found that the successful ecosystem-based approaches were not the highly technical ones that began by creating complex ecosystem models, followed by goal determination exercises and then adaptive management. Rather, tentative and experimental approaches undertaken by groups of people faced with immediate problems showed the strongest results.

A central institutional issue here might be called 'burden of proof', although it is unfortunate that this phrase has its source in law rather than science, because what I am talking about here is a more scientific than legal idea of 'proof'. If the EAFM is to be rooted in ensuring that people have the ability to effectively raise concerns about the environmental impacts of certain activities, then there has to be a level playing field. It cannot be the case that those taking a conservative position in relation to sustainable use are always reacting to existing uses. A participatory approach to the EAFM requires balance, and such balance is achieved when the onus is on the users to demonstrate that their activities are sustainable. This cannot be taken too far, of course, especially in a legal sense. We cannot require that a permit be obtained for every new fishing net. Placing the burden of proof on economic interests has also led to real scientific difficulties, as in the case of drug trails (Guston 2001b), where insisting that the industry pay for the science leaves it under the heavy influence of those who gain from particular outcomes.

This idea of the EAFM as both a technical and social dilemma is reflected in the programmatic tension identified earlier between the EAFM as interagency coordination and the EAFM as increased stakeholder participation. These approaches to the EAFM are complementary in the sense that the social approach relieves the technical one of always having to have the big picture ready, while the technical approach provides the social approach with a filter to sift signal from noise. This tension is also reflected in science in the form of comprehensive and particularistic approaches to generating ecosystem science for advice discussed below. Results-based EAFM is an institutional framework that considers both dilemmas that I return to in Chapter 8.

In the midst of these complex governance challenges, the question of how best to produce scientific advice for an EAFM has fallen most heavily on ICES. How ICES has responded to this challenge is the subject of the next section.

6.3 ICES: The challenges of scientific advice for the EAFM

6.3.1 Introduction

Science for the EAFM is extremely challenging and faces a host of unknowns. A marine ecologist provided me with the following list of priority areas of research:

> Q 6.8 The ecology and life history of most fish species in European waters is only partially known, and large gaps in knowledge exist. This knowledge

> is primarily restricted to some of the commercially most important species, and even here serious gaps exist. For example, modern tagging methods are only now beginning to quantify how factors such as size, gender, prey abundance and environmental conditions affect the spatial distribution of a well-studied species such as cod. Moreover, many fish species are moving to new geographic areas as climate change progresses; ecology and life history of species in these new situations are therefore unknown and will be insufficient for management applications as new fisheries on these species develop in future. Similarly, ecosystems and food webs undergo important changes in structure and function over time (e.g. due to fishing, climate variability, etc.); even if particular species remain in present locations as climate change progresses, new knowledge of how the ecology and life history of those species are affected and will respond to different ecosystem and food web configurations is needed. Studies which improve knowledge of reproduction, migration, growth and survival of various life history stages, and of how species interact in time and space are needed to improve biological, ecosystem and fishery management models (Brian MacKenzie, personal communication).

The ICES leadership sees the EAFM as requiring a good deal more than just providing information from a broader range of marine science disciplines. The major conceptual challenge is how to integrate these disciplines into a research programme that will become a source of comprehensive, coherent and consistent advice. In fact, the Advisory Programme has for many years included the Advisory Committee on Ecosystems (ACE) and the Advisory Committee on the Marine Environment (ACME) in addition to the Advisory Committee on Fisheries Management (ACFM), even if in the past the first two committees had not had to field nearly as many requests for advice as ACFM did. These three committees are now combined into the Advisory Committee (ACOM). A scientist who is a leader in the Advisory Programme:

> Q 6.9 Scientist One: The ecosystem approach means an adaptive approach, with social balance, focuses on the process. Integrating assessment groups means using long-term perspectives, emphasis on topics will vary over time. There are GLOBEC groups working, SGSE groups working on sea bird ecology, and cooperation with the REGNS process. Is there a problem of coordinating this with the advice process? We are being asked for long-term status approaches such as harvest control rules, recovery plans, and ecosystem health indicator frameworks and to contribute to the implementation of an ecosystem approach on an appropriate geographical scale. *(Observer's notes at the Management Committee for the Advisory Programme meeting, September 2004)*

Several things are important to note in this quote. The first is that the EAFM is coming to ICES at a time when they are also trying to deal with several other more or less related problems such as long-term fisheries management strategies and harvest control rules. Both of them are related to the demand for multi-species, fleet-based advice discussed in Section 6.1. The second thing worth noting is the way this scientist defined the ecosystem approach in very broad terms as an adaptive process with a social balance. ICES as an organisation is determined to remain a purely marine science group. They have strongly resisted frequent suggestions, in my opinion for very good reasons, that they should take economics and other fisheries social science on board as part of their advice. Many people within ICES, however, recognise the importance of understanding the development and use of scientific advice as a social process. This is evidenced, for example, by the creation of the Working Group on Fisheries Systems to focus on just these issues. While as scientists their first response is to see the EAFM as a primarily technical dilemma, many members are also open to understanding how it acts as a social dilemma as well.

Therefore, it is not surprising that the ICES leadership sees a series of issues around their institutional organisation and culture as central to meeting the conceptual challenges of the EAFM. Even the single-species models used in fisheries management, while grounded in good science (Rosenberg et al. 1993), are caught in a squeeze between demands for more precision and the costs of delivering that precision (Degnbol 2003). These problems increase literally exponentially when species interactions are considered, as became evident in the discussion above of the multi-species, fisheries-based approach. This has resulted in a situation where models purporting to be useful for ecosystem-level decision-making are being subjected to scientific standards that are much lower than those for single species (ICES 2000).

ICES is trying to deal with a tension that is similar to the one discussed in the last section between the technical definition of the EAFM that emphasises comprehensive, inter-agency, decision-making processes and the social definition that emphasises transparency and participation. One would think that scientists always jump to a technical understanding of the EAFM. And when dealing with it in the abstract, that is what they usually do by defining the problem as a need for 'integrated assessments' of ecosystems or something very similar (Google Scholar yielded 1420 hits for the exact phrase 'integrated assessments' in documents with 'ecosystem'). Many scientists are searching for models that can be used in the ecosystem approach in a similar way to the stock assessment models currently being used for single-species management. A number of the activities around developing the ecosystem approach, e.g. the Regional Ecosystem Study Group for the North Sea (REGNS) within ICES that is trying to build a set of ecosystem models of the North Sea, have as an eventual ideal the idea of managing with an integrated assessment.

> Q 6.10 Scientist One: None of us can produce integrated advice, we don't know how. It has to start from the bottom and move up. Scientist Two: We

can do this within the existing system. **Scientist Three: I would like to see one group giving integrated advice before I change the whole organisation. Observer from DG MARE: A structure to integrate advice? What are the questions we are posing? What do you think we clients are asking you to do? The Commission is not as interested in integration as they are in specifics**. We want long-term management, effort management, and impact assessment for stocks, the environment, the economy and society. You should keep looking at what your clients want to see. *(Observer's notes at the combined meeting of the Advisory Committee for Fisheries Management, the Advisory Committee for Ecosystems and the Resource Management Committee, September 2006)*

The reaction of the observer from DG MARE is very important because it shows how the tension between a comprehensive versus particularistic approach to the EAFM exists among ICES clients as well as within ICES itself. This person is not concerned with integrated advice, let alone overall ecosystem modelling. He wants specific answers to specific questions and doubts the utility of integration in achieving this. This attitude is not shared by everyone at DG MARE (see Q 6.46) and is likely to be much less prevalent among other clients such as DG ENV, HELCOM and OSPAR, but DG MARE is currently the single most influential client. DG MARE focuses on fisheries and sees the EAFM as mainly about improving fisheries management. HELCOM and OSPAR are concerned with the general marine environment and are much more interested in developing comprehensive, inter-agency, decision-making processes. Integration efforts like REGNS, including attempts at developing ecosystem-level models, will continue to be an important part of ICES's response to the EAFM regardless of the clients' desires. This is because they a) represent the cutting edge of knowledge with a scientific appeal and b) adhere closely to the scientists' ideal of the kind of information that the EAFM should be based on. They understand the difficulty of modelling an entire ecosystem; realistically, they are hoping to be able to derive a set of general indicators of ecosystem health that could be used as the basis of management.

The remainder of the chapter focuses on how ICES took on developing a system to produce scientific advice for an EAFM. The main focus is on the fisheries aspects of this, i.e. how ecosystem considerations would be incorporated in fish stock assessments and then into fisheries management advice. This is because this was ICES's main focus in the meetings I observed, but developing advice based on fisheries impacts on the environment would have many similar social aspects. I begin by examining how the tension between the particularistic and comprehensive approaches to the EAFM led to a two-pronged development strategy. Then I turn to the relationship between methodology and the two basic scientific cultures within the ICES community. Finally, I examine the issues around the mobilisation of knowledge and quality control.

6.3.2 EAFM advice and the two-pronged strategy

Within ICES, the mainstream view of the role of science in policy, introduced in Chapter 3, is the assumed starting point. The challenge becomes fitting the realities of the EAFM into that model of the role of science. For ICES, harvest control rules and their associated precautionary trigger points form the framework for how objective scientific advice is translated into a policy decision. The report of the Working Group on Ecosystem Effects of Fishing Activities (WGECO) describes this ideal:

> Q 6.11 This framework is embedded in a social and policy governance system. The governance process has a vital role in augmenting the science-based framework with targets and societal values that influence decision-making. However, the intent of these science-based precautionary frameworks is to ensure that scientific knowledge is captured clearly in objective, rule-based advice to management. In the end, society may choose not to comply with the science-based advice, but well-tested empirical rules at least make the science input to the decision-making process explicit and objective (ICES 2007b, p. 7).

In the standard application a stock assessment model estimates one parameter about the condition of the stock – the spawning stock biomass or SSB – and one parameter about the fishing pressure to which it is being subjected – fishing mortality or F – and these two estimates with associated measures of uncertainty are plugged into an 'if/then' harvest control rule. The form of this rule might be, for example, 'if SSB falls below X, then F must be reduced to Y'. The automatic nature of the harvest control rule creates a feeling of objectivity because the 'science', i.e. the SSB parameter, is formally separated from the 'management', i.e. the new target for F. This 'social and policy governance' narrative hides any lack of objectivity – including most strikingly in the WGECO quote (Q 6.11), the application of the highly political precautionary approach – firmly within a black box on the science side, while leaving society with the political choice 'not to comply'.

In the search for an equivalent framework, the basic logic is the same. F, current SSB and future SSB are examples of pressure, state and response indictors that form the basis of most approaches to understanding ecosystems. The problem is the far greater complexity of the ecosystem, which makes the separation of politics and science even more difficult. The fisheries problem is an industry-driven problem with fairly clear agreement in principle on the objective: catching as many fish as possible in the long term. This problem drives the selection of indicators, making the science 'simple' in that sense. Finding equivalents for an EAFM depends on agreeing on what are often contested objectives and developing useful indicators. Hiding political choices on the 'science side' becomes that much more obvious and difficult.

In trying to figure out how to provide advice for an EAFM, ICES self-consciously adopted a two-pronged strategy with an 'integrated' prong – that holds onto system-level modelling as an ideal – and an 'event' prong that links fisheries management advice to specific events in the ecosystem that have implications for fisheries advice. As the leaders of the Advisory Programme put it in a document addressed to clients:

> Q 6.12 ICES has been working on the development of an ecosystem approach to fisheries management through two routes:
> Incremental additions of ecosystem aspects to the fisheries advice in ACFM and advice on ecosystem health and specific issues through ACE;
> An overall 'system health' approach, integration of multiple sources of information using the North Sea as a case study through the REGNS process (MCAP-MICC 10-11 April 2006 Doc 9).

One prong, the second in Q 6.12, was given an early form by the REGNS group, where an integrated assessment of the North Sea ecosystem was attempted. It was based on the eventual goal of producing models that could be plugged into the ideal governance framework in the same way that fisheries stock assessment models are used in the single-stock situation. Because of the complexity of this goal, the intent of the REGNS group was very exploratory in the ways it was linked to possible approaches to advice given. The REGNS group is no longer functioning. The other prong is given form by WGRED, the Working Group for Regional Ecosystem Descriptions, which tries to carefully develop instances of ecosystem-approach advice based on the link between environmental events and specific stocks to help the stock assessment better reflect environmental conditions. In other words, it describes specific ways that changes in the ecosystem have an impact on fisheries. In this approach the role of the stock-assessment parameter would be filled by the observation of environmental events that had implications for fish stock assessments. All that is required is that this link be understood and specified, a challenge that requires a tremendous amount of research based on the accurate anticipation of what events are likely to be important.

One helpful way to talk about the mid-term goal is being able to provide *integrated* rather than *concatenated* advice. Concatenated advice is advice based on a series of discrete descriptions of nature that are of sufficient quality in and of themselves, but the relationships among these parts are not addressed. Integrated advice is based on some understanding of these interrelationships. At first, such advice would not need to be based on a fully integrated ecosystem assessment, but rather on particular links, such as the link between a type of event and the condition of a particular fish stock. Building the ability to provide specific forms of integrated advice would also be a path towards a capacity for integrated assessments.

Prong 1: Moving towards integrated assessments

REGNS was part of ICES's initial attempt to develop models and data for integrated assessments for the EAFM. The North Sea ecosystem was chosen because a great deal is known about its fisheries, and the physical processes have also been relatively well studied (ICES 2006a). In presenting REGNS at the ICES Annual Science Conference in 2004, the organisers emphasised that REGNS was finding ways to complement current ICES activities. The immediate goal was to enable 'partial integrated assessments' that were able to look at single changes in pressures on the ecosystem and the corresponding state changes. A central focus of the REGNS effort was to identify the required data and where it was available, starting with a list of 70 parameters that they saw as important for describing the ecosystem structure and function of the North Sea (Kenny 2004). Even the humble goal of partially integrated assessments was to be achieved within a broad and expensive ecosystem context.

Reactions to the REGNS presentation immediately took a practical turn:

> Q 6.13 **Scientist One: [After expressing appreciation] How can you argue that this is complete?** Scientist Two: … It was fairly easy to start with an ideal wish list and that is where the 70 parameters came from. Scientist Three: Lots of redundancy and they use MDS [multidimensional scaling, a statistical technique that summarises correlations among many variables] to deal with it. Scientist Four: What about geological processes? **Scientist One: You are tempted to completeness … you end up with a huge list that no one will pay for; the pragmatic way is to look at what data is already there, the third way is the Wadden Sea example, which is to choose these things according to issues of concern**. Scientist two: We will eventually come to identifying gaps, we started from theory. **Scientist Five: We are trapped in the past. We are trying to look at an ecosystem for management and have the data that was generated in respect to other interests. So a gap analysis is important. We are also dealing with an ICES WP structure not organised around delivering holistic information.** *(Observer's notes at the informal presentation of the REGNS process, ICES Annual Science Conference, September 2004)*

A tension emerged almost immediately between scientists concerned with how this huge number of parameters could be handled practically and the concern of Scientist Five that there were still gaps in what was being examined because the data had not been generated by looking at the ecosystem for management purposes. This vision of not being 'trapped in the past' includes a 'holistic' picture of the ecosystem that will require a restructuring of ICES's expert groups. It is interesting that one of Scientist One's alternatives, the 'Wadden Sea approach', began by seeing the problem as a social rather than a technical dilemma. The conversation soon turned to the political dimensions of the problem:

> Q 6.14 Scientist One: ICES has mechanisms for getting the Delegates to support this, and that is a clear set of terms of references. **A Delegate**: Don't shoot the Delegates. Countries' money has not gone in the ICES direction in the last 10 years. **We need to approach politicians to convince them that money for ecosystem work needs to be made available**. Scientist Two: We could write a proposal if we know what we are doing and take it to OSPAR. Scientist One: On that topic, this is related to the Marine Strategy that DG Environment and DG Fisheries intend to bring forward. This could have important funding opportunities, but we need to use compatible language. Scientist Three: ... This will definitely not be a marine equivalent of the Water Directive which was very expensive and did little. The Commission will not buy another of those. **Scientist Two: This is the crux, the funding comes from national governments, but the customers we have, e.g. OSPAR, want this done but don't have the money.** *(Observer's notes at the informal presentation of the REGNS process, ICES Annual Science Conference, September 2004)*

The EU and the member state governments are formally committed to an EAFM but are still going to be very careful about what they fund. The scientists in OSPAR and elsewhere that do support developing integrated assessments do not control the needed funds. The problem is how to convince the politicians. The problem also has national and regional dimensions where some countries and areas are more concerned about the EAFM than others, as is evidenced by the following quote from a WGRED report:

> Q 6.15 No countries around the Baltic Sea sent participants to the working group meeting. The working group updated some time series data in cases where 2006 values could be located. Otherwise, however, the OVERVIEW HAS NOT BEEN UPDATED AND WILL NEED ATTENTION FROM ICES (ICES 2007a, p. 1, emphasis in original).

Caught between the ideal and the practical, REGNS spent two years trying to figure out what an integrated assessment should be. In the end, they dealt with the problem, as is so often done, by broadening it so far that they could not be accused of leaving anything out:

> Q 6.16 There was confusion about the meaning and nature of Integrated Assessment which took time to resolve in REGNS – in fact, the first two meetings (2 years) were spent resolving this issue. For example we concluded that Integrated Assessment should be considered at three levels, namely: (i) policy drivers[13]; (ii) scientific understanding of ecosystem processes; and (iii) the provision of advice and the development of management tools. REGNS recognised that most of what is being called Integrated Assessment can be assigned to at least one of these three areas (ICES 2006a, p. 97).

Integrated assessments, following REGNS, cover policy, management and everything in between. It was a way to finesse a contentious issue. The objectives of integrated assessments are also quite different among the potential user groups. The REGNS group suggested that there were three groups that might want to use the products – policymakers, research scientists, and environmental managers – and that these groups had such different needs that sub-groups would need to be formed:

> Q 6.17 If the Integrated Assessment is aiming to deliver a report for policymakers, research scientists and environmental managers, then it is necessary to establish three subgroups to deal with their specific needs. REGNS made the mistake of trying to do it all at the same time with representatives from each area – the result was difficult to chair as each end user group tended to pull the work programme to meet their own needs. The policymakers tend to be most interested in indicators of marine ecosystem health, etc. and smiley faces. They want integrated outputs to show how effective or otherwise their marine stewardship policies are. Likewise, research scientists have specific needs to do with testing significance and understanding cause/effect relationships, whereas the environmental managers wish to maximise the effectiveness of their monitoring programmes (ICES 2006a, p. 99).

It is striking that the REGNS report assumed that integrated assessments would contribute solely to top-down management processes. This is particularly striking because of the amount of effort that the Advisory Programme has placed in communicating ICES science to user groups and other stakeholders over the past five years. However, REGNS was a study group taken mainly from the science side of ICES, and this is a good example of the cultural differences between the two discussed below. The second interesting assumption is that the managers and policymakers want to use the integrated assessments for monitoring and reporting on ecological conditions, while the use of the assessments in management decision-making is not mentioned. Even for this limited group the management implications of their conclusions are unclear:

> Q 6.18 The assessment has provided some valuable insights into the significance of the relationships between different pressure and state changes at different scales and the time scales over which changes take place. For example, plankton community data in relation to the physical and chemical oceanography reveals both gradients of response to the major riverine inputs of nutrients into the North Sea and sources of nutrients from the Atlantic. In addition, an assessment of all variables reveals two relatively stable states in the North Sea, one pre-1983 and the other post-1997. The intervening years are dominated by high ecosystem variability which represents a transition from one state to another and in part explains the number of studies which highlight different years for regime shifts. We conclude

> that defining such shifts is sensitive to the number and type of variables included in such an analysis (ICES 2006a, p. 1).

They did catalogue pressure-state relationships, such as nutrient inputs, that could usefully inform the management of some stocks. This usefulness would come into play if changes in these pressures were observed and sufficient research had been done before this observation to make a meaningful statement about the magnitude of the state change. This suggests a major challenge in the anticipation of important events and the prioritisation of research. This in itself seems a reasonable justification of efforts like REGNS. At the integrated system level they were able to say that the 'big picture' matters, in the sense that stable general states or 'regimes' could be observed, but with little clarity about what causes shifts between the states, or even when they happen. Integrated assessments for management do indeed seem a long way off.

Prong 2: The impact of ecosystem events on fisheries

The following exchange took place at a meeting of the Management Committee on the Advisory Process (MCAP), the ICES committee charged with overseeing the advisory process.

> Q 6.19 Scientist One: We are being asked for long-term status approaches such as harvest control rules, recovery plans, and ecosystem health indicator frameworks that will contribute to the implementation of an ecosystem approach on an appropriate geographical scale. Scientist Two: The first task for area-based groups would be to compile existing information on linkages and impacts and not pretend to try to integrate on a system level; to start in a humble way. *(Observer's notes at the Management Committee for the Advisory Programme meeting, September 2004)*

If humility means that trying to 'integrate on a system level' is not going to be practically feasible for a very long time, then how can the link be made between science and advice? Even if an equivalent to a stock assessment model could be constructed, the number of parameters and the extreme uncertainty about their measurement would make their translation into a useful 'harvest control rule or other rule about some type of fishing behaviour more difficult'. A more immediately practical device for translating an ecosystem assessment into objective advice for fisheries management is the 'occasional environmental anomaly', or more simply 'event'. The following quote is from the Working Group for Regional Ecosystem Description (WGRED), which is charged with aiding the transition of established stock-assessment expert groups towards producing advice for the EAFM.

> Q 6.20 If risk of very poor (or good) productivity does increase markedly under certain environmental and/or biological conditions, these conditions could be effective trigger points for conditional control rules. Such rules would be more effective in ensuring that ICES advice is robust to the occasional environmental anomaly; circumstances where advice based on assuming 'typical' conditions could go badly wrong, with severe consequences for the resource (ICES 2007a, p. 145).

The objective observation of events can be tied to the science side of an 'if/then rule' as long as a logical argument is developed for the link between the event and the response. This last requirement makes the focus on events a tentative and exploratory move:

> Q 6.21 **Scientist One**: Nobody is thinking that an integrated assessment in REGNS and integrated advice are the same. We don't know what their relationship is. We can't know that until we see an integrated assessment. **But what we want in the integrated advice are discussions of significant events each year.** We get information on big signals, and we give it a paragraph in a three-page summary of the ecosystem. We had better know what we want to say about it, and we are doing nothing to prepare ourselves to know. **One example was the absence of capelin in the area around Iceland. That is as significant even in that part of the world, and we did not know what to do.** Scientist Two: Who knew about it, just fisheries people or the whole community? **Scientist Three: We did not have any quantitative way of linking it to advice, we know the direction, but we don't know what else to say about it.** Scientist One: If we all support integrated advice, we need to think about what was done and who should have done it so when we get that signal we know what to do. This is what we need to think about. What do we do when we see that the food supply has failed? It is fine the first time it happens to not know what to do, but then we need to ask about the relationship between the food supply and fisheries advice as a part of integrated advice ... **Scientist Four**: There are two issues here. One is to create a methodological approach and have that ready for the advisory groups. **When the capelin disappear, it is too late to start the scientific process, we have to have a methodology ready. We need a procedure within the advisory system**. *(Observer's notes at the Consultative Committee meeting, September 2004)*

In this case the event – the absence of capelin, an important prey species – and its relevance are clear in a qualitative sense, but the tools for translating the event into advice are not. What is called for is an adaptive approach in which events are anticipated (not predicted!) and methodologies developed in advance for preparing advice when they happen. Such methodologies could be aided by research into an integrated understanding of the ecosystem level, but are themselves a much more humble endeavour. These discussions around the table made their way eventually into the report of WGRED along with suggestions for how the relevant expert groups could incorporate the events in their stock assessments:

> Q 6.22 The three events which were identified were:
> The continued low abundance and restricted distribution of capelin around Iceland. The consequences of low food availability should be taken into account in weights-at-age and other estimates of productivity used in short-term and medium-term projections.
> The very warm conditions in Barents Sea in 2005, combined with the high abundance of young herring, suggest that the predator-prey relationships currently included in the analytical assessment models for cod and capelin should be scrutinised carefully, and 2005 data examined for evidence of anomalous predation rates.
> The very low abundance of Norway pout and low abundance of sand eel in many parts of the North Sea suggest that the prey base for higher predators might be anomalously limited in 2005 and 2006. The most recent possible data should be used for weights-at-age in the projections, and survey data should be examined carefully for anomalous distributions.
> In addition, it was noted that the NAO has changed from strongly positive to near neutral in 2005. It is too soon to know if this change has important ecosystem consequences, and it is considered premature to speculate that another regime shift has occurred in the Iberian Sea (ICES 2006b, p. 1).

The identification of these events set the stage for the next step of trying to link the events to the stock assessments. This meant mobilising a very extensive research agenda that is still ongoing. It requires anticipating the kinds of events that may have an impact, and then understanding the relationship between the events and the fishery well enough to translate them into advice. It requires developing an anticipatory and adaptive knowledge system.

6.3.3 Scientific methods and cultures in ICES

The two-prong approach helps, but hardly resolves the disputes around how to approach the EAFM. The following quote illustrates a number of other issues.

> Q 6.23 **Scientist One**: We should not ask a group of people to compile [everything] we know and then we use that. We have to know what we want **and I don't have a clue what to write in the ecosystem section of an ACFM report.** Have we decided to write something we can document? **The client will not be happy with some general statements.** I don't know how to move forward from here. ICES needs to look at what will fill our needs. **Scientist Two**: The idea is not to make filler text. The job done by ACE [the Advisory Committee on Ecosystems] is what we need to do. They have information

about cycles in horse mackerel and the impacts on North East Arctic cod. Icelandic capelin was another example with the collapse of the food source in the system. **The beginning is to take whatever is known … Scientist Three**: I have two main concerns. **We want to model these things but we don't know the mechanisms**, without that we risk doing things without knowing why. **We are putting a lot of energy into moving from the single-species assessments while we sit and blame this approach for not working, but it has never been implemented in a political sense**, and then we go off and move in a scientific direction we do not know anything about. Scientist Four: We can put in some time series of the status of the ecosystem, temperatures, plankton production and standardise these things. Scientist Five: ACE … has identified some steps ICES can take to get over this hurdle. **Scientist Six: I don't speak against the ecosystem approach, but I am concerned with the impact on the ability of ICES to provide high-quality fisheries advice. We are overloaded just with fisheries advice, and now we are adding this ecosystem approach**, but the Commission still needs high-quality fisheries advice. *(Observer's notes at the Advisory Committee for Fisheries Management meeting, September 2004)*

The discussion collates a complex mixture of questions such as: modelling versus qualitative descriptions; what constitutes adequate advice; feasibility from a work-load perspective; and basic doubts about whether or not changing the scope of the analysis to include the ecosystem will increase the effectiveness of scientific advice within policy settings. Different scientists place more or less faith in the possibility and practical efficacy of finding a quantitative link to fisheries advice based on either ecosystem-level modelling or on links between specific ecological events and fishing behaviour.

