An Analysis of Some Institutional Aspects of Science in Support of the Common Fisheries Policy

Wilson, Douglas Clyde; Hegland, Troels Jacob

Publication date:
2005

Document Version
Publisher's PDF, also known as Version of record

Citation for published version (APA):
Wilson, D. C., & Hegland, T. J. (2005). An Analysis of Some Institutional Aspects of Science in Support of the Common Fisheries Policy: CEC 5th Framework Programme. Institute for Fisheries Management. (Working Paper; No. 3).

An Analysis of Some Institutional Aspects of Science in Support of the Common Fisheries Policy

Douglas Clyde Wilson
and
Troels Jacob Hegland

The Institute for Fisheries Management
and Coastal Community Development

PO Box 104 Hirtshals 9800 Denmark

Direct correspondence to Doug Wilson +45 98 94 28 55 dw@ifm.dk

Wilson, Douglas Clyde and Troels Jacob Hegland 2005 An Analysis of Some Institutional Aspects of Science in Support of the Common Fisheries Policy Project Report for Policy and Knowledge in Fisheries Management. CEC 5th Framework Programme No. Q5RS–2001-01782. Institute for Fisheries Management. Publication No. 126

This study has been carried out with financial support from the Commission of the European Communities, specific RTD programme Key Action Five: Sustainable Agriculture, Fisheries and Forestry; Research Area 5.1.2 Sustainable fisheries and aquiculture. It does not necessarily reflect its views and in no way anticipates the Commission's future policy in this area.

Table of Contents

Executive Summary

This report is part of an institutional analysis of the production of scientific advice around the Common Fisheries Policy (CFP). Research activities were carried out under the auspices of the International Council for the Exploration of the Sea's (ICES) Working Group on Fisheries Systems. ICES is an inter-governmental organization that coordinates and promotes marine research in the North Atlantic. It coordinates the activities of more than 100 working and study groups made up of marine scientists, most of whom are employed by universities or government fisheries agencies. Scientific advice for the CFP is developed mainly though not exclusively through the ICES system.

The research activities reported on here include the observation of nine scientific working sessions related to producing scientific advice for the CFP. In addition, we carried out 29 formal in-depth interviews with fisheries scientists or close observers of the fisheries science process. We also carried out a random sample attitude survey of European marine fisheries scientists with a total of 465 valid responses.

Scientific advice is a critical element with the CFP and many stakeholders have an interest in it. Hence scientists working to produce this advice are doing so in an environment that has many bureaucratic and political impacts on their activities. These include how scientific activities are funded, demands related to the ways the advice is formed and communicated, and several kinds of more or less direct political influences on the advice process. The norms of the larger scientific community also have an important influence on the advice formation process.

The working conditions of scientists are systematically affected by their relationship to the advice production system. Fisheries scientists on average scored their job satisfaction well above the mean of our survey's scale, scientists working directly with the assessment of fish stocks for scientific advice, while still scoring above the mean, scored significantly lower than other scientists. This greater dissatisfaction is linked to travel demands and frustrations about their chances to produce peer reviewed publications. The high level of uncertainty they have to deal with also spills directly over into their working conditions because they are often have no objective way to know when their work is adequately completed. On the whole, scientists working in marine conservation NGOs are the most satisfied with their job and those in government fisheries laboratories the least satisfied. Scientists in all employment categories are experiencing increasing pressure at work related to changes in funding structures and increasing administrative duties.

We found systematic pressure on scientists to "inflate the science boundary" by which we mean to expand the range of issues that can be legitimately resolved through scientific findings. Fisheries scientists are increasingly being asked to deal with problems and concepts more directly suited to the social sciences. They are having to expand their models to deal more explicitly with problems around the allocation of fish stocks in addition to simply assessing their biological condition. The scientists are resisting these pressures to inflate the boundary in various ways.

Perhaps the most serious pattern we found is a growing belief among scientists that the activities they are doing in support of the CFP are far from their understand of what science is. Many scientists are experiencing a form of anomie arising from being asked to play a difficult role under sometimes trying conditions and then having the results of these efforts disembedded from the culture of their scientific community by the management system and changed into something they no longer see as science. Analysis of the survey data shows that this experience has a significant, negative impact on job satisfaction that is independent of the problems with working conditions mentioned above.