Q 6.24 Scientist One: If the REGNS approach succeeds, why have we invested so much in this if it is not to provide integrated advice? Scientist Two: It will only be one part of integrated advice. These are two lines feeding into the shared product. Scientist One: I belabour this to make sure they do converge. Scientist Two: The WGRED is not integrated assessments, they are doing literature reviews. Scientist One: This word game will not sell to Delegates. *(Observer's notes at the Consultative Committee meeting, September 2004)*

Perspectives on methodological questions are related to the structural and finally cultural division within ICES between the Advisory Programme and the Science Programme. To some degree these two groups resemble proto-epistemic communities in that they show similar ways of approaching questions, but they do not have the sort of scientific consensus that characterises a full-blown epistemic community. The degree of cultural cohesion – and hence the appropriateness of the epistemic community designation – is certainly much higher in the Advisory Programme, with its more frequent and more intensely focussed events, than in the larger and much more diffuse Science Programme. There are also some disciplinary differences here as the most intense activities of the Advisory Programme are related to fish stock assessment carried out by quantitative fisheries scientists, while the Science Programme involves marine scientists of many disciplines. These differences are changing in response to the scientific demands of the EAFM, and this is an important driver of the cultural shifts needed in ICES.

The Consultative Committee (ConC) is the committee with primary responsibility for the ICES Science Programme. It is the structural equivalent of what MCAP and now ACOM are for the Advisory Programme. Scientist One in Q 6.24 is a leader among scientists involved in the Science Programme who are frustrated with what they perceive as an inability of getting more influence from 'science' into 'advice'. He is also someone with a great deal of previous experience on the advice side, and it is interesting to observe the degree to which his concerns about quality control led him to present himself, at least in these meetings in September 2004, as speaking for the science side of ICES. Like many other stakeholder groups, scientists see the EAFM as a tool for addressing their general concerns about management. This perspective on EAFM is both stronger and less self-conscious on the science side than on the advice side. In the following quote, this same Scientist One debates with his colleagues working on the advisory side.

> Q 6.25 Scientist One: The science community ought to want to move faster than the client commissions want to move in ecosystem approach, if we are doing our job right we are hitting a balance. They would like ICES to move at a glacial speed with ecosystem, and the science community doesn't think we are doing anything more than changing the package. Are we taking the whole of ICES's scientific knowledge, this has to be done ... Scientist Two [a leader in the Advisory Programme]: I saw the opposite, scientists wanting to go slower; this auditing [of the ICES strategic plan] is about auditing the science rather than the advice process. Clients expect more done by 2006 than we are moving towards. Scientist Three [also a leader in the Advisory Programme]: I agree with Scientist Two's assessment, internally in ICES we need to accelerate the scientists' support for this. What is not filled in yet is the science side of the advisory process. Scientist Four [also a leader in the Advisory Programme]: You can go to the action plan, we can't. Scientist One: This is where I am uncomfortable. The strategic plan was built for all of ICES; the science part does not take its orders from the client commissions. The audit should be on whether or not the science part is being used in the best way to support the advisory process. Sitting in ACFM last spring about Icelandic cod, we are still moving at a glacial pace in ecosystem aspects for fisheries advice, we clearly have no interest in this even though the other parts of ICES have put out a lot of data that they think ACFM should be using. There is a disconnect, and we need to find it and fix it. Their

perception is that the ecological and physical are affecting advice, they see it as relevant. If we are promoting ourselves in an ecosystem framework, we need to either fix the information they are producing or fix the way the advisory process uses the information from the other parts of ICES. *(Observer's notes at the Management Committee for the Advisory Programme meeting, September 2004)*

One important distinction between Scientist One's attitude and that of his colleagues is a different understanding of what ICES's advisory role is all about. For Scientist One the problem is that not enough of the ICES science has been brought to bear on advice. Indeed, he sees the two sides of ICES as moving away from one another. This is another understanding of the science boundary that draws the line within ICES itself. Scientist One, 'speaking for' the science side, wants ICES to draw on its scientific expertise as a way to accelerate the EAFM, with ICES taking the lead rather than just responding to the client commissions. At some points in the meeting, this construction took on the colour that EAFM is a scientific imperative that real scientists have to get behind. Indeed, at this meeting and afterwards, those representing the advice side in this conversation expressed some irritation at this construction of advice and science. If indeed Scientist One is speaking for a substantial body within ICES, there are expert groups that want to have an impact on the 'nature and content' of the advice and have felt frustrated that this has not happened.

Table 6.3 Attitude scales related to the role of ICES

Type of working or study group last attended	Mean	N
ICES should become much more direct in advocating fisheries conservation		
Has not attended a working or study group in the last 5 years	5.14	144
Last group was not a stock assessment group	4.83	166
Last group was a stock assessment group	4.27	123
Total p = 00	4.77	433
ICES should focus more on pure science and less on producing scientific advice for managers		
Has not attended a working or study group in the last 5 years	3.56	147
Last group was not a stock assessment group	3.45	165
Last group was a stock assessment group	3.03	124
Total p = .05	3.37	436

Scales run from: strongly disagree = 1 to 7 = strongly agree. Excluded in both questions are 20 who did indicate participating in an expert group but did not make clear which kind of group and therefore did not fit in any of the three categories, and six who failed to answer whether they had been in an expert group or not. Furthermore, six did not answer the question on ICES advocating fisheries conservation, and three did not answer the question on ICES focussing more on science.

Table 6.3 provides some evidence that Scientist One was not speaking just for himself. The first question relates to the degree to which ICES should advocate for fisheries conservation. Regardless of expert group, average scores are fairly close to the centre of the scale, suggesting at most a slight preference towards ICES increasing its direct advocacy of conservation. But there are significant differences between the three groups. Those who are not active in expert groups are the strongest advocates of this position, while those involved directly in stock assessment are its weakest advocates. The middle group includes the active ICES scientists who are not involved as much in assessments, exactly the group that Scientist One meant by the 'science' part of ICES. The difference between the two groups suggests that perhaps there is a group of active ICES scientists who would like to have an impact on the 'nature and content' of the advice, moving it towards a stronger conservation posture. However, the same pattern is found with respect to the statement that 'ICES should focus more on pure science and less on producing scientific advice for managers'. All groups are neutral with a slight preference against more emphasis on pure science, but the stock assessment group is significantly less supportive than the others. This suggests a relative reluctance on the part of the non-assessment group to get ICES further involved in the advice aspects, but this difference is clearly not large. On the whole Table 6.3 suggests that while Scientist One was not speaking only for himself, the scientists are broadly satisfied with ICES's current balance between advice, advocacy, and pure science.

The Advisory Programme is organised around responding to requests from the client commissions. ICES has a quite specific memorandum of understanding with the clients, and they are the source of much of ICES's funding. The clients are interested in the EAFM, so ICES is concerned with the EAFM. Scientist Two is frustrated by trying to get more information for advice generated by the science side in response to client pressures, hence he sees the scientists as slow in responding to the EAFM. Scientist One is also very concerned about advice for the EAFM. He complains that 'we are still moving at a glacial pace in ecosystem aspects for fisheries advice'. Yet in nearly the same breath he says, 'the science part does not take its orders from the client commissions. The audit should be on whether or not the science part is being used in the best way to support advisory processes'. What he is pushing for is seeing ICES's scientific advice as not being primarily about responding to client commissions. That is a relatively minor bureaucratic issue related to how some of the work is funded. Instead, he wants ICES to understand itself to be a group that is publicly committed to the EAFM and to the precautionary approach, and that is duty-bound to make these things a reality in the most progressive way possible. With respect to the ecosystem approach (Table 5.9) and the precautionary principle (Table 6.1), our survey results suggest that he is not alone.

This difference in starting points reflects the kind of scientific changes described by the Mode Two science theory (described in Section 2.2.1) that places emphasis on what happens when science is driven by the needs of clients rather than the desires of scientists. Mode Two theory suggests that the area where these conflicts are acted out is scientific quality control. In ICES this takes the clearest form in constant discussions of 'consistency' (Section 7.3.1). This helps explain how the cultural and methodological differences between the advisory and science sides in ICES are linked. This is not a dispute over basic methodology, nor does it reflect highly polarised positions. But there are noticeable methodological differences in terms of what constitutes adequate science for clients in the short term and promoting the EAFM in the long term.

Scientist One believes that the production of quantitative ecosystem advice is necessary and is eventually going to be possible (as in the reference to the 'word game' that the same scientist made in Q 6.24). He is concerned that his colleagues working on the advisory side will become content with qualitative descriptions that are injected into the current approach to advice as a sort of EAFM gloss on business as usual. Scientist Two is a leader on the advisory side. The scientists working on the advisory side may or may not have a scientific scepticism with regard to integrated assessments in the long run, although as one of them put it:

> Q 6.26 Even in 20 years time when we have the whole integrated model in place, we are always going to get requests for advice on little things, we will always have incremental changes in advice. *(Observer's notes at the Consultative Committee meeting, September 2004)*

They are most concerned, however, with what can be done to meet their clients' current expectations about advice for the EAFM. They want to be ready with 'soft predictability' that describes scientifically plausible scenarios while steering away from precise numerical forecasts about future states of nature. This is expressed in the following quote from a document prepared for a meeting with clients by leaders of the Advisory Programme:

> Q 6.27 The further implementation of an ecosystem approach will require that ecologists and stock assessment scientists cooperate more closely, and it should be accepted that advice can be given even if classical predictive models cannot be applied. The discussion so far has revealed that an ecosystem approach will imply a change in the predictability from the form ACFM has produced it in the past to a softer predictability based on indicators. A management system that is based on an ecosystem approach is likely to be more adaptive, which leads to a new role for science to add to the learning mechanism in an adaptive management framework (MCAP-MICC 10-11 April 2006, Doc 9).

Conscious or not, there are noticeable links between the different methodological emphases, the role that ICES members play on either the science or advisory side, and the different ways they imagine the role of science within the larger marine environmental management system. Those with responsibilities for advice are more apt to consider issues from the perspective of seeing the social dilemmas underlying the EAFM. They are more focussed on particulars, more concerned with stakeholder participation, and more comfortable with creating 'serviceable truths' (Guston 2001b) for use in decision-making that may not contain the complete scientific picture. Those on the science side are more apt to think of the EAFM as a primarily technical problem, to follow more traditional standards for science and to be more optimistic about the value of integrated assessments. The two-pronged approach is a mechanism for keeping the whole ICES system on track towards the EAFM while these issues are worked out.

6.3.4 Cultural and organisational shifts

The ICES leadership sees the need for producing integrated advice as requiring much more than just a shift in subject matter and the development of methodologies. They also see it as requiring a shift in the ICES culture that goes well beyond different perceptions of scientific issues. An important part of this is the relationship between the Science Programme and the Advisory Programme.

The Advisory Programme has historically been heavily weighted towards the production of fish stock assessments. The annual round of assessment expert group and advisory committee meetings, often involving the same people year after year, has created its own culture. This culture is described in detail in Chapters 4 and especially 5. In brief, these scientists are sent by the member state governments that employ them to assess stocks that are important to their country. They meet in very intense working environments. In response to a set of 'Terms of Reference' (ToRs), they produce assessments of fish stocks that meet the requirements of the TAC Machine. These experiences often leave scientists frustrated that their real insights about the condition of fisheries are not being communicated. They do not have much time for figuring out how to make new scientific information fit into their ToRs (Q 6.28).

The Science Programme also has its own, less articulated culture rooted in the disciplinary interests of the attendees. As discussed in Chapter 4, the science expert groups are more attractive than the assessment groups, and there is a tendency for very senior scientists to attend these groups and not the assessment groups (Table 5.5).

Discussions among the ICES leadership also suggest that it can be challenging to convince the scientists attending expert groups under the Science Programme to respond to ToRs that do not fit into their particular scientific interests or even their understanding of what processes are important. Several times I have heard the ICES leadership expressing strong frustration over particular expert groups, and their chairs, that consistently ignored advice-related ToRs. Groups also exert leverage simply by deciding which of their several ToRs they will emphasise. While on the advice side scientists are sent by their governments to assess fish stocks of national economic interest, scientists attend the expert groups on the science side to share research. Such expert groups are more like small, highly focussed scientific conferences than the scientific assembly lines on the assessment side. As a leader in the Advisory Programme put it when addressing a meeting of chairpeople from the science side:

> Q 6.28 We have two cultures. The advisory culture is about meeting deadlines, not summarising new knowledge, and it is conservative in adopting new science. The science culture is less pressured and less conservative ... The bottom-up works well, the top-down works poorly, sometimes science does not listen, and the advisory committees want some science to be done, but these requests are lost, advice does not listen to new science. *(Observer's notes at the Workshop on Expert Group Performance, March 2006)*

As reflected in the previous section, the cultural differences do not reflect so much two different understandings of what it means to *do* science in the general sense as what it means to *practise* science day-by-day. This is an important distinction within the Post-Normal Science perspective discussed in Section 2.2.1. The Advisory Programme worries about time deadlines and delivering the information that clients want. The Science Programme worries about pushing the conceptual edge. It often also takes a harder line in matters of scientific quality. The chairperson of one study group, who was trying to make some multi-disciplinary progress that would be useful for stock assessments, told me the following story:

> Q 6.29 At the first meeting I had an idea that it would be a collection of biologists, oceanographers and assessors, and we would be very focussed on producing a model and methods that would enable working groups to do forecasts taking into account biological processes. My feeling about that was that they should be biotic, in other words ... say you have Barents Sea cod that are dependent on capelin, and you would be modelling cod on the basis of the capelin population, which is predictable to a certain extent, certainly for a couple of years because you have some stock structure rather than basing it on temperature, which is probably ... the key driving factor but you can't forecast it. There were all these biologists and oceanographers who had their pet data sets of sea surface temperatures in the North Sea or something that they felt had to be included, whether you can forecast with them or not. *(Interview with ICES Study Group Chairperson)*

The 'pet data sets' in this case reflected the scientists' interests. But they also reflected what even the person telling the story calls 'the key driving factor'. Indeed, the REGNS groups argued that the North Sea fisheries reflect abiotic changes more closely than anything else (ICES 2006a). The chairperson had a background within the ICES Advisory Programme. He understood why the study group needed to focus on the biotic elements. But this meant trying to get the scientists to ignore the 'key driving factor' because that factor was not useful for the forecasts needed. This proved a difficult task, especially as few scientists place very much faith in assessment forecasts in the first place. They both recognised the same 'key factor' from the perspective of the scientific description of how nature worked. They did not agree about the use of this key factor in practice.

The organisational and cultural problems that ICES is addressing in order to make the EAFM possible can be seen as falling into three categories that I discuss in turn: motivating scientists to bring ecosystem information into the advice and make use of it when it is available; setting up the needed communication systems to collate that information; and finding the required time and money.

Motivating EAFM contributions

One of the basic requirements of achieving the integration needed for the EAFM is a substantial input from the Science Programme to the Advisory Programme, and this requires a change in culture.

WGREG is one of the expert groups looking for this kind of input:

> Q 6.30 It is clear that if ICES is serious about placing its assessments and advice into an ecosystem context, that is not going to happen by having most of ICES continue business as usual while a few working groups somehow develop and apply new suites of much more complex – but scientifically equally sound – tools for ecosystem approaches to each step in developing advice. ICES – and the national laboratories – have to fully commit to this framework as the foundation for their science efforts, if the policy commitments that have been made to the ecosystem approach in the Bergen Declaration, the Common Fisheries Policy, the EU Marine Strategy, and many other policy documents including ICES own centennial Copenhagen Declaration are to be matched by practices in science and management (ICES 2007a, p. 143).

WGRED is making an appeal to the Science Programme expert groups on several levels here. First, they are making it clear that the development of science for the EAFM is not merely good science, it is the new and more complex – read exciting and publishable – science. They also appeal to a sense of duty towards progressive policies embedded in recent, and fragile, policy declarations.

Even when environmental information is made available, the advisory culture can block it from being incorporated into the assessments and related advice. This can happen because of issues of saliency, as in the example of water temperature being the key, but useless, driver in Q 6.29, or because of issues of credibility, especially when having to use categorical information that might be difficult to link to fisheries advice. The WGRED as part of its facilitation role did some direct documentary research on how and how often the various assessment expert groups were incorporating environmental information:

> Q 6.31 WG members reviewed the environmental content of a number of assessment working group reports … [the table finds a number of working groups that did not take advantage of available information]. From this review of content, it is clear that the concern is real. At the same time, the reservations of the working group chairs were heard clearly. It is far from clear how to use many types of environmental information, even if it is relevant to accounting for stock fluctuations (ICES 2006b, p. 117).

The problem may not be so much a lack of will as it is doubts about the credibility of the information and unclarity about how to use it as part of the stock assessment process. Part of the problem is that the environmental information is mainly qualitative, and the assessment groups must produce quantitative descriptions of stocks. This, by itself, is challenging, and the assessment groups are very hard pressed for time. WGRED, while understanding the perspective of the expert groups, is still frustrated:

> Q 6.32 The near total lack of uptake of environmental information in the computation aspects of population reconstructions/analysis is undesirable. However, there is nothing we can think of that would have a better chance of affecting the population analyses than the work of SGPRISM and SGGROMAT, and neither of those expert groups produced a detectable impact on practice (ICES 2006b, p. 117).

Setting up the communications systems

The second part of the problem is developing the communicative systems needed to mobilise and focus knowledge for the EAFM. When knowledge about ecosystem events relevant to advice exists at all in ICES, it exists in a diffuse form somewhere in the expert groups of the Science Programme. To be mobilised for advice, it must be found and focussed. The organisational strategy they employed was to set up what they called a 'clearing house' for identifying, locating and also directly facilitating the participation of the expert groups in the EAFM.

Q 6.33 When one group of experts observes a significant event, then ICES knows about it. I have never heard what the mechanism for such a clearing house is, but we have discussed it before. Another thing is tools and guidelines that, when the clearinghouse disseminates the information, the assessment groups treat it the same way, consistent, credible advice. *(Observer's notes at the Consultative Committee meeting, September 2004)*

REGNS took on this facilitator role early with respect to the North Sea.

Q 6.34 Scientist One: REGNS is developing quantitative skills, tools to serve the future. In 2004 they gave ToRs to a selective set of working groups, and we examined their responses in their reports. One or two ignored it, one or two asked why they did not get ToRs. Feedback came at the ASC [Annual Science Conference where group ToRs are determined], and more ToRs which are for more specific data sets at particular resolutions. They may not like these specifics, but that is fine as they need to be thinking about specific data. They [REGNS] have appointed five facilitators to work with the working groups. They will have a workshop in Copenhagen in May to pull the data together. They are going to invite experts from Canada who are looking at the Scotian Shelf. *(Observer's notes at the Consultative Committee meeting, September 2004)*

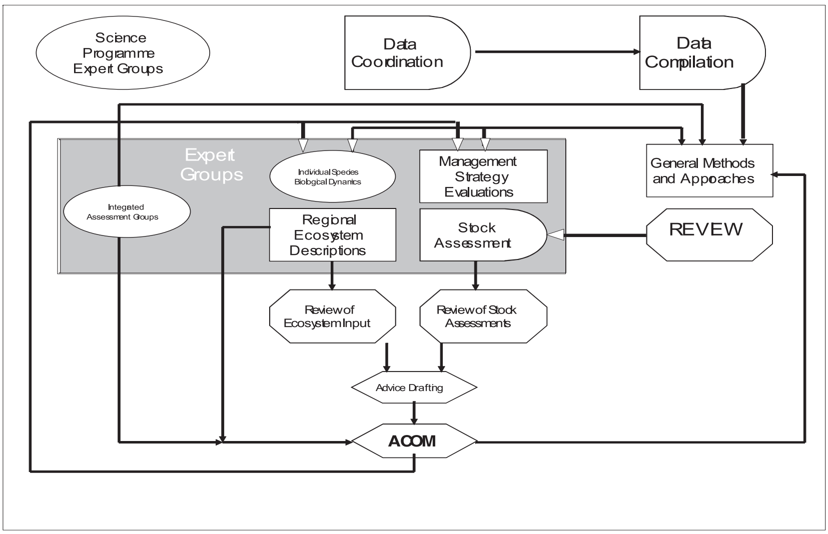

Figure 6.1 WGRED's diagram shows the different types of groups in ICES that will make it possible to develop integrated EAFM advice. The shapes reflect the nature of the various groups. Oval groups are science-oriented expert groups, rectangles with curves on the right are advisory working groups, rectangular groups straddle this boundary, octagonal shapes review, while hexagonal shapes are strategic, advice production and certification groups.

The REGNS group went to work creating ToRs for the information they needed for the North Sea integrated assessment and got mixed responses. Some groups were happy to respond, while others ignored the requests; still others wanted to be asked to participate. REGNS's response was to appoint facilitators who would be able to monitor the content and quality of the data being collated while coordinating the communications between REGNS and the other expert groups. REGNS became a 'clearing house' for mobilising information about the North Sea ecosystem. As REGNS was a temporary group with a specifically North Sea focus, WGRED, the group carrying out the linkages between ecosystem events and stock assessments, i.e. the second prong in ICES EAFM strategy, soon found itself taking up the clearing house role on a more regular basis.

Figure 6.1 is the latest iteration of WGRED's work on designing this clearing-house function. The diagram is the idea of one expert group charged with this work and is very much at a draft stage, but it does give a picture of what a functioning clearing-house dynamic might look like. The science groups are related to the advice groups through what amounts to a series of groups carrying out parallel clearing-house functions. The most important of them are the groups dealing with general methods and approaches; in ICES jargon they are referred to as benchmark groups because they review scientific approaches to developing advice on a periodic basis greater than a year. Benchmark groups have traditionally focussed on advice for a fish species or a group of species, but the idea can certainly be expanded to other units of advice under an EAFM. The review group directly underneath the benchmark groups reviews everything coming out of the box above it, where general methods and approaches are considered, and a 'benchmark standard' is set for a particular assessment (Jake Rice, WGRED Chair, personal communication).

Other clearing-house type groups in the diagram include groups that evaluate the effectiveness of various approaches to management and groups that develop regional ecosystem descriptions. Some groups do the expert evaluation of status and trends of individual ecosystem components. This can mean individual fish stocks but could also be aggregations of species. They assess the condition and behaviour of these components, i.e. the state and response in the well known PSR model. The Ecosystem Impact Expert Groups mainly deal with the 'P' – pressures. However, the individual component groups will often examine pressure in the case of fishing mortality because the standard stock assessment models actually link pressure (fishing mortality), state (population size and structure), and response (forecast population size and structure) (Jake Rice, personal communication).

The communication aspects of the cultural change problem are in some ways the most basic element because it is here that the cultural and technical issues meet each other, particularly in the form of problems with quality control. This is a key to moving towards integrated rather than concatenated advice. This issue is taken up in more detail in the next section.

Time and money

The third organisational challenge is mobilising the time and money needed to get the ecosystem information into the advice. The ICES leadership is aware that the most difficult challenge is addressing the problem of an already overburdened system.

> Q 6.35 Scientist One: We have a lot of things on the table. One is about the culture. Scientist Two addressed that in his presentation. We have to change the culture and tradition and that is more than the ecosystem approach, it is also time dimension, measures. We are getting all these groups to look at different issues, and this is more work on an already overburdened system. So this question of ecosystem cannot be stuck onto working groups. *(Observer's notes at the Consultative Committee meeting, September 2004)*

The appeals needed are not just to the consciences of their colleagues but to those who control the purse strings. ICES's position as a link between other institutions consisting of a set of both contractual and voluntary relationships may be a source of its flexibility, but it is also a source of vulnerability and dependency:

> Q 6.36 ICES cannot meet the requirements in isolation and must work with other organisations to deliver advice that is timely, responsive, relevant and credible. These issues to be addressed in cooperation with other organisations include:
> 1) The extent to which National laboratories commit the scientists needed to support the implementation of an Ecosystem Approach.
> 2) The extent to which Client Commissions request and pay for advice that is strategic as well as tactical.
> 3) The extent to which the activities of the ICES Science Committees support work that will underpin the implementation of an Ecosystem Approach.
> 4) The extent to which the National and International bodies that fund the science conducted in National laboratories are supporting projects consistent with the development of the Ecosystem Approach (from Document Nine: Key Issues for improving the output of the Advisory Committees, ICES Clients Meeting 2006).

The following two quotes from the informal REGNS meeting show the practical need for input from the science expert groups, and the difficulty of getting that input, and link them to the delegates' control over the 'voluntary' input of the employees of the National Fisheries Institutes.

> Q 6.37 Scientists One: ICES probably already holds 19 of the 70 parameters [needed for the REGNS ecosystem assessment]. The German SYCON study

could offer a potentially significant contribution of data. It is a bit impenetrable. They have a time line. If they don't have the data in time, they will not stop but start reallocating resources to fill the gaps, but they hope they will have it because of all the studies on the North Sea. **Scientist Two: In theory this is excellent. We had talks in the oceanography committee which has a lot of requests, but they have no members here in the relevant groups. We have to convince the Delegates that this has value.** *(Observer's notes at the informal presentation of the REGNS process, ICES Annual Science Conference, September 2004)*

The second suggests that what is needed is an appeal to interests as well, because the needs extend beyond the delegates' reach.

Q 6.38 Scientist One: WG Habitat wants to spend next year working on a cooperative research report, not getting stuff for us, so we will need help getting stuff like this. At the moment I don't think you have got to this stage. Scientist Two: People are interested in what is happening. **This Canada stuff is good science. ICES does not have all the skills we would want to apply. How do we attract skills beyond the control of the Delegates?** *(Observer's notes at the informal presentation of the REGNS process, ICES Annual Science Conference, September 2004)*

ICES has a unique structure as a multi-lateral organisation, a scientific association dependent on many voluntary elements, and an official source of scientific advice to governments. This structure has many benefits including most importantly being a network in which the knowledge needed for the EAFM can be found. Many of the same elements that make possible the willing participation of many scientists, however, make the focussed mobilisation of their knowledge more difficult.

6.3.5 Mobilising knowledge and quality control

In addition to bringing about the changes needed to mobilise ecological information for advice, ICES also has to address a question they usually referred to as 'consistency' but, as discussed at some length in 7.3.1, this term points to multiple aspects of quality control. Many scientists see consistency as one of the most critical issues in giving advice (Wilson and Delaney 2005), and it is an important consideration when trying to mobilise ecological knowledge across ICES. The problem emerges in facilitating the uptake of the science into management and is less often an issue of credibility than it is one of legitimacy:

Q 6.39 Scientist One: The advisory committees cannot themselves be the ones to draft this, but it is just something to put in front of clients who are now saying you talk a lot and you show us little. Scientist Two: This last

> point is why we continue this. When we do the work plan, we position ourselves to more than just talk a lot. **In all my experience, when advice is weakest is when they can exploit inconsistencies that lets them throw it out**, it is in these inconsistencies between the few cases we know what to do and the many where we don't. *(Observer's notes at the Consultative Committee meeting, September 2004)*

At the simplest level ICES faces a problem of consistency in the compatibility of data gathered in different places for different reasons and in different ways (type B consistency in the discussion in section 7.3.1). This kind of technical consistency is being addressed through the ICES Data Centre and the ICES Quality Assurance Programme. The EAFM creates new challenges, but they remain issues of technical collation.

> Q 6.40 Scientist One: Our work has centred on a more pragmatic approach with the addition of some elements of the ecosystem approach in an incremental process. We cannot afford two different data sets on the same thing. We have started with physical oceanography forcing on the development of stocks and then other fisheries interactions. *(Observer's notes at the Consultative Committee meeting, September 2004)*

A problem REGNS encountered with these technical inconsistencies was that the required data were held by so many different organisations. Their solution was to develop regional assessment databases:

> Q 6.41 In the experience of REGNS, it is simply too ambitious to expect that we can simply download all the relevant data with appropriate QA from the numerous sources on the fly for assessment purposes. The systems are simply not sufficiently well developed at a consistent level across the various organisations to do this efficiently. This is not likely to change significantly over the next 5 years, so the need for regional assessment databases is a solution for our immediate needs and will also help to prioritise access to the most relevant data from the archive databases (ICES 2006a, p. 97).

All of these problems of technical data collation, however, are well within ICES's expertise and simply a matter of finding the resources to address the amount and the complexity of the data needed for the EAFM. The EAFM, however, presents deeper problems with data types that are much more difficult to address. Much of the environmental information relevant to the EAFM is qualitative. This is not qualitative in the social science sense of textual information subjected to hermeneutic analysis, such as the qualitative research you are reading right now. The issue here is using information that is qualitative in the statistical sense of being categorical. Ecological information is often in the form of state changes. These state changes can be observed and measured in a rigorous fashion to meet the requirements of scientific credibility through a transparent explanation of how what is known is known. The problem here is saliency rather than credibility or legitimacy. This kind of categorical information is difficult to resolve into specific quantities: the kind of numerical conclusions on which the TAC Machine depends to divide the fish.

> Q 6.42 Scientist One: We have generic resolutions for each working group, and one of them points at using the WGRED. **Scientist Two: We are looking for a mechanism to more formally present qualitative information; I don't see this as a problem, as in the past when all qualitative advice got thrown out, as long as we now have a change of philosophy.** Scientist Three: We have to make sure this change of philosophy takes hold, and that all groups take these phenomena into account. Scientist Two: The North Atlantic oscillation is the example. We don't know how it works, so working groups will treat it differently. Scientist Three: If this is the key way that we intend to go as we go through ToRs, and we find that we don't know what to do, we figure it out. **Scientist Four: We need the change to take place that we move towards the systematic qualitative information, we can't take this on board in the old-fashioned way that says you have to predict numbers. This requires a cultural change for us and for managers**. *(Observer's notes at the Consultative Committee meeting, September 2004)*

Scientist Two sees the issue as one of presentation: if the qualitative information can be presented in a rigorous way, then it should in principle be as useful as quantitative information. The only real block to the use of qualitative information is the philosophy of the user of the advice. The question is, of course, more the usefulness of the information to the management bureaucracy, which is more a matter of institutional and political imperatives than of philosophy as that term is usually understood. The use of the term 'philosophy' here is a boundary device seeking to keep the decision about the saliency of types of information for policy a scientific rather than a political one. At this particular meeting of the Consultative Committee, it was also clear that some scientists – those more involved in the Science Programme – were using the term 'change in philosophy' with an emphasis on the integration of ecosystem information in management advice, while others – those more attached to the Advisory Programme – were placing more emphasis on receptivity to qualitative information and soft predictability.

Scientist One is also concerned that the consistency between groups will be hard to maintain because not enough is known about the underlying processes. It is not sufficient that they see a correlation between, for example, the North Atlantic Oscillation and stock abundance. They also have to understand the mechanism beneath the relationship; otherwise, different expert groups will interpret the correlations differently. There is disagreement in the group about how difficult this general problem is to address:

Q 6.43 **Scientist One**: We need to be comfortable with the idea that the chapters that are the integrated overview are just as good science as the other chapters. We can't just get people together and write them. **I have no faith in a bunch of scientists writing an essay on the environment. SGE and WGECO with more or less the same ToRs produced products that were irreconcilable. Scientist Two**: In my mind this is not complicated; it is clear what needs to be done. A lot of linkages ... Last comment first, **that is not a unique problem, that is a problem for all our advisory committees. For ACFM a different sub-set of members would make different conclusions about a stock. This calls for good peer review.** At WGRED we said we wanted to be proactive and get members from the science committees, good people, and have peer review mechanisms. *(Observer's notes at the Consultative Committee meeting, September 2004)*

At another point in the meeting, Scientist One says with respect to the issue of what kind of ecosystem advice is adequate for the EAFM that 'this is really convincing me that ICES will divide down the middle and the advisory and science sides will go their own ways', and at another point he becomes annoyed and accuses the advisory scientists of not seeing the complexity that WGRED has to deal with.

Scientist Two is a leader in the Advisory Programme sitting somewhat uncomfortably in a meeting of the committee that oversees the Science Programme. (This is the inverse of Scientist One in quote Q 6.25 who in the MCAP meeting was surrounded by Advisory Programme people. The same person is Scientist One in both quotes.) From his perspective the problem of consistency and quality control has always existed in fisheries stock assessment. There it has been addressed by peer review processes, and this approach should work for the ecosystem aspects of advice as well. He is also someone who has been promoting more open and participatory approaches to ICES science, and these two aspects are related in important ways (Funtowicz and Ravetz 1990). He (Scientist Three in the following) also expresses real frustration during the meeting with regard to what he sees as resistance to needed reorganisation because certain scientists and certain expert groups have unrealistically high expectations of what kind of science is required for EAFM advice. (This continues on from Q 6.24, and for continuity's sake I have kept the same numbering for the same people.)

Q 6.44 Scientist One: This word game [qualitative descriptions of ecosystems in the reports of assessment working groups] will not sell to Delegates. Scientist Three: ACE should embody this stuff; it should not have a separate working group to do this. WGRED should be a way to bring these different things together. Scientist Two (annoyed): It is mind boggling that we go back to square one. It is clear that the working groups do not want to deal with working groups. WGRED pulls together information to give to the assessment working groups. That is not advice. Scientist One: I have worked 20 years on this, and we do not have the tools to do anything but

> write an intellectual essay. I don't see us focussing on how to do this. We will have a chapter that says these events happened, and we don't know what it means. I don't see this taken up by anybody. *(Observer's notes at the Consultative Committee meeting, September 2004)*

One strategy that emerged for addressing these quality control dilemmas is having a series of coordinated research foci over time. The idea is that by mobilising expertise step by step, on one issue set at one time and on another issue set at another time, ICES will be able to develop a repertoire of methods to link environmental events to advice. The hope is that when a critical mass of experts has been mobilised, they will produce tools that assessment working groups, as well as other groups charged with producing advice, will be able to take up on their own when events arise:

> Q 6.45 Just as the classic approach to single-species assessment and advice did not have to be reinvented for every stock, perhaps we are on the verge of learning tactics and strategies from these intensively explored cases that will make application to additional stocks, species, and ecosystems move much more swiftly. But that is optimism, however cautious, and not a certainty (ICES 2007a, p. 143).

When this strategy was suggested at this meeting, the response from the DG MARE observer (a different person than the one quoted in Q 6.10) was to ask about REGNS. REGNS in many people's minds was the model of the future ecosystem approach that would make ecosystem advice based on system-level analysis possible:

> Q 6.46 **Scientist One: I support focussing on a few things and doing the job to the point where you can recommend changes in practice**. If you are going to deal with trophic dynamics and species interactions, the easy problems are the ones we can model with a VPA. If you are going to go to the hard questions, we need sea bird and marine mammal people there. **Scientist Two: I agree we have to limit the scope of what we can handle in a week or so instead of coming up with a list of things we should have done**. First, we should make things really operational. DG Fisheries Observer: My recollection of REGNS was that it was to be the model, and it seems to be the best shot of ecosystem advice, we need to have a discussion with the clients if this is working before starting in another direction. **Scientist One**: I was Chair [of a supervisory committee] **when we said to go with the REGNS process, it was not to be the basis of advice, but a way of looking at integration for what groups designing advice should do. REGNS was never meant to be the starting point of advice generation**. (*Observers notes at the combined meeting of the Advisory Committee on Fisheries Management, the Advisory Committee on the Marine Environment and the Advisory Committee on Ecosystems, September 2006*)

The first prong of ICES's response to the EAFM, embodied until recently by REGNS, will certainly continue, but for the immediate future the second prong, embodied by WGRED, will be the priority for the ongoing production of scientific advice. WGRED sees itself as an organisational location for the coordinated research focus strategy:

> Q 6.47 In its role as the clearing house and communication link between the accumulating science and knowledge of how stock dynamics are affected by the physical and biotic environment and the activities of specific assessment and advisory components of ICES, WGRED proposes to focus each meeting on a well-defined set of tasks to facilitate transition from research to operations in this important area. It proposes that in 2007 one focus would be a consideration of several case histories on how to test management strategies and harvest control rules for their robustness to environmental uncertainty (ICES 2006b, p. 118).

The hope is that a comprehensive approach to science for the EAFM can be built up one issue at a time, leading to an eventual convergence between the system-level and the particularistic approaches.

6.4 Conclusion

Any ecosystem-based approach to governance contains two simultaneous requirements that can easily work against one another in practice. On the one hand, an ecosystem approach requires increased interagency coordination; meaning in practical terms the mobilisation of many different forms of both scientific and managerial expertise. This requires centralized decision-making. On the other hand, an ecosystem approach requires more decentralised and participatory decision-making across multiple scales. This requirement is mainly driven by the need for detailed information, and finally knowledge, about processes – both ecological and social – that could be taking place across many scale levels from local to global.

This dual character of ecosystem approaches is reflected in the two ways that we can think about the kind of knowledge dilemma that the EAFM presents. The first is conceiving EAFM as an essentially social problem where EAFM works by ensuring that anyone can effectively identify issues needing attention. The strength of this approach is that it recognises that the information requirements of EAFM are vast and boundless, and it defines how an adaptive social system can be sensitive to environmental changes. Its challenge is the scaling-up of information about problems from the anecdotal form, in which it will be initially generated, to systematic information that can characterise higher-scale natural processes.

The other half of this dichotomy is conceiving of the EAFM as an essentially technical challenge, where the workings of the ecological machine have to be systematically described and its problems diagnosed. The strength of this approach is that it makes possible an analysis of how ecological system processes are related to one another and can be most efficiently addressed. The challenge is the complexity of the system, which can never result in a fully satisfactory model. Problems arise at the level of measuring and gathering information that can be integrated into systemic models and in having a good grasp of the limitations of the models and their associated uncertainties. It also presents a danger that the information that feeds the models will receive more attention than the information that does not, based simply on this fit rather than on more substantive criteria.

These approaches to the EAFM are not mutually exclusive, indeed they are complementary in the sense that the social approach relieves the technical one of always having to have the big picture ready, while the technical approach, cautiously and humbly applied, provides the social approach with a filter to sift signal from noise.

Tackling the scientific problems of the EAFM led ICES to adopt the two-pronged approach to finding ways to produce useful advice. As described earlier, one prong is based on anticipating and understanding the implications of important environmental events for fisheries. The other prong seeks to develop ever better integrated models in the hope of more integrated assessments. Any specific piece of advice will be related to some event. What integrated advice will mean is selecting and putting together a fairly small number of relevant issues, not simply as a concatenation of descriptions in an advice document, but with an analysis of their implications for one another. The *ad hoc* nature of the events-based approach, of course, means that the integrated modelling approach is more attractive to the scientists. So while the long-term hope is for an eventual convergence, both prongs will continue to be needed for the foreseeable future. The expert group scheme offered by WGRED in Figure 6.1 assumes that they will both continue to be important. One hopeful suggestion has been for a coordinated scheme under which ICES shifts focus and concentrates expertise first on one set of related issues and then on another. Each time the goal will be to create frameworks for consistent advice. This may turn out to be an important mechanism for this eventual convergence. One can imagine that the events will define the issues to be addressed while the state of the modelling art will define the frameworks for advice.

Table 6.4 is suggestive. It combines the idea of the social and technical dilemmas with ICES's dual approach. Each of the cells contains a short description of the institutional outcome of the resulting combination.

Table 6.4 Contrasting approaches to EAFM and its related advice

	EAFM as a technical dilemma	*EAFM as a social dilemma*
Integrated models	Quantitative management producing advice based on decision-support rules	Participatory modeling to clarify and test assumptions of those negotiating advice
Events-based advice	Categorical information requiring an extended peer community to translate into practical advice	Stakeholder input into scientific analysis to translate anecdotal concerns to systematic information for eventual advice

The mainstream view of the role of science in the advice process appears in its pure form only in the upper left cell. While I have critiqued the utopian application of that view, this should not be interpreted as meaning that final decisions based as much as possible on some objective criteria are not desirable, they are merely far from sufficient. There is still a critical role for science in management, especially with respect to setting overall limits on ecosystem impacts. The discussion in this chapter suggests that at the current state of the art, and given the uncertainty of the marine environment, this could be a permanent constraint. This mainstream, objective, limit-setting role should be based on some very simple indicators and causal assertions. The remainder of the science for the EAFM is going to require new ways of working with the science boundary. The other three cells suggest forms that supporting institutions could take.

The interesting question is whether ICES has the capacity and willingness to find new ways of working. This question does not have a single answer. ICES is many things and has many attitudes towards the meaning of science. All the parts of the ICES culture are in agreement over what it means to do science, but there are many different and sometimes incompatible ideas about what it means to practise science. These differences form an important background to understanding ICES in general, as well as understanding ICES with respect to the EAFM. The next chapter examines the more general political realities of ICES in the hope of tracing changes in the understanding of scientific practice and of the science boundary, and their implications for the EAFM.

7. The debate over the reorganisation of the ICES Advisory Programme

This final chapter in the ICES case study examines a recent episode of restructuring the Advisory Programme. This restructuring provided an interesting research opportunity. Chapter 6 presented the institutional issues around developing an effective scientific knowledge base for the EAFM. The debates around restructuring the Advisory Programme allow us to examine how the EAFM requirements fared as just one of the issues driving the restructuring. As an intergovernmental organisation charged with producing scientific advice in a high-stakes, high-uncertainty environment, ICES is a political system as much as anything else. An episode of major change allows the political tensions to come to the surface.

The new advisory structure's most important change was the dissolution of ACFM, ACE, ACME and MCAP, to be replaced by the creation of a single advisory committee – ACOM. Diagrams and basic descriptions of the old and new ICES structure can be found in Section 4.1.4. The new system came into being on 1 January 2008 after an intensive discussion that began in earnest at the ICES Annual Science Conference in September 2006. The beginning of 2008 hardly put an end to the debates around the restructuring and its implementation. Nor did the restructuring process have any real beginning point. One Advisory Programme leader I interviewed pegged the beginning of the current round of reorganisation to the mid-1990s when there were a lot of debates on the reform around the Advisory Committee on Pollution. The fact that that committee did not have formal, national representation became an issue as it began to have a higher public profile. This respondent sees issues of balanced representation between member states being an important driving force in the restructuring process.

ICES is an institution adapting to a political environment that in turn reflects the evolution of environmental concerns. It is always restructuring itself one way or another, going through phases of more or less intense changes. When you are structuring marine biology, an ICES leader explained, you always end up with two choices: you can structure the science 'holistically' by areas, or you can structure the science 'atomistically' by species, or more often species groups such as pelagic, demersal, shellfish, etc. Both have scientific advantages, and both have scientific adherents. So to understand what is going on in ICES now, it is essential to see that the current restructuring is a pendulum movement towards a holistic area structure, happening because of the ascendance of ecosystem considerations. Griffith (2003), a former Secretary General of ICES, describes this cyclical movement from area to species in detail. His figure showing the evolution of the science committees (Figure 7.1) clearly reveals the movement between the area and the species-based groups and the compromises between the two. The final phase moves into a more programmatic distribution of tasks that uses categories such as ‘living resources’ and ‘fisheries technology’, which are even broader than areas.

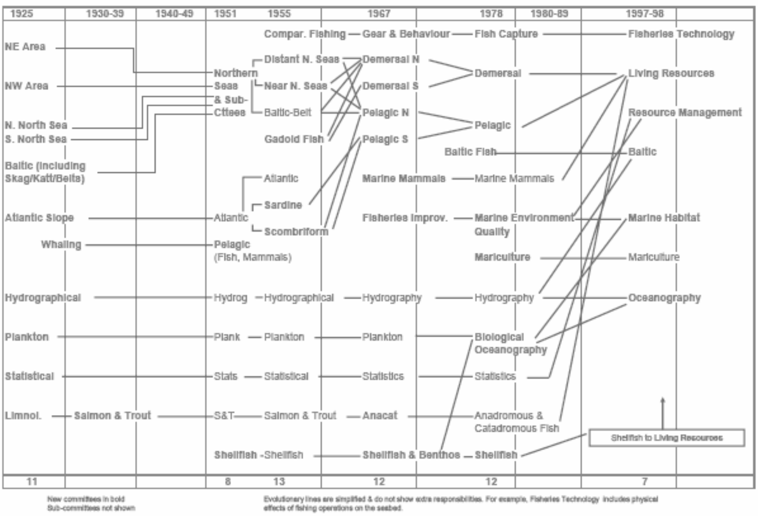

Figure 7.1 History of ICES restructuring from Griffith (2003)

These broader categories are perhaps an attempt to get around the inherent trade-offs, but it is very difficult to organise science to be both atomistic and holistic at the same time. One very important example of the cost was the conceptualisation of the precautionary approach as ‘stochastic predictability’ (Degnbol 2003) that I have mentioned several times. In the early 1990s there was a lot of enthusiasm around developing an ecosystem approach. But this development was overtaken by an atomistic conceptualisation of the precautionary approach driven by the single-species advisory needs of the TAC Machine. Because of international movements and agreements after the Rio Summit, the precautionary approach and its implementation became the top priority. The residual regret from this shift is still evident, as can be seen in Q 5.12. The ICES leader cited above called it a step backwards in science terms, because suddenly scientists had to use their energy to define reference points and analyse stock recruitments. He believes that this intense focus on the single-species approach continued right through the 1990s, and that serious attention to the EAFM within the ICES community did not begin until 2000, a change marked by the creation of the Advisory Committee on Ecosystems (ACE). At that time there was an extensive debate on the advantages of a single advisory committee, but that idea did not prevail until the creation of ACOM eight years later.

Attempts at restructuring carry a lot of baggage. When major changes in ICES's structure are being contemplated, the opportunity arises for a number of agendas to be 'bolted on'. Because of the sense in which the restructuring is always going on, one leader views this process in historical terms, arguing that the bolting-on is what makes the basic restructuring imperative:

> Q 7.1 Delegate: But you can build in, in redefining a new process, you can build in, you know, better quality systems or whatever, because that was one of the weaknesses of the old system, that it began simply as an advisory committee for fisheries actually. And people did it the way they thought it needed to be done 25 years ago. And then as the demands of the system increased, for example because you needed better quality control, because you needed more transparency to involve stakeholders – those kinds of things were bolted on. And you ended up with a more and more cumbersome system.

Indeed, the question 'how did the EAFM fare in all of this?' introduces its own bias about what is central and what is being bolted on. Many, but far from all, ICES scientists saw the EAFM as the main issue for the restructuring.

The chapter begins with a general description of how the restructuring discussion process took place, basically from the 2006 Annual Science Conference (ASC) through the final decision by the ICES Council. Then I turn to an overall description of the general political context around ICES. The next section describes six important themes in the restructuring debate. The chapter concludes with a discussion of the implications of the restructuring for the EAFM.

7.1 The process of restructuring

The final decision on restructuring the ICES Advisory Programme had to be made by the ICES Council. The decision on the proposal discussed here was taken by the Council in October 2007 for implementation in January 2008. By the time the proposal reached the Council, however, it had gone through a considerable process of discussion.

The initial ideas for the new advisory structure were formally presented for the first time at the ASC in Maastricht in September 2006. The ASC is not merely a science conference, there are also a great many ICES committee and research project meetings going on around the edges. Representatives of MCAP, the committee responsible for the Advisory Structure, and the ICES Secretariat worked together to formulate an initial set of issues and ideas and present them to the major committees on both the science and the advisory sides. This generated considerable and ongoing feedback. Over the following year the restructuring proposals gradually solidified. By the ASC in Helsinki in September 2007, a fairly detailed proposal was on the table. At this point, with the Council meeting only one month away, MCAP began to treat the process more as one of selling the existing proposal than discussions to develop a proposal. MCAP developed a presentation, and the Advisory Programme leadership carried it around to both science and advisory committees, selling the proposal. This included a meeting with some of the delegates, at which the critical group of the NFI Directors was well represented.

The response of the scientists varied. Many were nervous about what they were creating, but at the same time the process was advanced, and major changes were not going to be entertained. A long discussion took place at the ACFM meeting in which many people suggested that ICES should experiment with the new system in an incremental way. Perhaps it could be an experiment with just some groups. They were afraid of 'the devil in the details' and were reluctant to jump into a new system all at once. The conclusion was that this was not a very practical suggestion. The basic structure would have to be one way or another, and ICES does not have the time or resources for parallel systems.

The critical response came from the meeting with the delegates. This was not an official Council meeting; this was an informal meeting of delegates present at the ASC. The main concern that MCAP and the Secretariat had at this point was that the proposal would not be acceptable to the delegates. This was their last chance to get feedback from them, and feedback they got. The informal meeting produced a list of 33 issues about the proposal and its implementation that they were concerned about. Ten of these concerns were structural issues that the delegates themselves would have to decide about, the others were more general issues for the whole ICES community to address. The EAFM was clearly not the most immediate issue for the delegates with respect to restructuring. In fact, the only two concerns the delegates had about the EAFM were quite similar: The 17th item asked, 'How will the integration work?', meaning the structure of the new committee, and the 24th asked, 'How will the integration of advice happen?', referring to its product.

Nevertheless, the EAFM played an important background role in Council discussions. In response to a question about what was the hardest thing the Council had to address in the restructuring, a delegate said:

> Q 7.2 To think what the problem was, that we were trying to solve. I think trying to get a clear idea of what the issues really were. And I'm not so sure that that has actually been identified. This ecosystem approach was kind of talked about a lot in the context of the restructuring. I'm not sure that's really understood, and therefore it's not really very clear what it is we're trying to do in restructuring.

The delegates also set up a committee to work on these issues and create a final proposal to come before the Council in October. The announcement of this decision at the last meeting of MCAP was greeted with some nervousness. Everyone knew that the delegates had the final say, and one scientist pointed out to MCAP that the creation of the delegates' group was exactly what was needed to 'get the ball over the line' in the Council meeting. But seeing the final month of the development of their proposal taken out of their hands by a group of NFI Directors was not easy for the leadership of the Advisory Programme. Several responded in typical ICES fashion by sitting in the MCAP meeting making up descriptive, and somewhat disrespectful, acronyms for the delegates' group. They will not be repeated here.

At the meeting in October, the delegates did adopt the new proposal. A respondent from the Advisory Programme leadership described the two main issues discussed with the full delegate group at the October 2007 meeting. He said the delegates were most concerned about the relationship between the ACOM leadership – the chair and three vice-chairs, and the Secretariat. Some were concerned about setting up parallel structures and wanted the new advisory group under the Secretary General. Others wanted the ACOM chair to report directly to the Council. The other main concern was the separation of the review groups and the advice production groups. The only major structural change they made to the proposal from MCAP was that they separated the groups charged with the scientific review of the outcomes of the expert groups from the groups that created the advice based on those outcomes. In the initial proposal these two functions had been placed in a single group. The arguments for these two approaches are presented below.

7.2 The main fault lines in ICES politics

Before addressing the major themes within the restructuring debate, it will aid their understanding to describe a bit of the general political tensions in which ICES operates. As an inter-governmental organisation, ICES politics are driven mainly by the political imperatives of the governments of the countries that make up its membership. These are usually expressed through the National Fisheries Institutes, although the relevant central ministries are sometimes directly involved. These two do not always express the same priorities. One ICES leader commented at the September 2007 MCAP meeting that when central ministries 'say they will do something, the NFIs are often quite independent of this' (see also Q 7.57). The relevant ministries for ICES range from those having a primarily economic focus to those having a primarily environmental focus. ICES's role as advice provider for the Common Fisheries Policy, a policy critical to some but not all of the member countries, complicates matters even further. We could conceive of most of ICES's political tensions as running along a three-way circuit among ICES, the member governments, and the European Union.

One of ICES's basic purposes is to take a strong leadership role in marine science, which includes both speaking with an authoritative voice and setting an international scientific agenda.

Article 1 of the ICES Convention reads:

> Q 7.3 It shall be the duty of the International Council for the Exploration of the Sea, hereinafter referred to as the 'Council',
> (a) to promote and encourage research and investigations for the study of the sea particularly those related to the living resources thereof;
> (b) to draw up programmes required for this purpose and to organise, in agreement with the Contracting Parties, such research and investigations as may appear necessary;
> (c) to publish or otherwise disseminate the results of research and investigations carried out under its auspices or to encourage the publication thereof.

To maintain this leadership role is something many ICES scientists see as a very high priority. The catch is expressed in the Convention with the phrase 'in agreement with the Contracting Parties'. Each member country has its own national strategy for marine research, which forces ICES to try to bring these strategies together in a fairly top-down manner without any leverage beyond persuasion. This creates ongoing tensions with respect to both setting a science agenda and speaking with an authoritative voice. The following notes from a discussion at the committee charged with overseeing the Science Programme illustrate these tensions. The last comment by Scientist Two also illustrates how the third pole in ICES's three-way political circuit, the EU, influences these tensions between ICES and the NFIs. The ICES hand is strengthened vis-à-vis the NFIs of EU member states because a common voice has a greater influence on the Commission. As we will see below, ICES member countries outside the EU also strengthen ICES vis-à-vis the EU.

> Q 7.4 **A Delegate**: The framework document is different from the other science strategy in that it recognises that ICES is a headquarters and **each country has a national strategy for marine research. ICES can try to bring these together. This is different from bottom-up, it is very top-down**, but we are an intergovernmental organisation. Another issue is the lack of support for the activities. The view of the Council is that this is not our fault as lab directors, if someone proposes a working group I am not going to vote against it, but it does not mean I will send anyone. **We have lots of committees that do not attract anyone, and they [the Council] conclude that there is a lot of science going on that is not really important. Society expects ICES to have a science view but it does not, except what goes through Council. When working groups produce something it has a big 'you can't use this'**

> **on it.** [All expert group reports have a caveat that they are not to be cited without the permission of the Secretary General]. **They are not the ICES spokespeople.** But other [science providers] are jumping in, and ICES is being marginalised. We would like the science side to be more empowered and focussed on key programmes that make a difference and so that society can see we are making a difference. **Scientist One: On the one hand you highlight that each NFI has its own priorities, on the other you say the Delegates feel that the things ICES is doing is not that important. Is this just a competition that ICES is losing because each Delegate is promoting his own institute instead of a community?** There is some kind of contradiction here. What ICES can do is take advantage of its being international, and that means setting the agenda for its own research. Delegate: My own view is that all the members would love ICES to be more dynamic. It is not just wanting their own institutes to be more powerful. **Scientist Two: Scientist One is sort of right, but wanting an institute to be best means partly going through ICES. If you have a number of member states Europe listens better.** *(Observer's notes at the Consultative Committee meeting, September 2007)*

This 'more dynamic' general marine-science leadership function, and maintaining the authoritative voice that goes with it, is not easy. Resources are available for generating ongoing advice through the NFIs. But the NFIs must compete with academic institutions for the pure (or at least less directly applied) science funding that is available. The ICES network lends its participants important advantages in seeking funding. There are marine and fisheries areas within the EU Framework Programme where the size and experience of the NFIs give them advantages over universities, but the funding for which this is true also tends to be for short- to medium-term science for policy support. ICES scientists are very concerned about the availability of this 'less applied' funding.

> Q 7.5 Scientist One: ICES's main weakness is a lack of money. The work we do for national managers or RACs has money involved, and this leads to a vested interest, so working for these groups the question is, can you show scientifically that X is possible. In ICES we can set the scientific agenda and not be told what to do. Scientist Two: In comparison in STECF you have a sense of continuity, in ICES you can look at bigger questions. *(Observer's notes at the Resource Management Committee meeting, September 2007)*

ICES – in the sense of the legal entity created by the Convention – is involved in few research projects itself, and ICES scientists have said that the NFIs actively discourage this. ICES – in the sense of the network – is deeply involved in large, multi-partner research projects involving both NFIs and academics. ICES facilitates these projects in many ways, and the projects create powerful synergies with ICES expert groups, but the project resources remain with the NFIs. The majority of these projects are EU-funded and involve EU NFIs; Iceland and Norway count as 'EU' in this context, because they have reciprocal agreements with Brussels.

Later in the same Resource Management Committee meeting, an ICES leader said 'until a few years ago we were in the business of organising international projects, in those days the resources for this were in the NFIs, now they are not. I do not see ICES in the business of doing projects, but there is a niche for feeding the priorities to the funders'. This comment, from a leader who happens not to come from an EU country, expresses a common idea among ICES scientists about ICES's research leadership role. It is interesting because the most direct NFI influence on the single most important science funder, the EU's Framework Programmes, bypasses ICES altogether and flows instead through the European Fisheries and Aquaculture Research Organization, which is made up of the directors of the NFIs. The ICES delegates do not choose ICES as their primary vehicle for seeking to influence marine science research priorities in Europe.

There is also direct competition between ICES and the NFIs for the resources needed for the Advisory Programme. One lab director told me that he considers this a symptom of a much deeper contradiction within the CFP:

> Q 7.6 The European member states have decided that the Commission should be the one handling the fisheries questions, but they've not been willing to also then let the Commission have control of the resources on the national level necessary to do the job.

Another lab director, however, put a little different spin on this tension:

> Q 7.7 I'll have to give you a bit of history. The way ICES traditionally has worked for fishery advice, which I guess is probably, you know, maybe 80% of all the advice that ICES does, is that it set up stock assessment working groups that met for a fixed time in a fixed place and then they passed on their reports to the ACFM, and the meetings typically took place in Copenhagen. And that was probably okay when the demands of doing an assessment were much less than they currently are, you know in the past a typical stock assessment report might have only been 60 pages, whereas now it's probably about 1000. That reflects the fact that for any particular assessment, the quantity of work done is huge compared to what it was 20 years ago, and yet you're still expecting the same sorts of people to do the same amount of work in a week in Copenhagen.

For at least the five years that I have been observing ICES, the leadership of the Advisory Programme has been striving to shift the role of their expert groups from actually crunching the numbers on the stock assessments to finalising and reviewing the assessments and beginning the task of translating them into advice. The idea is that the initial analysis will be done at the NFIs before the expert group begins. This desire was forcefully expressed in the proposals for reorganisation. Expert groups, especially those related to assessments are simply much more effective if the scientists, and the data, are prepared ahead of time. From the NFIs' perspective, however, this means an expansion of ICES activities beyond just the time boundaries of the ICES expert groups to the home offices as well. The following is a quote from a communication from a lab director to MCAP expressing strong concern about this aspect of the changes:

> Q 7.8 It is obvious from [a preparatory document for the meeting] that more work and responsibility shall be moved from ICES into the national labs, i.e. personnel shortcomings shall be reflected (or guided) towards the national labs, and the Delegates shall have the responsibility to safeguard that there is enough qualified staff for the EGs [expert groups]'as required by ICES' and for the RGs [review groups] 'as required by ICES', these being two phrases which bring a new taste into the recipe. In other words: instead of making a numerical analysis, if at all the necessary resources are available for the new structure, the responsibility for (probably) more input is shifted elegantly to the national labs. *(Observer's notes at the Management Committee for the Advisory Programme meeting, September 2007)*

To this a leader of the Advisory Programme responded:

> Q 7.9 [The lab director] sees this new plan as pushing work from ICES to NFIs, but all we are asking is that people do their work at their desks at the NFIs rather than at ICES.

Again this tension between ICES and the NFIs has an important EU dimension. A leader of the Advisory Programme in an interview:

> Q 7.10 We are in a competition situation with the EU and STECF because they pay travel for the people participating here, and, not surprising, the institutes are more likely to send someone to STECF for two reasons, the one is that it doesn't cost them anything on the travel budget, and secondly you're one step closer to the decision.

The problem of the effective use of scientists in expert groups is considerably exacerbated by demands from member governments that their national scientists be included in various expert and advisory groups, regardless of the scientific necessity for such inclusion. Issues of national representation create complexities in the assignments to expert groups. A leader of the Advisory Programme described it this way:

> Q 7.11 Resources are used inefficiently, and the reason why they're used inefficiently is because of mistrust ... Countries do come running and say 'we want our scientist to sit there'. There is an enormous amount of people

> sitting in working groups doing nothing except observing. They are formally participants, but actually they are not contributing and every chair of a working group, very unofficially and so on, is ending up with four, five people he doesn't really know how to keep busy.

An efficient approach to ICES work would depend, for example, on having a Danish institute that is very good at a certain task come to ICES expert groups and focus on that task. But the Danish government (and this is not to pick on Denmark, the same is true to some degree of all the EU ICES members) sends its people to expert groups that they see as politically sensitive. This could mean sending them to groups working on the assessment of a stock of interest to the Danish industry – and in this case the Danish scientists would likely be the real experts – but it could also mean sending them to groups working on issues that are politically sensitive at an EU level. When, in the respondent's example, Danish scientists attend the Deep Sea Working Group, they are doing so because someone in their ministry sees this as an environmental issue of general European interest and wants Denmark involved.

This same problem of distrust among the member governments has led to differences in rules about attendance at expert group meetings. Scientists can attend the meetings of expert groups under the Science Programme at the discretion of the chair, but if the committee is under the Advisory Programme, then they must be nominated by a delegate. This has led to political games around the programme assignments of expert groups. It also plays into the debates around stakeholder observers of ICES groups. The organisation of observers for expert or review groups meeting remotely, by video-conferencing for example, has been questioned by delegates on the grounds that it is impossible for everyone to know what kinds of conversations are going on between scientists back in their labs and the stakeholder representatives.

As the following exchange at a meeting of the Advisory Committee for Ecosystems indicates, this problem will become even greater with the implementation of the EAFM because of the smaller pool of more specialised knowledge that will be required:

> Q 7.12 Scientist One: I think for a group like ACE the key thing will be the review groups where you integrate the advice. How do we find the expertise to draw on and make sure it is represented in all review groups? I see a few of them with lots of people wanting to sit on them while others will be lacking in expertise. This needs to be communicated to the bureau, Delegates and NFIs. The expertise we need is only concentrated in particular parts of the ICES community. In many of these areas people will be asked to look at things outside their national interests. The Delegates need to understand this problem as they are used to having fisheries experts that know their local stocks. Scientist Two: We need to build the capacity through the

international ICES community. *(Observer's notes at the Advisory Committee for Ecosystems meeting, September 2007)*

This three-way political circuit between ICES, its member governments, and the EU suggests that it makes a significant difference if a member government is within the European Union, or more accurately the Common Fisheries Policy, or not. This distinction, however, is really three-way. There are those governments that are part of the CFP, those that negotiate with the EU over issues for which ICES gives scientific advice, and those that are outside both the CFP and these negotiations. Most ICES members fall within the CFP, the second category is made up by Norway, Iceland and Russia, and the third category by the USA and Canada. When asked about how these differences influenced the reorganisation debate, a delegate responded:

> Q 7.13 I would say that that's probably one of the most significant areas of cleavage on the Council and not surprisingly Norway, Iceland and Russia feel, I think, that [the reorganisation has] been driven by the needs of the EU countries ... I think there's a worry in Norway and Iceland, and I'm not sure how much Russia necessarily shares it, that the EU's needs can undermine ICES – not deliberately but just because of the ways things move. I mean, you could argue for example that STECF could do everything ICES does in terms of fisheries management, and I think Norway in particular and to a lesser degree Iceland is worried about that. And so they want to make sure ICES is strong and functioning so that it's a bastion against the EU-bloc.

Scientists within the Advisory Programme have to be continually aware of these differences. This is illustrated by the following exchange that took place at one of the central meetings for the coordination of the Advisory Programme.

> Q 7.14 Scientist One: Because ICES is wider than the EU, and the RACs are basically EU, and they are just one of the industry bodies (*sic*)[14]. The important thing is to be aware of this and not exceed the boundaries. Scientist Two: That is an important observation. I had an issue [at a pelagic RAC meeting] about mackerel in January, I asked for the Norwegians to come and they were not included so I declined to participate ... you as chairs of working groups may be in a similar situation. We are not part of a directed process serving one part of the community. Scientist One: It is also fair to have a distinction between stocks and areas that are internal EU and those that are shared. Then the relation between RACs and the Commission and ICES will be different. *(Observer's notes at the Annual Meeting of Assessment Working Group Chairs, February 2008)*

Scientist Two had to decline participating in this event because the RAC was an EU body that was addressing a stock shared with Norway, while excluding Norwegian participation. ICES has been making a conscious effort to support the RACs as much as it can given resource constraints, and Scientist Two, as it happens, has been deeply involved in this effort. But ICES cannot be 'part of a directed process serving one part of the community', even when the 'one part' in question, the EU, is ICES's largest client. Some within ICES believe that ICES should go further in using the CFP/non-CFP cleavage among its members to increase ICES independence vis-à-vis the Commission. Here is one delegate's opinion expressed in an interview:

> Q 7.15 Delegate: We put too much weight on what the Commission wants [people argue that it is because they are the paying client, but] it is in the Commission's interest, as well as Norway's interest, that the Commission does not come to the negotiation with one recommended TAC and Norway comes to the negotiations with another recommended TAC ... it would be completely impossible for the Commission [to refuse to use] ICES advice because they don't like the way ICES is organised.

He is, of course, well aware that the Commission contributes a considerable part of ICES's income and that the Commission always has the option of getting its advice from STECF without using ICES. However, he believes that the fact that the EU must negotiate the sharing of stocks with Norway, Iceland and Russia means that they must use ICES advice rather than advice from a body internal to the EU. So it follows that the fact that the Commission can turn to STECF should not be seen by ICES as a reason to give the Commission's opinions extra weight in the debates over the reorganisation. Of course, one can point out to this delegate that there are many EU stocks that are not shared with outsiders, but he will rightly respond that the shared stocks in question are among the largest and most valuable ones in European fisheries.

Finally, the other category of ICES members, the non-CFP members that do not share fish stocks with the EU, also plays an important role. The following is a quote from an interview:

> Q 7.16 Delegate: With the exception of the advice about sonic impacts, Canada and the United States don't have any vested interest in the advisory process ... those countries, in particular the United States and to some degree Canada as well, are able to speak with some authority because they don't actually participate in the advisory process, well they're not customers for the advice – they're honest brokers, and that has helped. Because of this these two countries played an important role in the restructuring.

The United States held the chair of the Council sub-group that was formed in September 2007 to finalise the restructuring plan and present it to the full Council for approval in October 2007. The current chair of ACOM is also an American, creating the interesting situation that an American is the single most important person in the system creating scientific advice for European fisheries.

This concludes this very short description of the political landscape around ICES. Obviously, describing the political dimensions of a network this size and this layered could be a book in itself. However, I think this sets the main context needed for understanding the themes and agendas that made up the debate over restructuring the ICES advisory process.

7.3 Themes within the restructuring debate

This section outlines six recurrent themes in the debates over the restructuring of the Advisory Programme. Clearly, a complex debate can be divided into an endless number of 'themes'. What is presented here represents an interpretation that I arrived at through the process described below. Indeed, all of the qualitative material in the book was organised for presentation in a similar fashion, although the goal of identifying a set of themes for a specific discussion gave the method a sharper than usual focus.

The themes were identified by going through the material from the interviews and meeting observations, paragraph by paragraph, and assigning to each paragraph any number of short codes naming a topic or specific argument. Examples of such codes include 'workload', 'regionalisation' and 'advice must be consistent'. These codes were not created beforehand; instead they were generated by reading, interpreting and comparing the paragraphs using a well-established social science technique called 'grounded theory' (Glaser and Strauss 1967). For the material in this section a total of 53 such codes were created. Then similar codes were merged together. The results were examined for the amount of material that they covered. The themes presented here came from codes that seemed to cover a distinct part of the debate and were attached to more than a few paragraphs, the smallest number being ten. I do not report these numbers because they do not mean anything in a statistical sense. Indeed, no check was ever made to ensure that every paragraph in all these documents that could be coded by a particular code was so coded. That would not only have been an exercise in spurious precision, it would be misleading because it would imply that the boundaries between these coded concepts are well enough defined to yield countable units. These numbers were simply a rough guide helping me focus a purely qualitative, hermeneutic exercise. The codes are described below using the relevant parts of the paragraphs that were used to define them in the first place. Not all paragraphs that attach to a theme are reported, many are redundant and others appear elsewhere in the book illustrating other points.

The themes that emerged to describe the discussion around reconstruction are 'consistency', 'advice and review groups', 'workload and timing of advice', 'the respective roles of the Secretariat and ACOM', 'observers', and 'the EAFM'. I present them in this order.

7.3.1 Consistency

Few words are used in ICES with the consistency of 'consistency'. Setting aside basic scientific review, which is the topic of the next section, consistency is the most common concept ICES scientists draw upon when discussing quality control. Inspired, I suppose, by the Mode Two science theorists who point to quality control as a key question in examining changes in science institutions, thinking and asking about what is really meant by this word when ICES scientists use it has become a preoccupation of mine. I am convinced that the combination of the importance of this word for these scientists, and the various things they mean when they say it, is an important clue to the relationship between science and scientific advice and to the tools they are building to respond to the paradoxes of transparency.

In an interview a leader of the Advisory Programme indicates that he thinks that consistency is the real problem that the reorganisation is trying to address:

> Q 7.17 Advisory Programme Leader: When we started this, it was not as much integration *per se*, but to make sure that ICES had consistent answers, of course that requires integration, but the real concern was the consistency, that we had three voices. Three different people for all their qualities, of course, will have a slightly different tack on anything, and we didn't have a proper coordination device in the old system … They had rather free hands. I must also say that over the years we have had relatively few problems, but it became very clear that the issue of integration or needing consistent advice became more and more important because of the ecosystem approach which meant that we had several organisations dealing with, if not exactly the same issues, then very much the same.

The change from three advisory committees to a single ACOM means that it will be easier for ICES to put out a consistent message because there will not be three advisory chairs with relatively 'free hands'. The reason that this had become much more of an issue than it had been before is because with the EAFM, the former advisory committees, ACME (Marine Environment), ACFM (Fisheries Management) and ACE (Ecosystems), were being asked to address the same issues, where in the past they had been able to operate relatively discretely with little problem. In the past, consistency has been more of an issue for fisheries advice. ACE and ACME tended to address fairly discrete requests for advice, while the fisheries advice was given for a variety of issues that were very similar in form. The ecosystem and environmental advice was also usually given only once for the same issue while in fisheries the same kind of advice recurred, so questions of temporal consistency arose as well.

This placement of consistency at the heart of the motivation for restricting the Advisory Programme, and the linkage to the EAFM, is echoed by a delegate:

> Q 7.18 Delegate: I think that one of the most important things was that we couldn't have a system that allowed different groups of experts to give different opinions.

While another delegate feels that the Council has still not addressed this question forcefully enough:

> Q 7.19 Interviewer: Have you noticed much concern with consistency in advice among the Delegates? Delegate: To the extent we have discussed that, which we have done too little … I don't think there has been very much; the question has not been raised very much.

Some clients have also expressed concern with consistency of advice, while others have not:

> Q 7.20 Advisory Programme Leader: The key example of a client was Norway, who repeatedly has said to us that a key feature of ICES is consistency, in time, between areas, and this has been followed up, so, yes, it's a client concern. We have not had the same thing from the EC surprisingly enough because the area they are dealing with is much bigger and therefore by definition much more diverse.

Norway, with its comparatively homogeneous set of fisheries, is particularly interested in consistency. The EU, however, with its comparatively heterogeneous fisheries, is less concerned with consistency. The Advisory Programme Leader finds this surprising. From a formal perspective, greater heterogeneity would seem to increase the demand for consistency, but it may also make it seem less desirable if attempts at consistency were to hide critical differences.

Types of consistency

Just in this short introduction to the discussion of consistency, the term has been used in several different ways. I find it helpful to identify six different meanings of the term consistency as I have heard it used in ICES. They are ordered along an intuitive continuum from technical to social consistency. Types A and B are almost entirely technical in nature while the last four are related directly to advice. Types C, D and E have to do with both the social and technical organisation of advice, while type F is almost entirely social and, in fact, is an issue that arises for any large organisation, scientific or not.

- Type A is consistency with the accepted standards of science, and it responds to questions of credibility;
- Type B is consistency of methodology across time, space and scale that allows the integration of information and creation of new knowledge. It is related particularly to methods of data gathering. Again it responds to questions of scientific credibility;
- Type C is consistency with standards of advice, and it responds to questions of legitimacy;
- Type D is consistency in advice in response to a single policy question that involves multiple scientific disciplines. It responds to questions of saliency;
- Type E is consistency in the way scientific methods and scientific advice are linked across time, space and species. It responds to questions of legitimacy;
- Type F is consistency of message from the various parts of ICES, and it also responds to questions of legitimacy.

Type A is seen by ICES as primarily a question of peer review and is discussed in the next section. **Type B** was addressed in Chapter 6 in respect of the question of integrated as opposed to concatenated advice. Type B consistency makes integration possible.

One of the background agendas in the restructuring was to facilitate Council oversight of the Advisory Programme because of a concern in the Council about **Type C** consistency. In this case it is consistent adherence to advice practices that reflect international agreements. This has meant the precautionary approach in particular, but interest in ensuring adherence to EAFM is beginning to be felt as well. Under the structure where MCAP sat at the top of the advisory pyramid, there was a fairly weak link between the Council and the advisory committees. Council would set policy but, with MCAP meeting only twice a year to oversee the advice, the Council's own oversight, at least on a formal level, was distant and somewhat after-the-fact. According to one Advisory Programme Leader, the Swedish government in particular was concerned about creating clearer lines of command from the Council on down. Our respondent described this as 'not an attempt in any way to influence the science' but to ensure that the precautionary approach was being used. As discussed several times in this book, within the ICES culture, insistence on the precautionary approach is not considered a source of influence, or at least undue influence, on the science. This desire was justified in part by the fact that the member governments are signatories to international agreements that require the precautionary approach. Currently, a parallel movement is taking place with respect to the EAFM, with citation to the 2001 Reykjavik Declaration through which governments have committed themselves to the EAFM.

Type D consistency is raised by ICES leaders with respect to coherence of advice between fisheries, ecology, and environment now that they are being asked to tackle similar issues in a multi-disciplinary framework. One hypothetical example of this kind of consistency, offered to me by one of the delegates I interviewed, was that if ICES is giving advice about the exploitation of a fish stock, the former ACFM might advise one level of exploitation based on sustainable harvesting, and the former ACE might advise another based on the needs of sea birds. ACOM in this case would presumably choose one of these two levels as the 'ICES advice'. This would require either an ICES judgement about which was more appropriate, or more likely a dialogue with clients about which one they really wanted.

Type E consistency is the most interesting. I asked the leader of the Advisory Programme, who had told me that consistency was a main driver of the restructuring, what he meant by 'consistency':

> Q 7.21 Interviewer: What does consistency mean in this case, consistency of quality? Advisory Programme Leader: Consistency in making similar judgements on similar types of issues. So if you have lousy catch-data in one area then you provide an assessment and an advice on that basis, and in the other area you have the same lousy data but you don't provide them an assessment and an advice. I think the concern is mostly on the advice part, but there's also some concern about why do you have this method in area VI A for haddock and this other method in the Irish Sea when you have exactly the same data situation? Interviewer: Is that ever arising enough to send you back to the work you've done to do it over? Advisory Programme Leader: No. Interviewer: Have you ever done that? Advisory Programme Leader: No, I think what happened is that review groups have raised questions; why do you do this? But there's very little follow-up on what happens with that question.

The question is one of 'similar judgements' about 'similar issues'. The question often seems to be couched as questions about methodology, and failure to meet this kind of consistency is often referred to as an 'error' that should be caught by review. A good example of this is the herring incident discussed in the next section in which the problem was not an error in the sense that a scientist used a method that was wrong, it was that a scientist used a method linked to a piece of advice that was not the same method that had been linked to that piece of advice in previous years. The last part of the quote is also interesting as it points beyond a simple concern with consistency to a concern that consistency is very difficult to control. Follow-up on quality control issues is a problem that ICES scientists have mentioned in a number of contexts. This is also discussed further in the next section.

This respondent offered the following as an example of a client-raised problem with Type E consistency. There was a debate whether the deepwater redfish (*Sebastes mentella*) should be treated as one population all through the northeast and actually a bit into the northwest Atlantic, or whether it can be separated into a number of different stocks. There is no consensus on this. So after much debate, ICES decided to treat the deepwater redfish as just two stocks. A similar assessment had to be done on the black scabbardfish (*Aphanopus carbo*) that ranges from south of the ICES area deep into the North Atlantic. The evidence for treating this as a single stock is also uncertain. Our respondent in fact thought it was perhaps even less certain than for the deepwater redfish. However, ICES decided to divide the black scabbardfish into a number of separate stocks. A client then wanted to know why one fish was treated one way and the other fish was treated the other way. Type E consistency raises some very interesting sociological questions about the meaning of consistency that I return to at the close of the section.

Type F consistency is very much a concern of the ICES leadership because ICES is such a public body acting on the public stage. This is often certainly the primary way the Council, as the intergovernmental overseers, imagines ICES. From this perspective ICES must not only be consistent, it must speak with a single voice:

> Q 7.22 Delegate: The advice given out by the three advisory committees for ACFM, ACE and ACME was advice given on behalf of the Council. So the Council basically said, 'we will give advice, but in order to do that we have to set up an advisory committee to do that for us, and we delegate that responsibility to ACFM or whatever'. And so whatever is in that document coming out of ACFM is ICES advice. But you have, as you pointed out, all these other science groups, you know working groups, or various descriptions that produce reports that say certain things, and you see on the ICES website all sorts of interesting articles about scientific things, which are just things said by scientists that happen to be publicised in some form or other through ICES. That's not what ICES says, that's just interesting work that ICES is involved with, and so it shouldn't really be talked about as 'ICES says'. But of course if you're an outsider and you log on to the ICES website and you see a sign which says 'latest hot topic' and it refers to some interesting science done by a working group, you don't know that that isn't what 'ICES says'. It's on the ICES website!

ICES is a large network that is involved in many events. It also hosts a website where research results are shared. While expert group reports all say, 'The document is a report of an Expert Group under the auspices of the International Council for the Exploration of the Sea and does not necessarily represent the views of the Council', many readers will not know what 'the Council' is or why its views matter.

ICES members have gotten into trouble over much less formal communications than an expert group report. One example is an incident when the Annual Science Conference was held in Scotland. ICES made a strong effort at the time of that meeting to reach out to the public. Part of this was publicising the meeting, and to this end some initial abstracts and titles were distributed. These were abstracts of conference presentations, a long way from peer-reviewed results. However, one abstract about sea lice was submitted from a scientist at Scotland's NFI, the Fisheries Research Service (FRS) in Aberdeen. This abstract led not long after to the BBC pounding on the Minister's door saying, 'this paper says that your policy on sea lice is a load of rubbish!' This, of course, quickly meant that the Minister was pounding on FRS's door. Of course, it is clearly the journalist's responsibility, when reporting on science, to understand the difference between an abstract of a conference presentation and a peer-reviewed finding. It was an ICES meeting, however, and ICES is the official advice provider. This means, for one thing, that ICES must reach out to the general press – and especially to the rather volatile fishing press – rather than deal only with professional science journalists as an academic group of scientists might. It also means that when the press does associate a 'finding' with ICES, however unjustified, it will give that finding increased weight. More serious examples of this problem of the official ICES voice versus ICES as a scientific network have arisen in the very sensitive area of work within cod and climate change. Research has been placed on the ICES web page that in the eyes of many members of the public was inconsistent with what the official ACFM advice has said about cod. For ACFM to include this in their advice would require generally accepted mechanisms for making that link.

How ICES as a large network speaks in the press with one voice is one challenge. However, there are also problems with how the press is handled just within the Secretariat and the Advisory Programme:

> Q 7.23 A Leader in the Advisory Programme: Yeah, there were specific incidences that happened about who takes responsibility for the press for example after the meeting. Where by tradition the Chair of the Advisory Committee will handle the press, but a lot of the press was going to the Secretariat, and it was unclear what the balance was between the two. It especially became an issue when [a Secretariat employee] was no longer participating in the meeting.

Ensuring consistency

Plans and ideas for ensuring consistency contain the same or very similar mechanisms to those described in the later part of Chapter 6 for mobilising knowledge for an EAFM:

> Q 7.24 **Scientist One: How do we ensure the consistency of the advice, that has been a big issue for us**, the documents say the AMG [Advisory Management Group, i.e. the ACOM Chair and Vice Chairs] should ensure consistency, which is quite a task. Scientist Two [A leader in the Advisory Programme]: There is a Secretariat behind them. Scientist One: Yeah, but they are not touching the advice, **how can this be ensured if the advice is not touched by an overarching group?** Also another idea was that the review groups would have outsiders and people will change, we will lose this. **Scientist Two: The review process will give us the final draft of advice, but there will also be a review process focussed explicitly on science** that is part of the timing discussion we go to next. We will have a link between the review group and the benchmark system. **Scientist One: Two different review groups then? Scientist Two: Yes, one for advice and one for methods with different participation.** *(Observer's notes at the Advisory Committee for Fisheries Management meeting, September 2007)*

Benchmark and review groups have a role in ensuring consistency. Indeed, from an organisational viewpoint, consistency and integration are the same, or perhaps more accurately, they lead to the same kinds of organisational structures because consistency is what makes integration possible. In Q 7.18, in response to the interviewer's opening question about what the respondent saw as the most important issues to be addressed by the reorganisation, the delegate emphasised the importance of consistency in the sense of 'one voice'. This quote is what he said next:

> Q 7.25 Delegate: And I think also there was also recognition of the fact that many advisory requests required more than simply fisheries specialists, you know, chemical knowledge or something. But there were many advisory requests which required a sort of multi-disciplinary view. And therefore we needed a system where you didn't simply arbitrarily say: 'Well, we need a fisheries committee or an ecosystem committee and a sort of pollution type committee.' We needed something that allowed that expertise to come together where it was necessary. Interviewer: So while setting up quality control and an ecosystem approach are two different considerations, they're actually all very deeply interlinked. Delegate: Yeah. But you can use the ecosystem approach … I prefer to think of it as more of integrated science, as one of your design criteria.

Other aspects of the EAFM threaten consistency. Most ICES scientists agree that the EAFM must be regionally based. For many, this means that the Advisory Programme must have strong regional organisational components. Many of the proposed review and advice drafting groups are to be regionally based. Yet, a number of scientists are very concerned with losing consistency of the advice because of the regional orientation, and they want to see more cross-regional interaction and structures set up to ensure cross-regional consistency:

> Q 7.26 Scientist One: In Canada we have had to hold meetings on how to do the science in different regions. It is being done differently. How can we ensure that the work done at an NFI level treats the problem the same way? In ACFM we saw how FISHBOAT [a research project] did exactly the same work that an earlier working group had done with no indication that people knew that. We can't continue to do business in the new framework this way; we need the equivalent of methods working groups for these environmental issues. The NFIs will have to operate under a common framework even if they don't want to. This culture already exists in the fisheries side. *(Observer's notes at the Advisory Committee for Ecosystems meeting, September 2007)*

The consistency debate continues and will only intensify in the development of the EAFM. On the one hand Type B consistency is what makes the integration of information possible. Pieces of information must be blended together from different areas and different times, from different units of analysis, disciplinary concepts, and data-gathering procedures. All the little differences that block the comparisons needed to build a picture of the sea have to be beaten into a smooth conceptual consistency.

On the other hand, the questions that the EAFM proposes to address makes one wonder how such consistency can ever be possible:

> Q 7.27 Delegate: To be quite honest how can you be consistent on an advice if the stocks are reduced dramatically one year to another? Look for example what's happening in the North Sea, where one copepod suddenly disappears, to be replaced by another copepod ... but ... the first one was ready to be eaten when the fish spawned and now when the fish are immature there is nothing to eat because they have been replaced by the other one.

Type E consistency and the science boundary

Types A and B consistency are for the most part questions of scientific standards and requirements. In clear contrast, Types C, D and F are mainly questions of advice and are not treated as scientific questions within ICES. Type C consistency involves adherence to legal and political standards. Type D consistency – as in the example of the sea birds – involves organising information to answer a coherent set of questions. Type F is not even really an issue of scientific advice. Having a single official voice is a general question for any organisation, scientific or not, and in this case it is made more complex by ICES's network structure, and the weight of that voice in policy-making, but not because it is dealing with science as such.

It is Type E consistency that most solidly straddles the science boundary. Here, the consistency question illuminates the line between science and advice. The advisory scientists in ICES often talk about Type E consistency as if it were consistency of method, i.e. Type B consistency. But they do this with respect to questions which are similar merely in form. This is not the methodological consistency that is needed to integrate scientific knowledge; this is something entirely different, and equally important, hiding under the same term.

The question the client asked about deepwater redfish and black scabbardfish is in no sense a scientifically coherent question. Science asks for factual evidence about how many stocks of deepwater redfish or black scabbardfish are in the area. The client's question does not address the evidence about how many stocks of deepwater redfish there are, nor does it address the evidence about how many stocks of scabbardfish there are. Rather it is asking a question about ICES as an institution. Did two different groups of scientists, looking at a similar question about two different stocks, approach the question in the same way?

This is the form of a classic scientific reliability test. The reliability of any measure is how it performs under repeated tests. Does it get the same result every time under the same circumstances? The reliability test is being applied to ICES to see how well ICES measures the fish in the sea. One might speculate that the question being in the form of a reliability test may help account for the way it is taken as a question about the quality of science. But the application of reliability testing here is purely metaphorical. ICES is not a Secchi Disk, and deepwater redfish are not black scabbardfish.

The source of the question is not, in fact, in science at all. The source of the question is the notion of fairness about the equal treatment of black scabbardfish fishers and deepwater redfish fishers, transmitted through politicians, ministers, NEAFC, and/or DG MARE. These groups are looking for science to provide the same transparency of argument to each advisory issue, indeed Type E consistency is about increasing transparency by making the scientific methodologies involved more tractable and accessible.

This argument in no way demeans the seriousness of Type E consistency. This is a very serious question, but it is not a question about the quality of science, it is a question about justice and accountability in the legal process of fisheries management as it is expressed in the practice of science. It is a question about justice rooted in a scientific reality: the high degree of uncertainty about the number of deepwater redfish and black scabbardfish stocks. If there was enough evidence to justify the differential treatment of deepwater redfish and black scabbardfish, then that evidence would be how ICES would answer the question, rather than promising quality control mechanisms that would allow it to pass a metaphorical reliability test. This form of consistency is about scientific practice under conditions of uncertainty. The question it raises is if it is ever possible to identify scientific methodologies that achieve this kind of consistency, or can this form of consistency only be approached through procedures for interactive social processes.

7.3.2 Advice and review groups

The only major structural change made by the Council to MCAP's proposal in September 2007 was the separation of the advice drafting groups from the review groups. It is not surprising that the Council focussed its main attention on the review process. Review and quality control are of central concern within ICES.

> Q 7.28 Interviewer: So in terms of Delegates where would the actual impetus [for the restructuring] have been then? Delegate: Yeah. I think most Delegates wanted change, but I think the reasons why they want a change was somewhat different. I think a large number of countries felt that the quality of the advice coming out wasn't good enough.

Quality control in the ICES system is just plain difficult. There was an incident with the forecast for a very important herring stock that happened in the autumn of 2007 when the restructuring debate was at its height. The 'error' was in a technical calculation and was an error only in the sense that the procedure followed was not the one generally followed by ICES. Because the usual scientist was not able to come to the expert group, another scientist did the analysis, and he did it in a slightly different way. Not a mistake in the sense of doing science badly, simply a way that was not consistent with ICES's usual practice. It happened because the expert group was under pressure, the new scientist did not know that the software had already been written to do this analysis, so he wrote his own spreadsheet and made the calculations in a slightly different order. This was not a conceptual mistake, either order could be justified. It was simply not the order that ICES had been using. The change made a very large difference in the result, however – 200,000 tonnes of herring. No question was raised until the results were presented to the Pelagic RAC. Their questions led ICES to investigate, they found the problem, corrected it, and explained to the RAC what happened. This is an excellent story for illustrating why so many scientists, as discussed in Chapters 5 and 6, would like to stop trying to base fisheries management on quantitative forecasts – 200,000 tonnes is a lot of fish to depend on the order of a calculation. This incident was a very public failure of the ICES system in terms of scientific review, taking place right around the time of the reorganisation decision. It was mentioned as an example of the quality control problem by one of the delegates.

Time for reflection, feedback and follow-up is hard to find in ICES's tightly coupled system. One problem ICES is actively trying to address, but one that is very difficult in a large and complex network, is making sure that recommendations made by review groups are implemented. An Advisory Programme Leader said that even when clients have problems with advice to the point where they decide to reject it, for example to turn to STECF instead, there is no formal feedback to ICES, and ICES scientists only find out about it through informal channels.

In the reorganisation debate these problems were addressed in the context of the review process. As outlined in Chapter 4, expert groups on the advisory side are given a set of Terms of Reference (ToRs) based on client requests, in the past by MCAP and now by ACOM, that outline the scientific investigations needed to generate the advice. The ideal model of review that most ICES scientists have is that the expert groups produce a report outlining the scientific, factual findings required by the ToRs. This report is then subjected to a review process that allows it to claim the mantel of 'peer-reviewed' science. When the review process is complete and the facts are verified – 'ICES officially says this is the truth about the world' – then advice is drafted based on those facts – 'ICES officially says this is what you should do about it'.

This is the ideal model but, of course, various boundary issues confound this ideal model. It is difficult for expert and review groups to divorce the facts that need to be identified from the decisions that are to be based on those facts. It is also difficult to distinguish which parts of 'what you should do about it' are scientific judgements and which parts are political ones. When the question is how these processes should be structured and implemented, then all the logistical and practical problems of mobilising a scientific network come into play.

A delegate describes how he saw the debate in the Council:

> Q 7.29 Delegate: The review process was a big issue. The original proposal that MCAP put on the table was that the review process and advice drafting would be in one group. And people were very concerned with the idea that the review of the scientific quality would be coupled with the formulating of the advice. So that I think was the main concern. Interviewer: What are the arguments on both sides? Or on all sides ... Delegate: The argument for having a separate review group is that you can bring in people that are scientific experts who are not necessarily very good in writing, and then you would have a group of people who are more communicators who could take that information and write an advice. That's the one side of the coin, the other is that by having the review groups that we used to have on the ACFM, we have a very layered system with a number of small bottlenecks, so we have a working group that is sort of providing draft advice to the chair, so that's not necessarily a group view that is submitted there. And

then there's a review group that looked at the assessment but also looked at the advice, and that consists of just two or three people, so they had their go at the advice. And then there would be a drafting group on the ACFM and then there was the ACFM. So there were many layers that started fiddling with the text, and any text that you put to a group like that or to a group of scientists will be dissected. You can't just put a text and say, well this is the same text as last year – don't look at it. Because people will say; but I don't understand this sentence.

He highlights two of the key practical issues for advice and review groups. The issue of advisory skills versus reviewing skills was an important part of this discussion, as was the flow of the textual information as it moved from the expert groups through to the final advice. A scientific culture is a questioning culture, and few scientists are shy about raising questions or making changes in the text in front of them, even when such changes are not really the ones being asked for in the particular role they are supposed to be playing. There is a real price to pay in efficiency when the text passes through another group. A leader in the Advisory Programme describes the issue of advisory versus review skills with details that illuminate how hard it can be to get scientists to stick to assigned roles:

Q 7.30 Advisory Programme Leader: The [advisory] committees themselves did not have the expertise. Now what we tried was to take and say, we accept that these three advisory committees are not technical committees, and do not have the necessary strength. Because what happened was that ACFM in particular actually made judgements and said, ok, there are two conflicting views here, we make a choice, and when you analysed it, it came down to the choice of very often highly competent people, but few. There were two people sitting in ACFM who knew what they were talking about, and they would then carry the day because all the rest had to look to whoever it was, and say, 'you know what it is, you are a sensible guy and I will go with you'. But, as an individual, and I've been in that situation a number of times, I would not have an opinion because I don't have a clue... Interviewer: So, the problem was on the level of the review? Advisory Programme Leader: No, it was at the level of the advisory committee who behaved as if it was a technically competent committee. Interviewer: So they behaved as a review committee rather than an advice committee. Advisory Programme Leader: Yeah, precisely ... they didn't have the technical competence to write the review. Interviewer: Once something was reviewed they could write the advice? Advisory Programme Leader: Yeah, yeah... I would never be able to review a paper on the sampling of, let's say, contaminants in the North Sea, it's outside my competence, but I may be able to write an advice and say, ok, the scientific, reviewed information tells me this, I can also read and see there is a lot of debate, uncertainty here, and therefore the information is not particularly strong ... I believe that science has a role here; you can make a science judgement saying that your best guess considering the information is that, but that best guess includes value judgements in technical areas which are outside your competence, and that goes for all of us.

The last part of the quote illuminates one way that advice writing skills are understood. At that stage, technical expertise is no longer a requirement, but a kind of scientific literacy is. The advice writer must be able to make a 'best guess' that 'includes value judgements in technical areas which are outside your competence', but this is still a situation where 'science has a role'. The scientist is the one who possesses the required scientific literacy to make the judgements about the implications of the technical expertise of others. A key part of this scientific literacy is making judgements about degrees of certainty.

An important question in this discussion is that actual meaning of 'review'. Here is an interchange from an ACFM meeting:

> Q 7.31 **Scientist One**: To me a very big problem is that the review is so important; it does not matter where we get the information from as long as it is reviewed properly. I don't see the review group as also a [advice] drafting group, these are two different processes. **You need special people for scientific review, and they are not necessarily the right people for drafting text when you have to know management issues and the regional issues.** **Scientist Two**: The review group name keeps changing in the last year because of this mix of review and advice. It is two ways of reviewing. Review is connoted with research review of papers and so forth. **I am not too sure if the expertise types exclude each other, but we need the people. We may need less technical review and more overall review of quality.** **Scientist One**: I was looking for how much this is more concrete. In many points it is still quite open ... **I am not so worried about the scientific peer review, but someone must have the authority to reject a poor assessment.** *(Observer's notes at the Advisory Committee for Fisheries Management meeting, September 2007)*

Scientist Two contrasts the classical understanding of peer review with another kind, one that 'may need less technical review and more overall review of quality'. This scientist is a leader in the Advisory Programme. He wants the review to ensure, or perhaps *also* ensure, that the product is of sufficient use in the advice. Scientist One's final point is also a very practical one; he is not looking necessarily for the standards of journal peer review, but the gatekeeper function must still be in place. Both of these scientists are very active in the Advisory Programme and sitting in the ACFM. The topic of the meaning of review takes a different tone when an Advisory Programme Leader, the same one who just contrasted advice and review skill in Q 7.30, is sitting among the leaders of the Science Programme. He is Scientist Two:

Q 7.32 Scientist One: My question is, peer review from these review groups, is this ICES or outside, I see peer review in ICES as being incestuous. Scientist Two: We have some money to recruit from the outside. We do not have funds for a full external review, and we try to circulate external reviewers among groups. Scientist One: It is not needed every year but on a periodic basis. Scientist Two: At the moment we have more money than we have reviewers, it is very difficult to get people to come from far without honoraria. Scientist One: It can be someone in the system but not in the current process. Scientist Three: There is not enough difference here between the expert and the advisors. It is like a change to the past, presently the advice is not really in the expert groups, and I don't see why you have to ask the groups to start drafting advice, the experts are the experts, and writing advice is different, and presently the review groups do it. Scientist Two: We have had the practice that the chair provides the first draft [of advice] for 20 years. Scientist Three: Perhaps that is wrong. Scientist Two: The argument has always been that the expert groups are the best place to identify the key features of the advice. The problem with expert groups is not that they don't have a good feel of the features as much as not knowing so much about the context of the advice ... Scientist Four: Your clients are insane. 'I want in 10 minutes a sound paper on the EAFM for the 21st century'. This is difficult to deal with. On the role of experts in advice, I do not agree with Scientist Three. They are the right people to write it, but they are volunteers, and you cannot order them to make the advice. I am not sure if the advisory system, which is a big machine, is not forgetting this limitation. *(Observer's notes at the Consultative Committee meeting, September 2007)*

Here notions of review no longer stem from the practical grind of advice production in ACFM, rather questions about ICES's deviations from the ideal model of review dominate. Reviews should be carried out by external people, otherwise the process is 'incestuous'. Reviews must be clearly distinct from advice, and advice considerations must certainly not apply to the expert groups, even to the extent that the chair of the group writes the initial suggestions, as has long been done. Scientist Two does get some support from Scientist Four, who can see that serious practical issues arise when trying to mobilise ICES expertise to serve the needs of 'insane' clients. However, the 'your' in the sentence 'your clients are insane' may be the most revealing word in the exchange as it shows us how little some parts of even the leadership of the science side of ICES identify with the struggles of the Advisory Programme.

Another consideration in the separation of expert groups from advisory functions is potential bias:

Q 7.33 Scientist One: If the EG [expert groups] draft the advice, it may be influenced strongly by national interests. The backbone of a good advice is the objective and distant view on the assessment, forecast and outcome. EGs are usually blinded by details. An important strength ICES has so far

> is the EG-disconnected discussion in ACFM with a broad range of opinions being presented and turned upside down over and over again. *(Observer's notes at the Management Committee for the Advisory Programme meeting, September 2007)*

This passage reminds us of the very important point that this issue of the separation of review and advice is very much a part of the overall political context in which ICES advice is being produced and used. The final quote in the section delves deeper into this aspect. This delegate constructs potentials for stakeholder involvement as a key aspect of what might be regarded as the very technical and practical question of the dividing line between advice and review.

> Q 7.34 Interviewer: One of the decisions you made was to separate the technical review and the advice drafting. What were the arguments for and against that? Delegate: Well, I think the argument for was primarily to try and reduce the workload of the next step in the process so if you have an expert group which is sort of by definition the experts – if they draft the advice, the next group have got a basis to work from, they are slightly ahead of the game in progressing. The argument against it … has to do with the interaction between stakeholders and scientists – if you have an expert group which is also tasked with drafting advice and you allow observers or stakeholders to be engaged at that step, the stakeholders' focus is more concerned with what comes out in terms of the advice rather than technical details or the assessment. So there would tend to be pressure to produce an assessment which gave the particular advice that you wanted. Where my argument is that if you keep the expert group as an expert group, you can still have your stakeholders there, but you focus purely on the validity of the evidence. Then when you go to the advice drafting stage, again you can have stakeholders there, but now there's a constraint that you have agreed evidence.

This is an interesting twist, indeed in some ways an extension, of the ideal model of review and advice. The quote goes back to peer review's roots in communicative rationality and applies it to the current problem of stakeholder participation. Indeed, the delegate is making use of the communicative rationality-based linkage between scientific credibility and process legitimacy (Section 3.1.3) to design an interaction process to increase the legitimacy of advice. Separating the reviewing of evidence and production of advice becomes a way to facilitate communicative rationality by separating discussions that presuppose a consensus on factual truth from negotiations over values and interests. Clearly, boundary issues between facts and actions based on those facts already haunt the ICES system. This is true even when only scientists are considered. With increasing stakeholder interactions these boundary issues will not go away and will, in fact, intensify. Linking types of participation, however, to these basic distinctions of communicative rationality is not a denial of the importance of boundary issues. Quite the contrary, it is a way to design processes of interaction for handling boundary issues that can increase the saliency and process legitimacy of the advice, while leaving its credibility intact.

7.3.3 Workload and timing of advice

While the quality and consistency of integrated advice may have been the most important conceptual drivers in the restructuring of the Advisory Programme, the following quote from one of the delegates suggests that it was not necessarily the squeakiest wheel:

> Q 7.35 Interviewer: Who were the people that were pushing the change the most from the Council's perspective? Who did they feel like they were responding to? Delegate: I think there was an element of dissatisfaction amongst those engaged in the advice process that it wasn't working, that they were overloaded.

The way scientists within the advisory system feel that they are stretched to the limit is dealt with in some detail in Chapter 5. One of the main issues the restructuring debate was grappling with was the demand from the European Commission and its constituents that advice be delivered earlier in the year. The shift is often referred to as 'frontloading' the TAC setting process. A Commission Representative explains this imperative in terms of the basic political demand of shifting towards a more participatory management system:

> Q 7.36 Commission Representative: The main purpose of moving the advice is to leave time for the decision process for more interaction with the RACs and the Council of Ministers so we don't get the December stamped. This conflicts with stakeholder involvement. You can't set up a participatory process and then ignore it because of timing problems. *(Observer's notes at the ICES Clients meeting, April 2006)*

His point is supported, along with related frustrations, by the following excerpt from a letter from the North Sea RAC to the Commissioner:

> Q 7.37 The RACs consider frontloading to be of vital importance to the success of the RACs in providing strong, scientifically based advice. However, over the past two years, 2004, 2005, frontloading has on the whole remained an aspiration rather than a reality. In concrete terms this has meant that the Commission has not been able to provide adequate advanced information on its proposals to the RACs, or discuss the key elements in such a way that the RACs could provide considered comment. (*RAC letter to Commissioner Borg, 2 February 2006*)

This is a difficult question because the TAC Machine functions on an annual cycle that is rooted in the biology of most fish species and institutionalised in the rhythms of data-gathering. Responding to DG MARE's demand is not simply a matter of shifting the whole system back a few months. It requires processing the information and producing the advice through a more compressed process. Here is a leader of the Advisory Programme reporting on an early experiment with frontloading:

> Q 7.38 Scientist One: For the timing of the advice in fisheries there was a North Sea pilot project. What happened? Scientist Two: This was not impossible, but unhealthy. We reduced the meeting to 8 days from 10, and did it in May instead of September. They did the same amount of work in the 8 days so it was unhealthy. If we task groups to do jobs we have to have realistic expectations. If we want earlier meetings and advice, then we have to remove other jobs from the agenda. We propose shorter meetings earlier in the year, but deal with fewer stocks, and you will not address methodology. The methodology issues will be done in the easier part of the year. *(Observer's notes at the Management Committee for the Advisory Programme meeting, September 2007)*

The group they are referring to here is the same expert group mentioned in Chapter 5 where, when we were observing the group in 2003, the only time one of the scientists wanted us to know something specific for our report was that she had been working until 4:00 a.m. the night before.

This increased pressure leaves little margin for error, so it would be nice to know ahead of time how much error is going to be acceptable:

> Q 7.39 Scientist One: About timing under the new system, the advisory committee makes observations, but they do not do the work if it needs to be redone, but this will leave very little time for reconsideration following the client's needs. Scientist Two: The main thing we can do is planning with clear communication between ICES and clients. We need to know how perfect advice they want. *(Observer's notes at the Advisory Committee for Ecosystems meeting, September 2007)*

It is not just a matter of having a too tightly coupled system for dealing with error, although tight coupling, meaning that the components of a system have a prompt and strong impact on one another, is a dangerous condition for any system. As the North Sea pilot project mentioned in Q 7.38 implied, it is also a matter of the advisory process losing flexibility and creativity as time for reflection and change is squeezed out of the system. Methodological issues cannot be addressed in the expert groups, they will have to be addressed in the 'easier part of the year', meaning the period after the advisory requests for the year have been met.

As discussed in more detail in the section on basic political tensions, the question of time and money resources for the Advisory Programme go beyond ICES to requiring increased input from the NFIs. This is not an easy thing to coordinate:

> Q 7.40 Scientist One: The national labs are supposed to have more responsibility in data compilation, formatting, index calculation and assessment performance, whereas the EGs [expert groups] are considered to have more emphasis on the advice formulation based on the assessments provided by the labs. It needs to be pointed out here that usually data compilation, formatting and assessment performance are done by the same group of persons, who at the same time are members of the EGs and at least some of them being involved also in the RGs [review groups]. ICES is in danger of trying to make use of one and the same resource more than once. *(Observer's notes at the Management Committee for the Advisory Programme meeting, September 2007)*

Coordination is also required with other international advice generation efforts:

> Q 7.41 Advisory Process Leader: The key aspect is the workload. And then the [world] becomes a little bit bigger than ICES of course. And what I've been trying to do, and that's not necessarily connected to this change in the advisory process but more on a general level, is to remove the overlaps between groups that are operating under different umbrellas. So as an example we have removed the separate group on anchovy that was on at STECF. We have now brought that into the ICES system and taken it out of the former expert group that we had in ICES, so that we now have a dedicated group in ICES dealing with anchovy to remove this extra group we had in the past.

Streamlining the advice generation process involves judgements that are political and social as well as scientific. One obvious way to reduce the workload is to be more selective about which fish stocks should be assessed at which frequencies. The following discussion at ACFM shows the complexities of deciding how and by whom these decisions should be made:

> Q 7.42 Scientist One: I worry about criteria used to select the stocks that will get different rhythms. Is it biological, clients, commercial? Will it shift around or not? The small stocks with few data will be left out and the big stocks always done, this is not the criteria. It would be better to decide by long- and short-lived species. Scientist Two: I have a similar concern as Scientist One in choosing stocks. If we need multi-annual advice, how do we start this process? In the transition year the working groups should be working on this specific task of determining the rhythm of the stocks. Scientist Three: What we have tried to do in the last two days we have looked at the expertise and WG chairs, we have here to make an initial

proposal on rhythm. I agree with both of you we need criteria. My initial proposal was based on how it was used in the advice, but I agree we need to consider biological characteristics … Scientist Four: I agree we need to reduce the number of stocks, and the criterion is conservation … Scientist Three: We will provide advice when we have annual information that is useful for providing advice, where we only have catch trends, we don't need to provide a number every year. Commission Observer: Advice cannot be separated from the management procedure for a stock, short-lived and data-poor are specific issues, if there is no basis to updating, for other stocks a change to multi-annual advice must be linked to a multi-annual management plan. *(Observer's notes at the Advisory Committee for Fisheries Management meeting, September 2007)*

Some ACFM scientists are leaning towards criteria that reflect the biology or the condition of the fish stock. Another one agrees but is also concerned with those stocks that are feasible to address in terms of information flows and the mechanism of the advisory system. The Commission Observer's view is that choices about how to assess stocks should be driven by their overall management plan, not *ad hoc* judgements made by concerned scientists.

Much of the debate consisted of finding ways to do more with less, the reorganisation has created the potential for shorter meetings and more work at home:

Q 7.43 Delegate: So you now have the potential to offer a great deal of flexibility and therefore as a lab director it's easier if that happens, it's easier to allocate resources because they're not tied down in quite the same way where you got more choices to do things than in the current arrangements. Interviewer: And travel days there's probably differences. Delegate: Yeah, and certainly if you … You know, if your assessment is done offline, in other words not at a meeting in Copenhagen, you know, it can be done over a longer period, the people involved can still be in the laboratory [than all the way in …], so it saves a bit of trouble and a bit of money.

A lot of emphasis was placed on finding technological means to work at greater distances.

Q 7.44 **Scientist One**: We must do something about the workload on the working groups. So we separate this into two, a) a meeting for the basic advice strategy **and b) video conferences with a text to discuss to look at particular areas and stocks … Scientist Two: Can Russia and others do this? Scientist One: This system we have chosen is that the Secretariat provides the system costs;** the individual countries need the headphones and the camera. **Scientist Three: … The video conference requires a much more specific agenda.** Scientist Four: Will this work over 10 days with 2 hour slots. Scientist Five: We don't know all the implications, but we really have

> to go this way. **Scientist Two: Russia and the US with different time zones and different technology and costs, we can't make solutions that are implicitly discriminatory.** *(Observer's notes at the Management Committee for the Advisory Programme meeting, September 2007)*

The discussion of what to do about the workload contains many warning signs about a too tightly coupled system: separation of action and reflection; little opportunity to identify and respond to errors; and increased reliance on communications media that a) require specialised and centralised expertise, b) demand 'a much more specific agenda' and c) are even 'discriminatory'. Tight coupling results from many decisions that seem to be wise and necessary in and of themselves. One of the points made by many people in this book is, however, that ICES's strength, even its ability to provide advice for an EAFM, rests in the way it is a distributed scientific network that has managed to find many ways for its members to effectively raise questions. The danger is that the rationalisation of working practices, born of a real need for efficiency, may interfere with the communicative rationality on which ICES more fundamentally depends.

7.3.4 The respective roles of the Secretariat and ACOM

The General Secretary, through the Secretariat, is responsible for the organisation of both the Science and Advisory Programmes and ICES communications. The Advisory Programme is not supposed to be concerned with the content of the advice, but is responsible for technical support including organising its production and ensuring standards of quality and consistency. The Advisory Programme employs ten people within the Secretariat. Its traditional tasks include handling the press and clients. While there may be informal interaction between a client and the chair of an advisory committee, as soon as there are any formal issues, the Secretariat must become involved. Its heaviest work, however, is in the area of quality assurance. A respondent who works in the Secretariat provides the following description:

> Q 7.45 Respondent: We check to see if a document we get from an advisory committee is a lousy document, technically, and what I, we are checking again for is consistency, and we're checking for language. Interviewer: So it is a natural scientific review process? Respondent: It is … I'm just taking the trivial example to make the point, the actual advice number in a fisheries document appears I think in 5 different places, and we find examples every year where the number in one place is not the same number they cited in the other … we have five professional staff who each have their block if you like. It [the advice] is about 1600 pages a year … What really costs is, you get a document which was discussed in ACFM, and then the discussion stopped, and then it says 'Tim update' or whatever. This is high-

> lighted in yellow, and then you get a paragraph with no author, and you start speculating if this is the old text or the new … Then you read through the document, and if you find, let's say, nonsense … to give you an idea, I have heard, to do a little bit of study on the email conversations after the ACFM, and I think there was about 350 emails exchanged after the meeting.

This is basically an editorial function, but the amount of work and the number of judgements required make it a kind of 'natural scientific review process'. These were my words that the respondent agreed to, not the respondent's own words, and the example that followed immediately made it clear that what he had in mind when he agreed did not approach the 'peer review' level in the meaning of 'review'. However, it is clear that this is a very extended, lengthy and difficult scrutiny. Considerable discussion focussed on the relationship between the Secretariat and the new ACOM. This was not a new issue. As is illustrated in Q 7.49, the relationship between the three advisory committees, which had the final say over the content of the advice, and the Secretariat, which was responsible for the organisation of the production of the advice, has often involved tensions over day-to-day responsibilities. The Secretariat reports to the General Secretary. Under the old system the advisory committee chairs reported to MCAP, and under the new system the ACOM chair reports to the Council. However, one of these chairs told me, 'the relationship between the MCAP chair and myself had very few interactions, usually only around meetings and not on a day-to-day basis, [while] the interactions with the Head [head of the Advisory Programme in the Secretariat] were much more often, at least ten times more'.

With the reorganisation, the ACOM becomes a more independent body than the previous advisory committees. It combines the powers of the three former advisory committees and MCAP, and reports directly to the Council. Its status was laid out as follows at its first meeting in February 2008:

> Q 7.46 Document 31 as it has been adopted by the Bureau: 1. Empower the Advisory Committee (ACOM) – ACOM should be the sole competent body for ICES for scientific advice in support of the management of coastal and ocean resources and ecosystems. It should be empowered to design strategies and processes for preparation of advice, manage advisory processes, and create and deliver advice, subject to direction from the Council (including the criteria of version 3, which should hereafter be referred to as operating principles for ICES advice), budget constraints, availability of scientific expertise from member states, and practical constraints on the resources of the Secretariat. The content of scientific advice should be solely ACOM's responsibility not subject to modification by any other ICES entity. (*Quote from background Document 31, Advisory Committee, Februrary 2008*)

Several members of MCAP talked about the new division of labour between the Head and the ACOM chair in terms of the ACOM chair being the face of the advice to the outside world, while the Head addressed human resources and logistical organisation. But this was still unclear, and some believed that even greater unclarity existed in the division of labour between the Head and the three vice-chairs because they dealt more directly with organisational issues than the new ACOM chair is expected to do. The discussions at ACOM itself, as well as remarks in interviews, suggested that many issues about the respective roles of the ACOM chair and the Head would not be worked out until the ACOM chair was selected. Aside from roles, to the consternation of several MCAP leaders, people on the Council even questioned if the ACOM chair really had to be located at the Secretariat, or could he or she operate just as well from one of the NFIs. Their reaction was that the ACOM chair must be independent and that independence could only be achieved by attachment to the Secretariat. This lead to a rather impassioned speech by someone from the Secretariat to MCAP:

> Q 7.47 **Scientist One**: I was surprised there was so much dissent on the way this new position should be. We have to make sure this person is independent and highly qualified. **To make sure this person is independent, they need a good basis, and this cannot be in a single institute, it must be in the Secretariat.** This is the executive of the Council and Bureau and has full control. This is where it can be fully independent. So the full-time professionals must be in the Secretariat ... There is continual cross-funding of ICES's core budget and advisory budget, this is fair enough as it is the 'core business', but to organise that **we need a person who is at the Secretariat management group meeting every week while being independent and accountable to Council ... the person should also be paid by ICES not an institute, the person should be in the Secretariat day by day, or it will just go back to the Head of the Advisory Programme** [executive responsible for the Advisory Programme within the Secretariat] and we will have two parallel systems. *(Observer's notes at the Management Committee for the Advisory Programme meeting, September 2007)*

However, the employee of the Advisory Programme within the Secretariat that provided the description in Q 7.45 believed that the division of labour was clearer than many other scientists seemed to think:

> Q 7.48 Interviewer: What's your take on issues around ACOM versus Secretariat roles? Respondent: I think it's grossly overemphasised. To me a Secretariat has to operate within defined policies, and these policies are laid down by the Council in general, by the ACOM on more specific issues, and my take on it is that simply because the way they are nominated, let's say all executive things are through the Secretariat. Everything for which there is a consistency issue has to be done through the Secretariat, and a

> number of cases actually have to be done by – or checked by – the Secretariat because the, what we see, we see it from our, every time we change a chair there is no history ... The other thing is that I believe that in order for the system to work efficiently, any advisory group has to consider policies and see that the Secretariat adheres to the policies, but accepts that the executive branch is the Secretariat, not the committee itself.

Another respondent who is a leader in the Advisory Programme, but not a Secretariat employee, and who was particularly active in ACFM, sees the division as less clear:

> Q 7.49 Interviewer: How does the new system envision the relationship between ACOM and the Secretariat? Advisory Programme Leader: There will be a more active steering from ACOM on the direction of the advisory process and a stronger link between ACOM and the Secretariat through the Chair and Vice Chairs process of the Advisory Committee ... The interesting part of it is where is going to be the boundary between what is the policy of ACOM and what is the technical implementation of that policy in the Secretariat. And the idea is that [with the new Chair and Vice Chairs] there is a more professional basis to work on the policy of the Advisory Committee and to better steer the Secretariat approaches ... but ... the line between technical support and policy-making is a very difficult line to draw, and there has been a perception with some people, for example in ACFM, that the Secretariat was doing too much steering on content rather than on the logistics ... Interviewer: When does it arise? Advisory Programme Leader: It is not that much; well, it used to be quite regular in those days when the 'professional Secretariat' participated in the advisory meeting as the Secretariat by helping out on the formulation issues and things like that. There is a subtle balance there on how much input the Secretariat gives at that stage, and we had quite a number of incidences [three years ago] which I thought were inappropriate uses of the Secretariat in the meeting by sitting down, saying we did not like the proposal from ACFM and then writing something that they thought was better. Interviewer: Was the concern driven by thinking that they did not meet the requirements of clients? Advisory Programme Leader: Yeah, well, no I can't say that explicitly, they didn't think it was consistent, or they didn't like the way it was formulated.

He argues in contrast to Q 7.48 that 'the line between technical support and policy-making is a very difficult line to draw' and believes that it has created tensions around the degree to which the Secretariat should involve itself in the content of the advice. An Advisory Programme employee in the Secretariat thinks it would be helpful here to make a distinction between the ACOM chair and the vice-chairs, because as the chairs have an 'executive role', here in the sense of practical and logistical rather than decision-making, they should also be accountable to the Secretariat:

> Q 7.50 Interviewer: What has the debate been over who the ACOM Chair should report to? Advisory Programme Employee: There has never been a debate on the Chair. Interviewer: OK, he reports to the Council? Advisory Programme Employee: Yes, full stop, no debate. My take on it is that there is an issue on the Vice Chairs because they act as executives, and that is a major problem to me, that they actually go as far as starting discussing how the Share Point [a computer communications system located in and managed by the Secretariat] should be, again just to give you an example of the problem. We have established a Share Point system and so on, and it has a certain number of deficiencies, there is one particular feature which the chair wants to use, there is a number of arguments, technical, and I have forgotten them, against using it, the key point is that if you use that facility, you gain here but you lose other facilities, and the interim evaluation in the Secretariat was the gain did not outweigh the loss. From our perspective we used the other facility which will lose much more often than the first one. The [Vice] Chair, ok, can argue, but he tries to implement it himself, and that's normal. It's a trivial example, but it's just illustrative.

The tensions between the Secretariat and the ACOM system must be worked out through a series of small day-to-day decisions about how the computers will work. The most serious tension, of course, is over what counts as a decision about the content of the advice, which is now 'solely ACOM's responsibility not subject to modification by any other ICES entity' (Q 7.46) and what counts as simply an editorial decision as the Secretariat prepares 1600 pages of consistent advice in the week or so following ACOM meetings. These are all people who are used to playing the role of edited author, and most of them will have experience as editors as well, so they will muddle through while they exchange their 350 emails. However, this muddling through takes place within the broader ICES political landscape. There the ACOM-Secretariat tension reflects the larger divisions between the CFP members in ICES and those who negotiate with them.

The two quotes below come from interviews with delegates. The first one is a delegate from a member state within the CFP.

> Q 7.51 [continues from Q 7.13: … ICES as a bastion against the EU-bloc …] and where that has come to the fore in this ACOM-thing is in the status of the Chair. Now, Norway has argued that the chair and the senior advisors should be employed within the Secretariat. The other position, and it's true in a number of countries including North America, is that if you do that, you detach the responsibility of the advisors from the scientific community producing the evidence, because now you have a reporting line through essentially a bureaucrat. You know the General Secretary is now responsible for these employees who are bringing advice to customers. And so it's a bit like the prime minister reporting to the head of the civil service, which is not the right balance of responsibilities and … But Norway's reason for doing that is that they're fearful that if you create these positions outside

> the Secretariat and they are funded, that somehow that organisation becomes, you know the ACOM-organisation becomes a parallel organisation to ICES.

In contrast, the views of a delegate from a country external to the CFP:

> Q 7.52 Interviewer: How is the Council envisioning the relationship between ACOM and the Secretariat? Delegate: Well, that's interesting, because I do not like the present model. Why? Because as it is, it could reflect the way you look upon an organisation, and it definitely reflects different national cultures. And I've been disagreeing very much with [other Delegates] on the way we decided it. You have an ACOM that should do the advice. I would strengthen the Secretariat to develop the necessary data to make the expert groups work, to make the review groups work, and to compile and to finish the advice. That's something that the Secretariat should do. Some of my colleagues at the Council want the Secretariat to be only a technical meeting facilitator. What I see is that we are now building a parallel structure to the ICES Secretariat by developing ACOM and ACOM Chair and three Vice Chairs ... that would definitely establish an alternative Secretariat.

At issue is partly the question of what is the 'real' ICES. One part of this identity question is between ICES as a network or ICES as the Secretariat. On the one hand there are the 100+ expert groups who provide the intellectual currency in which ICES trades. The other is a building in Copenhagen where these expert groups often meet and where the only people who see themselves as actually 'working for ICES' are sitting. It is important to remember that ICES in legal fact is neither the network nor the Secretariat. It is an intergovernmental convention governed by a Council of Delegates of its constituent member countries. These delegates are the ones that hold the purse strings for both the network and Secretariat activities.

The first delegate sees the ACOM chair as a sort of prime minister. This is an unusual metaphor when you think about it. Both the ACOM chair and the General Secretary are employees of the Council. He is concerned that the ACOM chair being part of the Secretariat would 'detach the responsibility of the advisors from the scientific community producing the evidence', so both this concern and the use of the term 'prime minster' place an emphasis on how the ACOM 'represents' the broader ICES network that gives the science its legitimacy in a way the Secretariat does not.

This emphasis is rooted, however, in a formal notion of representation. ACOM is, like the Council itself, made up of two representatives of each member country. This was an important point emphasised within MCAP and other Advisory Programme meetings while they developed the reorganisation proposal. The bulk of the review and advice writing should take place in review and advice-drafting groups lower down, and then ACOM, where representatives of the member countries sit, will give this advice the international stamp of approval. So ACOM is indeed representative, but of the member countries first and of the 'scientific community producing the evidence' second. Indeed, while most scientists now appointed by their governments to ACOM have been active members of ICES, several have not been much involved in ICES activities before.

The first delegate's characterisation of the concern of those outside the CFP is that they fear that the network, led by ACOM, will develop its own organisation and even gain control of a budget. This seems accurate, given that the second delegate fears the eventual 'outsourcing' of the advice. He knows that the ACOM will need some sort of Secretariat, so the efficient thing to do is to strengthen the one ICES has. If not, he fears another parallel Secretariat will emerge, but it is hard to see where the resources for this would come from.

Other voices also have concerns about the degree to which the Secretariat embodies the ICES network. In this quote, from an Advisory Programme Leader not employed in the Secretariat, the concern is with the fact that the Secretariat takes its character partly from its location:

> Q 7.53 Interviewer: So why was the role of the Secretariat an issue when you did the reorganisation? Advisory Programme Leader: Well, it's the issue of what is the process from an international community that is going to give internationally agreed advice. You have a Secretariat which is somewhat dominated by Denmark, based in Denmark, with professionals that have been working there for many years and may have their own ideas about how the advice should be ....Interviewer: So this has to do with what the actual authoritative voice of ICES is? Advisory Programme Leader: Well, yes. Ultimately it is about who is providing advice. Is that an international committee that is embedded in the ICES system, or is it also in part run by the professional Secretariat that may have their own inputs?

This quote is an interesting contrast with Q 7.47, where the Secretariat was held up as a guarantor of independence. Here the Secretariat is seen as rooted in a particular national culture. Perceptions play an important role in both quotes. Denmark has no formal influence beyond that of any other ICES member. However, the Secretariat is in Denmark and employs many more Danes than any other nationality. The ICES Secretariat employs 48 people including the three ACOM vice-chairs. Of them, 15 of the 28 administrative and technical employees and 8 of the 20 scientists and managers are Danish. Many different kinds of independence contribute to perceptions of legitimacy.

The underlying concerns are with scientific legitimacy as I have defined it in Chapter 3. Is the scientific institution sufficiently balanced in its make-up to be trusted as a source of advice?

7.3.5 Stakeholder participants and observers

Questions about when and how stakeholders should act as observers, and even in some cases participants, in ICES deliberations was an ongoing theme in the reorganisation discussion. Many ICES scientists saw this theme as something extraneous that was being 'bolted on' to the reorganisation. Others saw it as a critical part of the discussion. This respondent tied the question to the EAFM:

> Q 7.54 Interviewer: How is concern about relating to the RACs an important or less important aspect of the reorganisation? Advisory Programme Leader: Well ... I think there have been a lot of mixed views on that. And to me it has been a very important aspect. The issue of transparency of the advisory process to me is a very important issue. And that links into the RACs, but it can be wider than the stakeholder organisations. It's not a necessary part of the changes in the advisory process. In my view I've sort of coupled it in with this, but I see also people who have worries about making the scientific system more open, being afraid of influence, etc. And I think the way I see this ecosystem approach is that it's not only about including ecosystem aspects in the way management is carried out, it also involves a different process of how the management is carried out. So I see the involvement of stakeholders in the process as part of an ecosystem approach.

Concerns about the impact of the stakeholder observers on scientific legitimacy were widespread in the discussion. The concerns articulated in the following statement, and the use of different steps in the process to balance those concerns, is quite typical:

> Q 7.55 Scientist One: The other thing is the proposal in documents for stakeholders to be participants in review and expert groups. I am not against stakeholder communication before the assessment in terms of the data we are using, such as data from the fishing industry, also after the assessment in review groups this will be productive to communicate well. But in the assessment groups producing the draft advice, we know very well that the stakeholders have a strong economic interest, so we may then question if they will influence the assessment in the early advisory process, that will not be good, and it will question the integrity of ICES, and that is important. In some meetings the scientists should be able to sit and do assessments without the stakeholders pushing. *(Observer's notes at the Advisory Committee for Fisheries Management meeting, September 2007)*

The CFP/non-CFP distinction also played a role:

> Q 7.56 Interviewer: Where among the Council did you note any tensions arising in the reorganisation between EU countries and non-EU countries?

> Delegate: There are two issues that have come to light where there is a difference of opinion, and one is the engagement with stakeholders – how much of the system is open and where are the points that stakeholders can get involved.

The idea that stakeholder participation in science should be regulated according to the steps in the science and advice development processes is a near-consensus at ICES; disagreement focuses on how to handle particular steps. In the following, the reference to 'reasons I have described earlier' is to the delegate's belief that the biggest problem with observers would arise in meetings taking place remotely, over the Internet for example, because scientists would not be able to see the interactions between the other scientists and the stakeholders from their country.

> Q 7.57 Delegate: I do not want them in the advisory drafting groups, for the reasons I described earlier, [another Delegate] wants them there. Interviewer: But you wouldn't object if they, if they're in the same room? Delegate: No, that's quite fine with me. Interviewer: So the issue is the structure, not the observers themselves? Delegate: Yeah. Interviewer: OK. Delegate: My ministry strongly disagrees with me. They want them out of the advice drafting groups. Interviewer: Why's that? Delegate: Fear of influence. Interviewer: But this is [the Delegate's country]? They bring their fishermen's associations with them when they go to the negotiations. Delegate: Yeah. Interviewer: So, what do they fear? Delegate: Do not ask me.

The delegate's ministry is very likely concerned that those observers will influence the advice, and they do not want the CFP industry to have influence at an earlier stage than their own fishing industry. The delegate himself, however, is actually concerned with structuring the stakeholder participation in a way that increases rather than threatens transparency.

The CFP/non-CFP divide does not explain all of the differences among countries in terms of observers.

> Q 7.58 Delegate: The countries that are most in favour of stakeholder/observer participation are EU, sorry not EU, UK, Netherlands, Denmark ... I don't know about Ireland, but United States and to some extent Canada. Those who tend to be nervous are Russia, Norway, Iceland, Spain, Belgium ... France tends not to express an opinion ... I think with that particular issue, you know, whether it's a participatory role or an observership role – I don't think that cleavage comes as an EU/non-EU. I think that has a lot to do with self-confidence, national self-confidence if you like. I think that some countries are further down the line of understanding that if you don't involve stakeholders, you might get technically good advice, but which is not accepted and therefore is of no use. While other countries feel that scientific integrity overrides everything, and therefore you must exclude all undue influence from non-scientists.

The overall discussion in ICES has reduced transparency to mainly a question of the presence or absence of observers. However, observers are only one technique to increase transparency. Observer programmes are clearly delicate. The paradoxes of transparency make formal attempts to ensure transparency complex. When techniques to increase transparency are not sufficiently transparent, the situation is worse than before. So if we are to have observers, the process of having observers must itself be observed. These ICES scientists place this problem in the context of countries observing countries. Just as critical, however, the legitimacy of the process depends on both the industry and the conservation NGOs being able to observe each other observing.

We also have to look beyond the question of the forms and structures of transparency to the quality of the processes. This last quote (Q 7.58) calls our attention to transparency as a function of capacity-building and scientific confidence. This confidence includes a willingness to be seen being unsure.

7.3.6 The EAFM and the reorganisation

As mentioned above, the issues that the informal delegates' meeting outlined in Helsinki did not place much emphasis on the EAFM. In fact, the EAFM did not play a very large direct role, although it did play an important indirect role, as evidenced in the discussion of these other themes, making up an important part of the context of the required changes. Different ICES scientists also had different perceptions of how much of a role the EAFM was playing. In the Council, in the perception of this delegate, the EAFM was more a gloss than part of the substance of the reorganisation, a gloss that was needed more by some countries than by others:

> Q 7.59 Delegate: … And I think some member countries are probably more concerned about being seen to be doing an ecosystem approach than others because their local politics demands it … Interviewer: In terms of trying to figure out advice for the ecosystem approach, how much does the Council tackle issues like what integrated science would mean? Is that something you guys would discuss? Delegate: No [the Council] tends not to deal with that sort of technical level at the Council meeting. … there was an agreement that the current advisory system wasn't working as well as we wanted it to. There were some countries who said, well in that case, you know, what should inform any reorganisation is the ecosystem approach. So … I'm sure if you asked some countries 'Why did you want to change the advisory system?' they would say 'In order to get an ecosystem approach'.

One interesting point that emerges from this quote is that the whole question of integrated science, i.e. the entire discussion outlined in Chapter 6, is not something that this delegate sees as relevant to the Council's work.

Here it is constructed as a purely technical question, not rising to the Council level. I think it safe to presume that this attitude is not shared by the entire Council, because of the frustration that the other delegate, quoted below in Q 7.64, feels about how ICES is moving towards integrated advice.

The EAFM is also a different priority for different clients:

> Q 7.60 Interviewer: How much of the change towards the ecosystem, in the parts of the reorganisation you were involved in, would you say the clients played? Advisory Programme Leader: I think there's a considerable influence from the clients. Interviewer: Which ones? Advisory Programme Leader: Particularly from the European Commission. They have a very strong driver for getting advice on a more integrated basis. The other commissions like NEAFC, NASCO, they sort of seem to be more like: Yes, we [all] want this ecosystem approach, but we don't really care too much about it. And in the Commission there are a number of people quite motivated to move in this direction. And the [MoU] between the EC and ICES is also much more explicit.

This perception that DG MARE is particularly interested in the EAFM is reinforced in the following exchange:

> Q 7.61 Scientist One: Another aspect comes from the advice for the corals we had to do this year. When dealing with non-fish quota, we have a different science base, if we can't quantify things it does not help people do their business very well. Demands like that are going to grow. Scientist Two: We can see that in the requests coming in, not just coral but climate changes and fish and ecosystem. Scientist One: We are not being nearly as helpful as we can be … Commission Observer: There is a difference between evaluating what has been done and what is asked for here, which is how to have a forward role of implementation. As a client, I see a high pressure for sound proposals on the table. We need to see this get to the advice level, as Scientist One has implied this is actually the easy part. Once we get into aspects we are less used to, it will be even worse. Politically, we must get our tactics into strategies, and we need advice for that. *(Observer's notes at the Advisory Committee for Fisheries Management meeting, September 2007)*

This discussion rehearses the difficulties in producing advice for EAFM and suggests that DG MARE at least is frustrated at the level of linking the EAFM advice into management strategies.

The following reflects a background emphasis on regionalisation as a way to approach the reorganisation that many ICES scientists shared. Indeed, recall the Advisory Programme Leader who, at the beginning of this chapter (p. 172), called the EAFM the rationale for the new historical wave towards holism due to ecosystem considerations.

> Q 7.62 Interviewer: So what kind of more change would you like to see? Delegate: I think that we have now changed the advisory process, we have decided that we should adopt the ecosystem-based approach, but still, we have the thematic expert groups, and then we have clients or customers, use whatever word you like, who ask for advice on a single-stock basis. So, what I think we should do is that we should have in a way regionalised the ICES advisory process, not the science process. I think the science process needs to be thematic, but the advisory process should be organised in a kind of regionalised way, that in a way causes the process itself to use a broader, more ecosystem-based approach. Interviewer: What kind of regions would you be looking at? Delegate: I would prefer the LME [Large Marine Ecosystem] level.

Some scientists also saw the combination of the three advisory committees into the one ACOM as a way of forcing an EAFM agenda:

> Q 7.63 Scientist One: As long as ACE and ACME existed, if we got a non-fisheries advice request, they had another place to send it, this was because they could not get them into the fish groups. From many years I know that when fish stock assessment working groups were assigned things that did not include SSB and F, these things did not get done. Now they have no other place to send them. Now it will be sent to people who do fisheries advice, and we will put people in the room who know about the bycatch species – like birds for example – when you have a client that is mandated to protect these species. If we give catch option advice ignoring other requests to ICES, that catch option advice is irresponsible advice, not merely not integrated. *(Observer's notes at the Annual Meeting of Assessment Working Group Chairs, February 2008)*

It is an interesting twist in this statement that the speaker is thinking of advice to clients based on what those clients are 'mandated' to do, rather than more directly in terms of the client's specific requests. The question of the EAFM and client requests also appears in the next quote. One of the interviewed delegates, the same one who considers regionalisation so important in quote Q 7.62, is not so optimistic about how effective a regional approach can be because client requests arise from the TAC Machine:

> Q 7.64 Interviewer: I'm interested in a point you made a little while ago, where you said that you should have worked more on ecosystem approach, but ... the rationale for ACOM is to bring the three advisory committees together so that it becomes more integrated. So, how was that a failure? Delegate: Unless you change the process, the underlying process, which means [the way] you compose the actual working groups that work out the data to do the advice ...Interviewer: I was hoping they would be doing that on a regional level, like you've described. Delegate: No, but they won't ... it has never been really discussed. The advice groups will meet to compose

> advice about some kind of request ... they will produce advice for the North Sea herring, the Norwegian pout ... etc. ... because that's what they are being asked for.

It is important to note here that the reorganisation adopted by the Council allows, but does not mandate, the organisation of review and advice groups by regions. It is ACOM that will determine how regionalised ICES's work will become, and there are real disagreements over the desirability of this because of consistency issues, as discussed above in the part of Section 7.3.1 that discusses 'ensuring consistency'.

A basic question this delegate raises, however, concerns the group level where integration takes place. This question is linked to regionalisation because ACOM will decide the degree to which expert groups, review groups and advice drafting groups are tied to regions. This question was the most common way in which integrated advice and the EAFM arose within the reorganisation debate. It is also a question that some scientists feel has been left hanging:

> Q 7.65 Scientist One: It seems like the integration stuff is always so vague, and the responsibility seems to fall on the assessment working groups. *(Observer's notes at the Annual Meeting of Assessment Working Group Chairs, February 2008)*

The group level at which integration will take place is critical. Many ICES scientists believe that real integration will never happen unless it takes place before the advice drafting group level and certainly before ACOM. Given the breadth of ACOM's mandate, there is little additional expertise available there for advice drafting, their job is to make sure that the advice is fit for its purpose. Even at the level below ACOM, if an advice drafting group finds itself looking at nothing but a set of distinct, independent assessments of different issues within some region, then they will be badly handicapped in trying to produce integrated advice, especially in the few days they have available to them. In the terms of the next quote, they will not have time to integrate advice; they will only have time to concatenate it. For many ICES scientists, integrating information for EAFM advice has to begin at as low a level as possible. One ICES leader explained it this way:

> Q 7.66 What the EAFM does is make one discuss a lot of different issues, almost all of which would be discussed anyway somewhere in ICES, but discuss them in one place and relative to each other. It is the people who can talk to all the different specialists and see how the individual parts interrelate to each other that are in short supply. That breadth of perspective has to be present in the review process, because if advice is to be integrated, then expert group reports need to be reviewed in two ways. One is relative to the specialised disciplinary content, which is the traditional and easier part of the review. The other is when this expert product is to be linked

> with others, the reviewers have to ask questions about how correct it would be to interpret and apply the results in ways that might not have been on the minds of the specialists who created the work being reviewed. And of course, the drafting groups have to have those people with the breadth of experience to understand several different sets of specialist results well enough to frame decent integrative views of them, and they tie the pieces together in a whole. Otherwise you don't get 'integrated advice', you get 'concatenated advice'.

Indeed, this ICES leader gave this as the most important reason why he supported keeping the advice and review groups together. Given the limited number of people who are able to handle the breadth of the specific issues that arise with respect to the EAFM, ICES cannot expect to be able to move advice along four separate levels and have it be truly integrated. Integrated advice should begin at the expert group level, and then be reviewed and carried forward to advice by the limited expertise that is actually available, and which is unlikely to be able to staff both a review and an advice drafting group.

7.4 Conclusion

Two more quotes provide illustrations of what I think are the main conclusions that can be drawn from this discussion of the restructuring of ICES. The first asks and answers a very interesting and basic question:

> Q 7.67 Scientist One: Why do we come to ICES? If we had a choice, would we choose ICES? Scientist Two: Here we sit in a big community and we are happy, then we are home and have another reality? How important is it? Scientist Three: It is important that the programmes come in and are more integrated and cross-disciplinary, ecosystem-based, this is why we want ICES. Scientist Two: It can do things we can't do alone ... Scientist Four: One argument is that it is more than just Europe. Scientist Five: We do need ICES, but we need more people, and we are being pulled into a number of organisations, ICES has to work more efficiently with other organisations. *(Observer's notes at the Resource Management Committee meeting, September 2007)*

These scientists did not reflect at all on ICES as an intergovernmental provider of official advice. They know that is why it is there – it is the reason why they do not 'have a choice' – but it is not their motivation. ICES provides a network that makes it possible for these scientists to achieve their goals, particularly with respect to the EAFM. Yet, their desires for ICES reflect some of its contradictions. They want the network, but they want it to work more efficiently, and it is under-resourced. They see in the ICES system a source of some of the frustrations described in Chapter 5. The next quote also addresses the question of the network, but it is the voice of someone who is directly responsible for the intergovernmental aspects of ICES:

> Q 7.68 Delegate: In my opinion, it's the distributed networks that gives ICES the authority to speak. Interviewer: OK. Delegate: It is as simple as that, because what you get is that you get every scientist to promote their views, their results, their findings, and then they have a kind of scientific consensus process that automatically feeds in the authority for ICES to speak. Because everybody has been participating, everybody has the opportunity to say their ideas, and, if there is agreement it is allowed to show it ... And the strength is they are very distributed ... In my opinion, ICES authority is based on the scientific activity in its distributed groups. If it had not been for the number of distributed groups, you wouldn't have the authority, because then there would have been the possibility that many would rest on the outside saying: Hey, you didn't hear me!

This delegate is arguing that the network is the source of ICES's authority because it makes criticism of results possible. No one is silenced, and this is what makes the authority of ICES's voice so strong. The network provides a social context that allows scientific process legitimacy in the sense described in Section 3.1.3. It allows both the scientific credibility and the 'something more' (3.1.3) that gives the credibility of the scientific process legitimacy in the eyes of its beholders.

He is describing something that is a bit contradictory: a distributed network that speaks with one voice! Yet, I think there is an important insight here, even if it seems paradoxical. What he is describing is what ICES, which often seems to have two faces, actually does do. This paradoxical structure has some great gifts, as will be discussed in the final chapter when I try to pull this case together.

However, it often seems to mean that ICES just continues in a state of constant reform. The final decision made by the Council forced ACOM to separate the advice and review groups, but left other critical decisions in ACOM's hands. These include the ToRs of the expert groups, which will in turn determine how low in the group hierarchy integration begins to be considered, and the degree to which the review and drafting groups will reflect ecosystem regions. The creation of ACOM is certainly one step towards organising the EAFM, but it cannot be said that any of the main issues are really resolved at this point. They are in the hands of the new structure.

8 Conclusion

8.1 A case of adaptive learning

In spite of the fact that the system of marine management that ICES feeds into has not been a successful one, I believe that this case study is a substantially positive example, and that ICES has lessons to offer to other institutions seeking to produce knowledge that can support an ecosystem approach. Of course, I cannot claim that ICES has been 'successful' in a general sense because the EAFM is only really beginning in earnest, and ICES plays one set of roles among many in determining policy outcomes. However, I do think that ICES has approached the EAFM in a way that is an example of 'double-loop' learning at an impressive magnitude. Double-loop learning is the idea that institutions not only learn and change in response to that learning, but that they also learn about how they learn. ICES has attempted to mobilise a vast array of expertise to meet a complex problem. For an intergovernmental organisation of this size and complexity, it has made an impressive start.

What is it about ICES that has made this adaptive learning possible? That seems to me to be the question around which to fashion a concluding chapter. Asking the question this way is not meant to ignore ICES's problems and constraints. It is to do quite the opposite. The fact that ICES is part of a bureaucratic management system which has often made the lives of its scientists miserable while failing to sustain fish stocks is precisely what makes the question interesting.

My response to this question can be summarised this way:

a. the ICES network contains a number of 'creative tensions' that provide space for ongoing reflection;
b. these creative tensions are very costly from the perspective of a large-scale bureaucratic system attempting to manage fisheries, and finally an EAFM, so there is a lot of pressure to 'resolve' them by suppressing or ignoring them;
c. the ICES network, however, is made up of so many distributed power centres in constant negotiation that the creative tensions are very difficult to suppress; and
d. out of these constant negotiations new mechanisms for transparency are being developed that are subject to the paradoxes of transparency

and require tightly drawn social spaces and associated languages to resolve the paradoxes.

This concluding chapter describes these four points. After doing so, it turns to a short reflection on the usefulness of the CST theoretical perspective. Then the final discussion focuses on the implications of the lessons from this case study for the institutions that evaluate, select and maintain policy responses within an EAFM. The central recommendation is to structure institutions generating and using knowledge for an EAFM by ordering them according to larger and smaller communicative scales, based on the complexity of the mechanisms needed to ensure transparency. I briefly describe this in terms of a results-based EAFM approach.

8.2 ICES's creative tensions

The major form taken by the creative tensions within ICES is the contrast between the Advisory Programme and Science Programme cultures. For a number of reasons, this contrast should not be overdrawn. For one thing all ICES scientists draw on a general scientific culture that is much more pervasive and stronger than any sub-culture within ICES. The two scientific ideologies that have repeatedly arisen in this case study – the EAFM and the precautionary principle – vary somewhat in levels of commitment (Table 6.1), but they are still very generally accepted. Furthermore, for any of the tensions I describe below, there are far more than two different positions. In fact, given that these are cultural ideas, and we are talking about scientists here, it is likely that ICES contains 1600 positions on each one. Nevertheless, I am satisfied that the distinction between the cultures of the science and advice sides of ICES is a useful one. It does describe fairly well the main poles of these tensions. Most importantly perhaps, the distinction is not my creation; ICES scientists commonly use it themselves.

8.2.1 Placing the science boundary

One tension between the science and advice sides is contrasting attitudes about the placement of the science boundary. This distinction became very clear in the discussions of how ICES should approach scientific review. 'Review' remains the key word in describing mechanisms for deciding what will and will not be declared science; but ideas about what review means vary.

On the science side, an ideal model of review, derived from the more general notion of 'peer-reviewed science', retains substantial force. Peer review requires trained experts in the subject of the review, and it should be independent, not carried out by anyone who was involved in the work that is being reviewed or otherwise with any sort of stake in the outcome. It should be substantially divorced from the advisory process and focussed only on the factuality of the description of nature on which the advice should be based (Q 7.32).

On the advice side, practical experience with the questions of credibility, legitimacy and saliency of advice has made them much more open to an extended peer community (Funtowicz and Ravetz 1990) understanding of review in the development of advice. Illustrations of how practical issues have this effect is found in discussions of how to explain advice to managers (Q 4.11, Q 4.12 and Q 6.1) and even in suggestions that advice should be made conditional on management actions (Q 4.14). A philosophy has emerged among some advisory scientists, reflecting the idea that developing advice, including reviewing the outcomes of expert groups, should be based as much on general scientific literacy and skills as it is on specific technical expertise. Review should be rooted in experience with advice, rather than in very specific forms of substantive expertise, i.e. 'less technical review and more overall review of quality' (Q 7.31, see also Q 7.30). This is not to say that scientists on the advisory side downgrade the value of technical review or reject the ideal model of review in principle. But there is recognition that it is not enough, and 'the more' that is needed may not fit the ideal model of review or involve the retention of all scientific decisions in the hands of ICES scientists.

The discussion of the skills of review and advice, especially the contrasting of technical expertise with the scientific literacy needed to write advice, provides some insight into the practical meaning of an extended peer community. The emphasis in Q 7.30 is on the ability to distinguish the degree of underlying uncertainty in the results. The extended peer community needs exactly this kind of expertise; judgements about uncertainty require the ability to judge scientific methods and background knowledge about the sources of the data. Judging the degree of uncertainty in a practical sense also requires judgements about the implications of that uncertainty for relating the results to the practices to be regulated.

8.2.2 Scientific activities within ICES

A second creative tension between the advisory and science cultures concerns scientific activities within ICES. This is mainly played out in issues over the role and function of expert groups. A large part of this is simply the degree to which scientists are used to putting their particular interests aside and responding to a specific set of advice-driven ToRs. These are differences about what sorts of questions are really worth the time of a group of scientists when they go to an international meeting to produce some product. These difficulties here are nicely visible in Q 6.29 where the chairman, who was used to working on the advice side, was perfectly ready to ignore the surface temperatures that everyone – himself included – agreed were the key driving factor in the relationships being examined.

Most of the other scientists wanted to work with their ‘pet data sets’ simply because they addressed the most important factor. The chair knew that these temperatures – key factor or not – had no saliency for policy, and the group’s time was better spent on things that actually mattered.

When considering an EAFM, the question of how expert group time will be spent is not trivial. Perhaps the main organisational challenge for ICES in contributing to an EAFM is mobilising expertise that has not previously been part of the mainly fisheries-driven advisory process. The expert group culture on the science side is much more used to doing what is interesting to them, rather than what is interesting to a client. ICES’s leverage over the way ToRs will be emphasised, or even addressed, is limited. The scientists who attend these groups tend to be somewhat more senior than those who attend the assessment groups (Table 5.5), and this also strengthens the ‘we are volunteers at ICES’ attitude.

While the ICES leadership may sometimes feel like they are herding cats, it is not entirely a bad thing that scientists in expert groups follow their own interests. These interests are about understanding ecosystem processes, and the inquiries cannot be limited to results with immediate use to policy setting.

8.2.3 Adequate Science for Advice

Another tension exists around ideas of what constitutes adequate science for advice. This is the tension that I am the least comfortable characterising as being between the advisory and science sides. While one side of the debate, those advocating ‘soft predictability’, tends to be from the advisory side, strong advocates of having quantitative forecasts as an important goal are also found on the advisory side. Managers want quantitative advice because solving the political problems they face starts with having a divisible quantity. This is not at all the same thing as the scientific motivation for quantification, which is about precision for the clear statement of hypotheses and potential replicability. Nevertheless, the high value that general scientific culture gives to quantification is drawn upon in support of the need for quantitative advice. This is evident, for example, in the disparaging references to a ‘word game’ and ‘intellectual essays’ in Q 6.44. This is broadly evident in the overall vision of developing integrated models for EAFM. Scientists also see quantification as a mechanism for increasing their own control over policy, as is illustrated for example in Q 4.17.

An internal connection exists between the desire for quantification and ICES’s concern with ‘consistency’. Within ICES the various types of consistency are what allow the different parts of the network – including the broader network of clients and ministries – to negotiate and hold each other accountable for the results. They are mechanisms of transparency forged in the face of the paradoxes. This case study has identified three types of consistency – Types C, D and E – that most directly link the social and technical aspects of advice. Of them, Type E is the most sociological in nature and attempts to keep track of the coherence of a particularly complex set of decisions. Type E consistency is consistency in the way scientific methods and scientific advice are linked across time, space and species. As argued in Section 7.3.1, this kind of consistency has very little to do with science as such, in fact the questions it asks are not particularly coherent from a purely scientific perspective. For Type E consistency, quantification plays still another distinct and critical role. It names the methodologies in their linkages to advice functions. The failure of Type E consistency in the herring incident (Section 7.3.2), for example, was identifiable through the equations used in the spreadsheet. To use a bit of STS jargon, the science/advice relationship is 'inscribed' by the methods of quantification, and therefore answers to questions about Type E consistency will consist of the comparison of equations and measurements. Textual information in most cases will prove too imprecise to allow evaluation. Quantification here is a tracking mechanism for ensuring the transparency of managerial rather than scientific decisions. It is a form of accounting in both senses of the term.

The other pole of this axis, soft predictability, is a child of uncertainty. Soft predictability describes scientifically plausible scenarios while steering away from precise numerical forecasts about future states of nature. It has emerged from the experience of the advisory side that humility is a virtue when one tries to base advice on predictions about the future. Soft predictability recognises that many ecosystem processes are going to have to be understood qualitatively, based on categorical variables and indicators. It asks that focus be placed on the substance of the issues people care about, in a manner that is not defined by the degree to which the issues are amenable to measurement and modelling.

Soft predictability also means that decisions are going to be made using judgements that will be difficult to document objectively in the sense that other scenarios may be equally plausible. This means that the transparency needed for collective action cannot be achieved through methodological rigour, not even for technical experts. Soft predictability interferes with Type E consistency as every issue has to be worked out on its own. It will be very difficult to meet the constant political demand that the links between methods and advice reflect similar judgements. This suggests that we should begin to think of Type E consistency as a requirement for consistency of social practices and forms of interaction related to advice production, rather than as a requirement for consistency in mathematical or technical procedures.

Qualitative inference requires a participatory approach to translate categories into advice because there are no other ways to get the needed transparency. Hence, to some of the advisory scientists soft predictability is not a loss of quality, it is a way to improve quality by moving away from reliance on predictions, putting more emphasis on uncertainty, being more participatory, and resisting the inappropriate inflation of the science boundary.

8.2.4 Promoting the EAFM

Finally, there is a creative tension within ICES about the role that it should take in promoting the EAFM. Some scientists on the science side want ICES to draw on its scientific expertise as a way to accelerate an EAFM, with ICES taking the lead rather than just responding to the client commissions. This was illustrated in Q 6.25, while the idea that scientists on the science side are stronger supporters of ICES playing an advocacy role is supported in Table 6.3. This is arguably also related to an emphasis on the EAFM as a technical challenge, because that makes their expertise a greater asset in seeking the changes they want. Support for such an interpretation was found by Dietz et al. (1989) for other environmental issues.

Advisory scientists participating in this discussion tend to see ICES's role in terms of responding to clients. Rather than seeing ICES as a leader in the EAFM, they see ICES as lagging behind the clients because of poor preparation in providing advice. Again Table 6.3 suggests that advisory scientists tend to be less supportive of ICES taking a more active role in promoting an EAFM.

However, it is among scientists on the advisory side that an interest in redefining the way ICES deals with the precautionary approach is most often heard. Their frustration with the single-species 'stochastic predictability' (Degnbol 2003) comes mainly from the idea that basing the treatment of uncertainty on the error term in a single-species model not only ignores many sources and types of uncertainty; it has also been a real impediment, in the opinion of several leading ICES scientists, to the development of an EAFM.

The precautionary approach is an important driver for reform because it places the question of uncertainty in the centre of the problem; and that uncertainty is the elephant on the table that is exactly what the scientists working within the Advisory Programme experience every day. Where the initial responses to developing the precautionary approach were hijacked by the priorities of the TAC Machine to be just an addendum to the single-species model, the EAFM restores a broader meaning to the precautionary approach and gives it a new power as an organising principle. While there are differences in emphasis, ICES scientists working in many different areas see an EAFM as an opportunity for real reform.

8.2.5 Guarding communicative rationality

These areas of disagreement and disparate emphasis are rooted in the desire of both sides to guard the communicative rationality that makes science possible. But the science and advisory sides see different aspects of communicative rationality as being the one most directly threatened.

One of the basic principles of rational communication is that anyone has the right to raise a claim. This is the aspect that the advisory side has come to respect and guard in a new way by recognising the importance of non-scientist input to the advice-development process. The advisory side has learned to relax their ideas of science in respect of ideas such as science being an "objective other" delivering "the truth". They have begun to focus on the creation of serviceable truths (Guston 2001b) that meet needs without necessarily being the best and most comprehensive science. They have begun to build in double-loop learning and carry out science as a client-based practice in which an extended peer community is part of the process of developing the knowledge base for policy. One of the key lessons that has emerged from ICES's struggles with providing effective advice is the realisation (Q 6.3) that interaction across the science boundary is not a way to dilute the authority of scientists, on the contrary it is a way to clarify and reinforce the boundary.

Another of the principles of communicative rationality, however, is separating claims with respect to truth from claims with respect to values. One of the great benefits of the ideal model of review is that it separates discussions seeking compromise from discussions seeking consensus on facts. This came out strongly in Q 7.34 where a delegate linked the separation of the knowledge base from the negotiations over advice directly to the question of transparency, when he argued that the different ways of involving stakeholders was his main reason for supporting the separation of advisory and review groups. This is the underlying rationale for the ideal model of review – and indeed the overall mainstream view of the role of science in policy – where the emphasis is placed on getting a clear and independent statement of facts before the related negotiations over values take place. The same principle is seen in the desire to allow expert groups to be about science and not have them unduly influenced by needs for advice. The science side is guarding the mainstream, ideal notions of science because they protect the rationality of the process and finally the radical transparency of the scientific process.

What the advisory side has learned from experience is the power of the paradoxes of transparency. The motivation for the protection, separation and even isolation of science to protect the ideal model is based on institutionalising communicative rationality and securing transparency; but taken too far it undermines transparency by increasing exclusion. Several times I have termed this kind of separation of science from policy as 'naive' because it underestimates the tightness of the linkage in practice between the scientific results and the management based on those results. Within an EAFM, relevant specialities may be so narrow that the ideal model would even exclude many scientists; indeed, the overly rigorous enforcement of these ideals would undermine the EAFM by leading to the concatenation rather than the integration of advice.

The error that might lead to the ideal model being taken too far is the confusion of transparency of method with objectivity of knowledge and the related notion of the scientist as possessor of that objectivity. This is particularly an error when that objectivity is then attached to a social role and put to work in a policy context. What will not be tolerated by the interested public is the idea that some people stand outside the process. When government bureaucrats try to point to some outside, objective other to justify their decisions, what people see is not science-based policy but the covering of backsides. The advisory side of ICES did not come to the conclusion that the extended peer community is necessary by reading sociological theories, but by trying to deliver on the needs of clients. A real danger is that some scientists will continue to believe, in spite of the evidence, that their policy influence depends on people believing that they are independent authorities delivering objective truths. This may lead them to place their energies into defending that image through excessive purity rather than realising – especially given the level of uncertainty within an EAFM – that the fact that they cannot reliably deliver objective truth means that their skills, and their independent scientific processes, are needed more than ever.

The opposite danger is just as real. This is the idea that the passing of the white-coated other means that the mechanisms of transparency that the science side has been defending are anachronistic. Yet again, the paradoxes dictate that this cannot be taken too far, or transparency will be lost. The strength of ICES is its dual culture and the creative tensions it produces. The advisory side's ability to address the needs of an EAFM requires that it remain linked with, and accountable to, the science side.

Type E consistency plays an important role as ICES tries to figure out how to negotiate this shift. Type E consistency is an area where ICES scientists experience a good deal of confusion, but this is partly because they think of it metaphorically as a kind of scientific consistency when in fact what is really required to meet this very real demand is a consistency of social practice in clarifying the science boundary in the linkage between methods and advice. Type E consistency is rooted in a desire for clarity and fairness. It is transparency of political process, and the only real response is consistent procedures of interaction. Boundary work is where the deep transparency of science is pushed to become the basis of social practice, which then becomes a direct challenge to the mainstream view of science as an objective other.

8.3 Scale-based pressures

For social systems, especially large, democratic ones, the steps of adaptation that come after sensing the need for change, i.e. finding potential actions, selecting and implementing them, take place under the organisation and leadership of bureaucracies. The main problem that we face as we try to figure out an EAFM, at least for those of us who prefer large and democratic societies, is that an ecosystem approach is not an idea that reflects the realities of bureaucratic management. As discussed in Section 6.2.2, an ecosystem approach makes many basic bureaucratic requirements much more complex, such as calculable rules to trigger responses, firm legal definitions, low transaction costs and clear areas of responsibility. Many of the institutional design characteristics that make an EAFM difficult are basic requirements of coordination of large-scale policies in a democracy. The most basic contradiction, perhaps, is between the need for strong legislation and clear, practical, inter-agency, decision-making processes and the need for cooperation from a very large number of interested stakeholders that operate on multiple scales. The goal must be increased interagency coordination through more decentralised and participatory decision-making across multiple scales.

Scale is the most important thing. Indeed, in a way this book is guilty of failing to examine the forest for the trees. I have focussed on what can be learned from the ICES system by examining the details of the advice-generation process; but most of the problems actually stem from the overall European fisheries management system. The sheer size of the attempt to manage fisheries on a continental scale makes it absurd. The reason I have not discussed this much, I think, boils down to the fact that for a sociologist of natural resources, this forest is a boring one. The basic problem with the CFP is rather prosaic, it makes very limited use of the principle of nested systems that has been recognised for many years (Ostrom 1990) as basic to the effective governance of shared resources. No other fisheries management system in the world seriously attempts to manage fisheries through such a huge, top-down system. On a continental scale, the complexity of the information needed simply cannot be handled.

From a CST perspective, information-processing systems on large scales rely too heavily on automatic mechanisms like authority and markets that work well within their appropriate spheres, but rarely use, and often interfere with, communicative rationality. A bureaucracy trying to administer science-based policy on this scale is constantly going to look for ways to inflate the science boundary – to squeeze out expensive and difficult moral and practical problems and replace them with more tractable technical ones that can be linked to simple authority and market-based solutions. Indeed, bureaucracies will always contain such pressures, and a large scale simply amplifies them. But treating a political problem as a technical one does not make it so; it merely distorts it and likely makes it worse. As has been repeatedly documented here, scientists resist these tendencies as best they can to protect their sphere.

The main attempt to address the basic problems with the CFP was the creation of the Regional Advisory Councils. This attempt was a very tentative one; the RACs are only advisory in nature and are small institutions that still operate on extremely large scales for a fisheries management system. They have taken this challenge on in an impressive way, and ICES has sought to support them, but they must be radically strengthened – and many 'sub-RACs' created and funded – before they can make any real difference. From the perspective of this case, the creation of the RACs revealed how tightly coupled the advisory system really is. The further incorporation of stakeholders, while an important goal in itself, has led to even shorter time periods for delivering advice.

This has increased pressure on a system that already has serious problems with quality control. The advisory system leaves little room even for the preparation of tasks, for example getting an initial assessment done before the expert group meets, let alone chances for structured reflection within the expert groups about how they might better address their tasks. One of the most disturbing things the case revealed is how little follow-up there is on what happens to scientific advice after it is passed on in the system. There is no formal feedback between STECF and ICES. If STECF rejects the advice for some reason, ICES may only be informed by word-of-mouth or not at all. Nor is there much follow-up in terms of maintaining the consistency of the advice, in spite of the emphasis the leadership places on this (Q 7.21). In a system that is this tightly coupled, pressures towards efficiency push out reflection and communicative rationality.

The excessively tight coupling of the system is also reflected in disagreements over the role of the Secretariat. To a very real degree it is that small group that provides the lubrication that keeps the system from freezing up. The working conditions they face, in the period between the meeting of the advisory committees and the delivery of the official advice to clients, reflect the cost of this lubrication. In doing this preparation, they have to straddle the need to prepare coherent documents with the need to honour the fact that it is the committee that speaks for the delegates in the statement of the advice.

The most serious problems emerge when the system is forced to confront scientific uncertainty. The stress that scientific uncertainty creates for individuals within the system was documented in Chapter 5. Assessment scientists often see themselves simply 'turning the crank'. Uncertainty contributes to difficult working conditions because it is so difficult to know when your job is done when that job is to adequately answer a question that is impossible to answer. It leads to scientists' anomie, and measurably lower job satisfaction, when they find that what they are doing has very little in common with what they thought science was about when they signed up (Table 5.10). Combining uncertainty with bureaucratic imperatives leads to scientists appealing to other scientists to 'stop pretending we know how many fish there are' (Q 4.16). Effective responses to uncertainty are further undermined when bureaucratic pressures lead to inadequate characterisation of the uncertainty through attempts to reduce it to a set of quantities or even a single quantity. Uncertainty is too complex a phenomenon to summarise in a precautionary reference point. Scientists know this and surround the numbers in their advice with text that describes the uncertainty. The degree to which such text has any influence on subsequent policy is at best questionable. In many cases it is simply the number that is desired.

The system does not look much better viewed from the top down. Managers are frustrated by the difficulties of getting scientific advice that meets their needs. There is increasing pressure to channel more of the advice through STECF, where many of the same scientists take on a role that is more directly controlled by DG MARE and so operate in a culture that is somewhat more responsive to their needs. The incident of the mixed-fisheries advice described in Sections 4.2 and 6.1 demonstrated how such responsiveness makes it possible for DG MARE 'to do more work with less data' (p. 114). This single anecdote was often repeated as a way of illustrating the differences that emerge between STECF and ICES. Another telling description is the one in Q 4.7 contrasting science with engineering. The idea that fisheries science is a form of engineering that is required to plug quantities into slots created by the management bureaucracy is meant to push aside questions of scientific legitimacy and peer review. Now, this image was one offered by a DG MARE scientist describing how other DG MARE scientists felt when they were under a deadline to get a management proposal out and could not get what they needed from ICES. It does not represent anyone's ideal or goal, but it does show what these people often feel they need. Engineering demonstrates its credibility by working. But this is not an example of Mode Two science responding to society's needs; rather it is a caricature of science producing 'serviceable truths' (Guston 2001b). 'Working' here does not mean maintaining stocks of fish; it means only facilitating the regulatory process in the narrowest manner possible. It is the ultimate image in this book of the imperatives of authority-based coordination squeezing the communicative rationality out of science. It suggests a perversion of science born from an oversized, tightly coupled, decision-making system that could never, in its current form, carry out an EAFM.

One of the impacts of the bureaucratic system on ICES is that the debate about transparency is cast as the presence of observers at various meetings. The consideration of observers for meetings, as well as standards of documentation for the sake of transparency, are required in the current MoU with DG MARE. ICES is responding, and stakeholder participation is being approached in a tentative way that attempts to balance the various concerns. Casting transparency as simply an issue of observers may be dangerous because of the ways the paradoxes of transparency undermine formal approaches to transparency. The step-by-step strategy could be the wrong one. The observer issue is itself 'gamed' in terms of which groups and countries are selected. These issues can be addressed to some extent by a much broader opening, as this would let the stakeholders, who have quite different and conflicting objectives, check each other's tendencies to seek to influence outcomes. The paradox of precision and expertise, however, means that only stakeholder groups that have the ability to hire scientists to be the observers are really in a position to benefit even from this formal transparency.

What is really needed, but which is costly from a bureaucratic perspective, is experimentation with ways that scientists can help stakeholders develop transparency with each other, with the ultimate goal of agreement about what is found in nature. Formal transparency does not come anywhere near tapping into the real power of transparency in accounting for how one knows what one knows. This will require other techniques within an extended peer community, based on practices of scientific facilitation towards the creation of serviceable consensuses on facts within a results-based EAFM framework (Section 8.6).

The need for this is evident in the strong desire that scientists have expressed for more interactive, advice-producing fora. Building interactions into the science for advice mobilises transparency at a level that can make a difference. One indication is the desire in Q 6.1 to move beyond directly using models to produce numbers that serve political requirements to building political options into the models as alternatives. This is an improvement on the less transparent habit of putting conservative assumptions into models in the name of the precautionary approach. It implies acknowledging and moving away from the 'hiding of political values within models' approach.

Scale plays an important role here as well. One pattern that has been noted in effective, cooperative approaches to marine management is the importance of cross-scale institutional linkages (Wilson et al. 2005; Degnbol and Wilson 2008). An example would be when an NGO, concerned with a narrow set of issues across a broad geographical area, makes an alliance with a local government concerned with a broad set of issues in a small area. Cross-scale linkages are critical, because they team groups with agendas that operate on various scale levels. This is going to become even more critical as we move towards an EAFM. Cross-scale linkages often make possible the small-scale interactive fora in which communicative rationality can take place, even when addressing problems at the large-scale level. These activities bring the scale of interaction down to a level where the paradoxes of transparency can be worked through in a way that avoids or reduces gaming. This is beginning to happen more frequently in Europe (Hegland and Wilson 2008). When scientists become involved, activities focus on creating boundary objects that link science and policy. The scientists function as facilitators in science-based decision-making processes. An area where this is taking important form, addressed in other publications (Hegland and Wilson 2008), is participatory modelling using scenario-based approaches that place the uncertainty at the centre of the negotiation.

The bottom line is that formal procedures to achieve transparency directly, whether through rigorous application of scientific ideals or through formal observer programmes, often encounter the paradoxes of transparency and end up making things more obscure, while they may be good in and of themselves. The real power of transparency is achieved when scientists address questions of the science boundary directly within an extended peer community.

8.4 The benefits of distributed power

This case confirms and perhaps expands our understanding of the importance of the polycentric network for science in support of policy discussed in Section 3.4. Both Ostrom (2001) and Cash and Clark (2001) emphasise the information-processing aspects of polycentrism. Ostrom discusses how polycentrism facilitates accessing various knowledge sources, identifying feedback potentials and experimenting with different approaches. Cash and Clark point to an improved capacity to provide coherence across scale levels while still allowing local specialisation.

The ICES case certainly confirms these points, but it also focuses our attention on the importance of the distribution of authority and control of resources. Ostrom (2001) characterises polycentric approaches as hard to govern, and the frustration of the ICES leadership with the simultaneously voluntary and non-voluntary character of participation in ICES activities is a good example. ICES is an intergovernmental organisation, a professional association and a loose network of scientists, and this multiple character is a critical aspect of how it functions. To various degrees, depending on the urgency of the issues to be addressed, the individual's seniority, and the attitude of his or her employer, the ICES scientist is both constrained to participate in an ICES expert group and has to be convinced to do so.

The scientists within the ICES network are related to various centres of resource control, usually cooperating but often contending. Through the delegates, the NFIs and the relevant ministries have formal control of ICES, but the scientists in the ICES network can tap into many sources of funds for their activities. While the Advisory Programme is formally independent of the Commission, it remains to some degree dependent on it for funds. When the scientists are operating through STECF, they are more responsive, but not fully controlled in that STECF expert groups are temporary structures made up of NFI employees who are also part of ICES. Along the advice production chain some activities are funded by the clients who want the advice and some are funded by the NFIs. Again the relationship is neither completely voluntary nor completely constrained. Both the clients and the NFIs, to a variable but growing degree, have to respond to pressures from both user groups and conservationists that are channelled through their ministries.

The European Commission is not just a client; along with the national ministries, it is also a source of funding for a multitude of marine-science research projects and focussed tenders. Many of the NFIs very strongly encourage their scientists to compete for these projects so that they can get a substantial part of their salary paid by them. Many of these projects are long-term and exploratory, while others are just a small step away from advice provision. Who comes to ICES expert groups on the science side, and what they do when they get there, are strongly influenced by these projects.

One of the strengths of the widely distributed ICES network, with its voluntary yet constrained working relationships, is that it forces negotiations between its various power centres that must make use of communicative rationality, and this creates room for reflection. The gamut of negotiations and discussions required in the reorganisation of the advisory system, and the number and complexity of the issues that were discussed, make this very clear. The reference in the latter part of Q 7.32, that the ICES 'volunteers' cannot be ordered to make a piece of advice, is a fine illustration of how this system works to resist the systemic pressures described in the previous section.

To address an EAFM, ICES needs to be able to tap into an extremely wide range of expertise. The scientists in Q 7.67 see ICES as a network that enables the projects and activities where integrated, cross-disciplinary work takes places and makes it possible to develop an EAFM. ICES is developing a way to mobilise the network to make EAFM advice possible by the shifting-focus strategy that will take individual issues one at a time and push them as far as they can towards preparation for advice. This will mean both anticipating and understanding the implications of ecological events and moving integrated models as far as feasible. The strategy will lead to knowledge about ecological linkages, and sometimes even integration, being developed by the main experts in a form that can be applied by the less expert but still scientifically literate. The shifting-focus strategy is designed to enable a network (and eventually an extended peer community will be required) that can respond to the complexities of adapting to ecosystem changes. It will require resources and a huge commitment from many experts from many disciplines.

As it seeks to respond to the EAFM challenge, ICES benefits from its complex power distribution. The requirements for constant negotiation slow the influence of systemic pressures within the large management bureaucracy that threaten to squeeze out communicative rationality. Such communications are costly and difficult to structure formally, hence the squeezing, but they are needed if science for an EAFM is to be possible. This implies that the cooperation needed from across the network to develop an EAFM cannot be forced, making the motivation of ICES scientists critical. These scientists are seeing many former assumptions about what it means to be a scientist and to do science questioned. They are tired of being asked questions they cannot answer, of being asked to create certainty, and of spending long hours in activities they do not see as science. Some are beginning to see and act on new interactive styles of science where they use their transparency skills with clients and stakeholders to help them design realistic management strategies. These scientists are committed to a precautionary approach to marine management, and the

EAFM promises vehicles for achieving that in ways that deal in a much more authentic fashion with uncertainty. It is a good bet that their motivation and creativity will allow the development of institutions that can create and maintain the knowledge needed for an adaptive, ecosystem-based approach to marine management.

8.5 Theoretical ruminations: CST and STS

A central theoretical goal of this book was to explore how Communicative Systems Theory (CST) could make a contribution to Science and Technology Studies (STS). CST suggests the possibility of a meaningful approach to understanding hybrid natural and social phenomena that has a systematic place for an analytic distinction between nature and society. Trying to understand human society as a system adapting to its environment requires finding a way to define system boundaries, and defining society as a meaning-based communicative system separate from its material surroundings does that. Understanding adaptation, in my opinion, begins with examining science as an institution and how science is linked to possibilities for collective action.

The vehicle for the linkage between CST and STS is the use of the Habermasian notion of rational communication. This is a sociological usage of the idea, not the much more common philosophical one. Communicative rationality is what allows people to make sense to one another day-to-day. It never reaches the 'ideal communicative situation' of the philosophical system, nor does it need to, rather it describes criteria people actually use to judge if the communication they are engaged in is leading to a mutual understanding. In my interpretation, communicative rationality also includes Habermas's (1984) arguments about the communicative interpretation of the objective, social and inner worlds. This is the idea that reaching mutual understandings takes different forms and follows different orientations in respect of those worlds, so that discussions of fact must be oriented towards a consensus about what is true, while discussions of values and interests must be oriented towards an agreement about what is fair. Science, in trying to achieve the radical transparency that is the ideal goal of the scientific method, formalises and protects rational communication. Habermasian systems theory is built around this concept, and in this linkage lies the promise that CST can contribute to STS. CST focuses on the quality of communicative systems within a political context. Rational communication's internal connection to the scientific method makes it possible to directly relate science to a system concept of society.

Therefore, one aspect of this study that I hope has demonstrated the utility of CST for STS is the analysis of scale that is pulled together in Section 8.3, based on the theoretical perspective laid out in Section 3.3.4. Scale is a funny concept. On the one hand, we know a great deal about it, at least judging from the amount of literature. On the other hand, a great deal of this writing boils down to an acknowledgement that scale is really important. Science for an EAFM is confronted with problems stemming from both the social and institutional scale. I think CST provides some new tools for understanding scale and institutions that are relevant for science studies. One aspect involves examining how communication mechanisms effective on larger scales (Section 3.3.4) create systemic pressures to inflate the science boundary. These pressures complicate the delicate negotiations around boundary work, boundary objects, and building effective boundary organisations.

Section 3.1.3 uses communicative rationality to illuminate the relationship between scientific credibility and process legitimacy. The argument is that process legitimacy results from applying the same set of basic principles that scientific credibility is based on – i.e. communicative rationality expressed as the radical transparency of the scientific method – to the broader social context in which the scientific activity takes place. I would argue that the basic point of the extended peer review in a situation of high stakes and high uncertainty is to provide legitimacy to the 'serviceable truth' when the uncertainty is too high to establish credibility. If this is reasonable, then this link between credibility and legitimacy based on rational communication sets up criteria that extended peer review should meet to achieve that goal: that there is no manipulation involved in the communication, that anyone involved can raise a question about any claim being made (White 1988), and that discussions of facts are oriented around finding a consensus about truth while discussions of values are oriented around finding a fair compromise.

ICES has not formally adopted the idea of an 'extended peer review', even while the Advisory Programme has increasingly moved in this direction in practice. This informality has its helpful aspects, but it pays a price in not delivering the increased legitimacy that a more formal extended peer review would. Currently, most extended peer review involves the clients and is more oriented around increasing saliency than legitimacy; although this is shifting with the increasing importance of the RACs. ICES's main effort to increase legitimacy through extended participation in scientific activities has been the 'transparency through observers'. This is a good idea, but its effectiveness is severely curtailed by the paradoxes of transparency. Developing a set of rules for extended peer review grounded in rational communication would be a helpful addendum to this strategy.

One result of the application of CST that I did not plan was the idea of the 'creative tension' (Section 8.2). The paradoxes of transparency were not something I had in mind when I began the study of ICES. The central role played by questions of transparency within the case forced me to think about the role that transparency and accountability played from a CST perspective. What I have concluded is that transparency and accountability act as integrating devices that allow institutions to achieve the simultaneous need to structure both competition and coordination in situations where mechanisms for constraining behaviour are insufficient. Transparency and accountability are aspects of communicative rationality, i.e. they are institutional forms that guard the ability to raise claims. They are ways of structuring situations that make a mutual understanding possible. Within ICES the disagreements that emerge between the science and advisory sides, in some respects the product of an ideological competition, are transformed into creative tensions through a combination of mechanisms of accountability and fairly wide distribution of power within the network (Section 8.4). The concept of creative tensions, which I first heard working as a community organiser, strikes me as a useful one as a guide for institutional design.

A related idea is the various types of consistency that emerged from the study. Types C, D and E consistency are hybrid concepts that help clarify how co-production is happening in ICES. What is interesting to me is how type E consistency in particular expresses a new set of skill-based tacit understandings of review. Although it is not commonly understood within ICES, as befits an emerging tacit understanding, the review criterion of type E consistency is aimed at legitimacy rather than credibility. It is particularly well suited to, in fact in some sense it demands, an extended peer review. In Section 8.6 below I apply this idea to EAFM institutions.

I would expect that the main ideas developed here could prove useful in the analysis of other areas of developing science for policy and even in other aspects of STS. Understanding the development of other extended peer communities may be enhanced through the linkage of the credibility and legitimacy of science in terms of rational communication. The analysis of the types of consistency is very similar to a research agenda on 'commensuration' that is already underway in other areas of social life (Espeland and Stevens 1998). The CST approach to social systems and adaptation should be able to help clarify the links between the environment, bureaucratic or market-based management, and science in other contexts.

8.6 Living with the paradoxes: A results-based EAFM framework

When working to resolve the paradoxes of transparency, the term that ICES scientists use most frequently is consistency. The forms of consistency are mechanisms that they are creating to make transparency and accountability possible among constantly negotiating power centres dealing with very complex issues. These centres are found both within ICES and among stakeholders and clients, so an extended peer community is involved that includes different kinds of expertise. These mechanisms are, I think, a bit more than simply the kinds of language developments one would expect in any sub-culture, for example, the creation of jargon words that allow people to save time when talking to people familiar with a field. All of the different uses of the term 'consistency' are about ways of making things comparable. They are a language of accountancy even when they are non-numeric. They are all tools that allow the different power centres within the network to account to one another for the decisions, i.e. the science/advice boundary judgements, they are making. They are critical to the function of the science-policy network.

To a degree, they reflect tacit knowledge, meaning that knowledge about how to use the forms of consistency is not fully articulated. This can be seen in the way the same word 'consistency' is used to express several different and very important bases for judgement. They rely on the kind of scientific literacy we were introduced to in Q 7.30 that is required when scientists must make judgements about matters where they are not technical experts. They must get a sense of how certain or uncertain the knowledge is, and in order to do this they look, more or less consciously, at how settled the knowledge is in its application. This implies a judgement about how consistently it is applied.

Once again attempts to create transparency lead to a new paradox. Accounting for scientific judgements depends on the development of new kinds of mechanisms for comparison that contain elements of tacit knowledge. Within the scientific network charged with ensuring that our decisions about the ecosystem rely on the most transparent knowledge possible, we are required to trust a sub-culture that is at times difficult to describe, even for its initiates. We have already recognised (Barnes et al. 1996) how dependent the scientific community is on trust. Now, as we seek to expand the transparency of the scientific community to include the translation of science into advice and action, we find that the mechanisms of transparency, while not limited necessarily to scientists or dependent on specific forms of technical expertise, require a good deal of initiation to make them work.

Transparency is always limited so knowledge institutions have to rely on a degree of trust. Dealing with uncertainty further demands the development of trust, because procedures of transparency are harder to bring to bear. This is part of what must be understood with 'extended peer review'. There is a strange relationship between transparency and trust. They can and must substitute for one another when one of them fails, but when both are present, they reinforce and strengthen one another. The experience at ICES is that ensuring transparency in a complex environment creates a demand for new mechanisms of comparison that are themselves complex and must be developed through experience and slowly articulated through processes of reflection.

Once more we are brought back to scale. Because of the complexity of the issues, the size of the group of people who have access to the languages required for transparency will necessarily be limited. This is social scale I am referring to here, not necessarily geographical scale, and these groups would often be created through geographical cross-scale linkages.

I would argue that an EAFM will require a nested results-based system, organised around both sets of economic activities and geographical areas.

The science to support these institutions will require the conscious attainment of Type E consistency, achieved through consistent procedures and styles of practice, rather than attempts to create a purely technical consistency in forms of advice. At a minimum, three levels would be involved, more might be required in practice, although this would not be desirable. We can think of them as communicative "spheres" arranged one inside the other like Russian dolls. Each sphere would have a complex interior characterised by skill-based mechanisms of transparency, such as shared ideas about consistency rooted in a context-specific scientific literacy. These complex interiors would be encased, inside and out, i.e., facing both the sphere further out and the sphere further in, by simple shells.

An example might be a group of managers, scientists, fishers, tourism representatives, NGOs and other stakeholders who are concerned with implementing an EAFM on a shared regional sea. The outer sphere would be a public process of limit-setting that would create publicly sanctioned limits on a series of potential impacts to protect ecosystem integrity. The middle sphere would again include many of the same stakeholders, for example recreational and commercial fishers, managers, scientists and NGOs, who are concerned with a bay (it could as easily be a set of mixed fisheries or other sub-set of issues), and who are charged with translating the broader public limits into operational limits on fishing in their bay. This might be limits both on catch and on certain types of habitat impacts such as bottom trawling or numbers of speedboats. This group would form a subculture which does not share specific expertise, but does share the kind of scientific literacy about their bay that allows internal transparency and the reasoned assessment of uncertainty. Once this group has translated the limits, the inner sphere, a smaller group of e.g. scientists and recreational and commercial fishers would develop scenarios for catching and sharing those fish within the constraints imposed by habitat considerations. Again, from an interior perspective this would be a sub-culture that shares considerable scientific literacy about the specific set of problems they need to address.

The limit-setting process in the outer sphere would be dealing with both environmental and social complexity, and they would be accountable in limit-setting both to protect ecosystem functions and to allow as much economic activity as possible given the constraints of protecting ecosystem functions. But the product would need to be relatively simple: they would need to set up indicators of compliance for the middle sphere. Two kinds of simple indictors would be required that would form the simple outer shell of the first sphere of decision making.

The first kind of simple indicator would be a set of social indicators that would need to be developed that the middle sphere would have to meet to show that it is made up of people who fully balance the relevant interests. This is the absolutely critical requirement that the paradoxes of transparency create for any EAFM. It is just as important as the technical indicators. What may not be obvious here is that it is in the outer sphere that addressing an EAFM as a social dilemma is most required. It is very difficult for local interests, which are focussed so much on their own immediate problems, to be open to raising and dealing with new issues. This openness must be enforced from above.

The second kind of simple indicator produced by the outer sphere is, of course, a set of simple ecological indicators that demonstrates that the decisions that the middle sphere is making are sustainable. The indicators will have to be based on some sort of integrated model of the ecosystem; the question of environmental events would be addressed in the middle sphere because such events happen too rapidly for effective response at the higher scales. These models must be very simple to get the job done. This will often mean that they address broad processes. Many of the more aggregate properties ecosystems are more tractable than individual properties. Total production, for example, correlates with many community properties, while trying to predict what happens to individual stocks is much more difficult. The United States has, for example, set a cap on total biomass removals from the Gulf of Alaska. What seem to be needed to set limits on exploitation are processes that are broad and simple enough so that models can provide useful forecasts of the implications of changes. Fisheries scientists are also developing a set of simple indicators with very general applications, based on the size of the fish (Hall et al. 2006; Pope et al. 2006).

In the middle sphere, the need for inner complexity and outer simplicity repeats itself. Limits are going to have to be clearly expressed. The processes that set those limits involve a sub-culture that is scientifically literate in dealing with the complexity of for example their bay. These groups are going to have to address environmental events and relate them to implementation of limits on activities. The impossibility of full transparency again requires that interests be balanced and that the limits themselves are clear.

Finally, in the inner sphere operational plans are created to meet the limits set by the middle sphere. The critical issue will be the burden of proof. Those who wish to pursue the economic or recreational activity will be responsible for ensuring the transparency that will allow them to be held accountable for staying within those limits.

These ideas are not entirely utopian; several existing institutional models approach them. The Marine Stewardship Council, for example, has created a broad set of indictors for sustainable fishing and uses scientist certifiers to work in detail with fishing fleets to decide how the indicators can be fairly measured and met in their particular situation. The complex details of ecosystem interactions are handled by a fairly small group, and provisions are made for any interested party to get familiar with the issues and comment on them. This model for handling information is a very good one, independent of the form that the external accountability takes. A government licensing programme could use this approach just as well as a private eco- labelling scheme.

The ICES network is uniquely positioned to contribute to such a multi-level approach to knowledge institutions for an EAFM. The distributed network is able to handle the development of many different kinds of scientific groupings involving cross-scale linkages. They are well ahead, for example, of the EU in moving in this direction. The RAC expert groups are the beginning of a structure for organising interested parties, but they are still quite aggregated. ICES scientists have already shown leadership in moving fisheries management in Europe in a more responsive direction. They are also open to addressing new problems working with groups of stakeholders.

To create an adapting social system, we must balance complexity and simplicity to allow as much transparency as possible, recognising that mechanisms for transparency create their own obscurities. The ICES case indicates that to achieve this we need to learn to trust and to pay the costs of a wide distribution of decision-making power in creating and using science in an ecosystem-based approach. The hypothesis it suggests is that negotiations among multiple power centres create the space for open, rational discussion, without which a social system can never sense the need for change.

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Appendix 1: Methods and Procedures of the Random Sample Attitude Survey

The quantitative methodology used in this case was a random sample survey of European marine fisheries scientists employed in the countries around the North Sea, namely Denmark, Norway, Sweden, Belgium, France, Germany, the Netherlands, Ireland, United Kingdom and the Faroe Islands. A total of 465 valid responses were received. The sample size was 900, which indicates a response rate of 51.7% – a relatively high response rate for a non-telephone survey.

Defining a valid respondent was not straightforward, as there is no generally applied, firm definition of a marine fisheries scientist. We chose the following definition: a person who works (or has worked) in a fisheries-related agency, academic department, advocacy organisation or consulting firm and either a) has an advanced degree (MS/PhD) in a marine science or b) whose duties include(d) substantial time spent doing or peer-reviewing marine fisheries research. This definition corresponds to the definition used in a similar survey carried out in the USA (Wilson et al. 2002). The sample frame was constructed on the basis of participant lists from ICES events and groups and through referrals. It was ultimately up to the respondent to decide if he or she fit the definition when receiving the invitation to participate. The sample frame contained 1087 names of scientists employed in the countries around the North Sea.

The first contact to the 900 scientists was established by email in February 2005. The email contained, on the one hand, our definition of a valid respondent together with instructions and information about the survey and, on the other, a link to a website with a blank questionnaire connected to a unique ID number. After receiving our email, the scientist had the following four options: a) reply that he or she did not fit the definition, in which case the respondent was replaced with a randomly selected scientist, who had not been selected in earlier random selections; b) reply that he or she did fit the definition but did not wish to participate, in which case the respondent was not contacted anymore and likewise not replaced; c) ignore the email altogether; or d) follow the link and submit the questionnaire. Reminders and emails to replacements for those who replied that they did not fit the definition were sent out in three subsequent rounds at approximately two weeks apart during March and April 2005. After this, paper versions of the questionnaire were sent to the respondents who had not yet responded with either a valid response or a refusal to participate (in total 27 refusals were received). A total of 398 scientists, 86% of all valid responses, submitted their questionnaire over the internet.

The choice to use emails and internet-based questionnaires contributed most likely to the relatively high response rate. It is our belief that the use of emails in itself did not introduce any bias; almost the entire population of fisheries scientists in Western Europe are users of email and internet.

However, some problems with the use of the email procedure were also experienced. A substantial number of emails bounced, and these fell into three categories. The first category was scientists who had changed their place of work. In these cases a search for the new place of work and email address was carried out through the internet. If that search was not successful, which it usually was, the respondent was put on a list of scientists to receive a paper version of the questionnaire at the last known address. The second category was scientists employed in institutions that had recently changed the format of their email addresses. This problem was usually easy to solve, and the email could then be resent. The third and final category was the smallest. This was scientists with restrictive spam-filters that did not allow our email to get through. These scientists were – if an alternative email address was not available – added to the list of scientists to receive a paper version. Another problem was emails that simply disappeared. We also got some responses from a few scientists who indicated that they could not read the questions in their browser. A paper version of the questionnaire was sent to those scientists. Finally, a few respondents chose to submit a completely empty questionnaire over the internet. Those responses were treated as refusals. We have no reason to believe that the difficulties experienced in the email stage introduced any systematic bias in the results we report on here.

Paper versions of the questionnaire were sent (with a postage-paid envelope enclosed) to the 488 scientists who had not responded with either a refusal or a valid response by the beginning of April 2005. This was done in order to make sure that as many as possible of our selected respondents actually received our invitation and were able to answer the questionnaire. It is, moreover, reasonable to assume that some people, due to the fact that sending electronic mail is free, regard a traditional letter as being more serious and worthy of attention, something that would contribute to a higher response rate. The use of paper versions worked also as an extra safeguard against possible unrecorded problems related to the use of emails and internet. In total, 67 valid respondents submitted their questionnaire by post. The response rate for the 488 scientists who were sent a paper version was less than 14%, but this was not surprising since the majority of them had already received our emails and chosen not to respond.

The use of response scales to measure attitudes is standard in social science, particularly in the form of Likkert items, as was done here. We used a seven-point scale, which allowed the respondent to choose an entirely neutral answer. These scales are treated statistically as interval measures with reportable means on the basis of both the assumption that respondents' estimates are interval-based and the fact that statistical tests get the same results when the scale data are treated as interval measurements as they do when they are treated as categorical. Long experience and a great deal of theoretical work with these kinds of scales have found that at sufficiently high Ns, well under what we used here, they are very robust to differences in interpretation among respondents, i.e. the fact that one person would score a two when another person with theoretically the same attitude but with a different mode of expression might choose a three. Hence the comparison of means and the calculation of correlations among such scales yield meaningful results (Nunnally 1978). However, the main use of the means and correlations of these scales is to test for associated attitudes and for differences among sub-populations. Standard statistical methods such as t-tests or regressions have been shown to give reliable results for these kinds of questions (Nunnally 1978). It is also reasonable to make rough statements about where on a scale a group of respondents scores, e.g. 'well above the neutral point' or 'around the neutral point', but these scales are not meant to invent and then measure precise differences among peoples' attitudes.

One particular difficulty in the analysis of the survey data stems from the importance in the analysis of distinguishing between scientists who are the most active in the ICES/STECF stock assessment work and those who are not. This is not something that lends itself easily to the survey respondents' self-identification. It is a difference that is important in our analysis, but not necessarily something that a scientist would be comfortable with as a way to identify himself or herself. What we chose to do was to use the type of expert group the respondent last attended as a proxy variable. We classified as a stock assessment expert group any group assessing stocks for advice or preparing data for immediate use in such assessment, and/or reviewing assessments for use in advice. The classification of people in this way is not entirely satisfactory, someone who is not deeply involved in the assessment process could have attended just one such meeting and it might have been the most recent one, alternatively a scientist that spends a lot of time on assessment could have attended another group. However, fisheries scientists are fairly specialised, and we think that this variable did a reasonably good job of measuring the difference we were after.

Fisheries scientists are employed in different types of institutions and engage in various types of work. These affiliations affect how the scientists perceive their working and career conditions (Wilson et al. 2002). We divided the scientists who responded to our survey in the following categories based on present employer: NFIs, academic institutions, NGOs, EU, other private and other government. We furthermore divided the scientists into categories based on the last working or study group they attended, which gives us an indication of the work the scientists engage in. Table 5.1 describes the composition of the surveyed scientists in relation to employment and expert group participation.

Notes

1. The problems I am addressing in this book are related to natural facts and the roles of natural scientists. Much of what I say is applicable to social science, but for a number of complex reasons I consider the use of social science and social scientific advice in environmental management to be quite a different subject that should be addressed separately.
2. I use an analytical definition of stakeholder: any organised group that has an interest in a policy and can effectively influence either the content or implementation of a policy is a stakeholder in that policy.
3. Speaking of institutions as if they were actors is not accurate. Only people are actors because only people decide things. However, it is a very useful shorthand that avoids many awkward constructions such as the 'If an institution is to continue to function, the collective decision-making of people participating in an institution must somehow ensure that that institution continues in the face of' that would otherwise have had to replace 'Institutions face' to ensure full accuracy.
4. The September 2003 meeting of the Working Group on the Assessment of Demersal Stocks in the North Sea and Skagerrak (WGNSSK); the October 2003 and September 2004 meetings of the Advisory Committee on Fisheries Management (ACFM); the 2002 and 2004 meetings of the ICES/NSCFP Study Group on the Incorporation of Additional Information from the Fishing Industry into Fish Stock Assessments (SGFI); the September 2004 meeting of the ICES Consultative Committee; and the September 2004 meeting of the Management Committee on the Advisory Process (MCAP). It also supported the observation of two meetings of the Scientific, Technical and Economic Committee for Fisheries (STECF).
5. The March 2006 Workshop on Review of the ICES Committee and Expert Group Performance; the April 2006 meeting between MCAP and representatives of ICES's clients; the September 2006 meetings of the ACFM, the Advisory Committee for Ecosystems (ACE) and the Resource Management Committees (RMC); the September 2007 meetings of the ACFM, ACE and RMC; and the February 2008 meeting of the ICES Advisory Committee (ACOM) and Annual Meeting of Advisory Committee Chairs.
6. Much of the data gathering and analysis work that went into Chapter 5 was carried out by my colleague Troels Hegland, and he is acknowledged there as a chapter co-author.
7. A more detailed discussion of the theoretical background of this section can be found in Wilson and Degnbol (2003).
8. The Science Programme has now (i.e. in 2008 after this manuscript was complete) also been reorganised, and this committee is now referred to as SCICOM, for Science Committee.
9. I use the following conventions in quotes. Bold type is used in meeting observer notes to guide the reader through the argument(s) that I am referring to in the

text without getting lost in the cross-currents of complex and often non-linear discussions. I retain the broader text because I believe that good qualitative research requires that the context in which statements are made be clear to the reader. Brackets [ ] indicate a comment I am making.

10. I need to caution the reader because many, but certainly not all, of the quotes and exchanges I am using in making these points about the science boundary are related to the single issue of what to do with mixed fisheries advice for demersal fishing in the face of the collapse of the North Sea cod stock. This is a function of the meetings I was observing and the fact that it was a very hot topic. I will also point out, however, that the mixed-fisheries problem is a critical issue in advisory reform, these events have influenced a lot of the thinking about new directions at both ICES and DG MARE, and, as I argue in Chapter 6, the mixed-fisheries problem can be thought of as a sort of introductory-level EAFM issue.
11. Much of what is presented in Sections 6.2.1 and 6.2.2 first appeared in a section I authored in the publication: Integrating fisheries and environmental policies. Nordic Council of Ministers, TemaNord 2003:521, ISBN 92-893-0892-3. Permission to excerpt these passages was graciously granted by the Nordic Council of Ministers.
12. It has become fashionable in the last few years to use the term 'governance' rather than government, governing or management. Under this fashion lies an important idea: a lot of different institutions – governments, markets, civil organisations – make decisions that have an impact on our environment and our lives. 'Governance' points to how all these things work together in decision-making processes.
13. When natural scientists use the term 'policy drivers', they are usually referring to policy documents rather than social or political forces that shape policy formation.
14. There is a disturbing tendency among ICES scientists to treat RACs as fishing industry lobbying groups, when they are in fact stakeholder fora with representatives of a number of non-industry groups, including conservation NGOs. This tendency is potentially damaging to the ICES-RAC relationship and to the development of the RACs themselves, because the responsibility of the RACs is to give balanced stakeholder input. If RACs are to function, then they exist not to lobby, but rather to be lobbied.

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