We argue that the pressures to inflate the science boundary will be a constant factor. Some are rooted in the unavoidable practical problems that arise when bureaucracies seek to regulate environmental activities. Others are rooted in the wider scientific community and the conservation values that are enshrined there. In response to the question “to what degree should judgements made in preparing scientific advice be influenced by the precautionary approach?” scientists’ responses averaged 5.92 on a scale where “never” was scored one and “always” scored seven. The precautionary approach creates ongoing difficulties for how science is done. Current practices for implementing the precautionary approach rely on calculating probabilities related to biological models. But many of the real sources of uncertainty in fisheries simply cannot be reduced to such probabilities. 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.

Scientists throughout the advice system are calling for increased reflexivity across the science boundary. They recognize first that they are not the only experts in the process and second that the uncertainty of the marine environment means that no single form of expertise has the right, or even adequate, answers. A growing group of scientists feels that advice needs to be produced in a much more interactive manner with managers and other stakeholders. This means that mainstream view of the role of science needs to be modified. If the exclusive role of scientists is to offer up to the political process the “objectively real” for all to see and make decisions about, then scientists will continue to be forced, more often than is healthy for either themselves or the management system, to create things in which they do not believe.

We conclude that rather than asking the scientists to be less than what they understand “scientist” to mean, we need to ask them to intensify their expert role, but in a new way. Of all the institutions in the fisheries system it is science that has built itself on radical transparency. Scientists are the transparency experts, they know what it really means to explain how they know what they know. Transparency, and the accountability it makes possible, is the key to making multiple stakeholder, adaptive management of the marine environment a possibility. Only bringing together the various forms of relevant expertise - scientists, fishers, conservationists - in a transparent manner can make the needed information available in a sufficiently timely manner. Scientists can help, and in many cases are helping, to facilitate interactions between stakeholders in respect to building an accurate common picture of the marine environment. This is a much more realistic role than being The Experts who tell the other stakeholders how it is.

Three different and to some extent competing institutional needs are experienced by fisheries scientists in Europe that at the moment are either not being carried out or being carried out by ICES alone. The first need is providing scientific advice for the CFP. The second need is for scientists to have an organized voice to express their views as professional scientists and employees. The third need is to organize to advocate their personal beliefs about reforming fisheries management. These things are to some extent being addressed through the ICES system. But the second and third needs must be separate from the advice provision system. Hence, we would recommend that fisheries scientists create a Europe-wide independent professional organization either within ICES but clearly distinct from the advice process or entirely independent.

Abbreviations

ACE	Advisory Committee on Ecosystems
ACFM	Advisory Committee on Fisheries Management
CEC	Commission of the European Union
CFP	Common Fisheries Policy
DG Fisheries	Directorate-General for Fisheries and Maritime Affairs
EBK	Experience Based Knowledge
EP	European Parliament
EU	European Union
F	Fishing mortality
HCR	Harvest Control Rules
ICES	International Council for the Exploration of the Sea
MCAP	(ICES's) Management Committee on the Advisory Process
NFI	National Fisheries Institutes
NGO	Non-Governmental Organization
PKFM	Policy and Knowledge for Fisheries Management Project
RBK	Research Based Knowledge
RSE	Royal Society of Edinburgh
SGFI	(ICES's) Study Group on Industry Information
SSB	Spawning Stock Biomass
STECF	(DG Fisheries') Scientific, Technical and Economic Committee for Fisheries
TAC	Total Allowable Catch
VMS	Vessel Monitoring System
WGFS	(ICES's) Working Group on Fisheries Systems
WGNSSK	(ICES's) Working Group on the Assessment of Demersal Stocks in the North Sea and Skagerrak

1. Understanding Science as an Institution

1.1. Objective of the Present Report

One aspect of the Policy and Knowledge for Fisheries Management Project (PKFM) is an institutional analysis of the production of scientific advice around the North Sea cod, which is the project case study. The present report is part of that effort, but the main focus is on the scientific institutions within the overall fisheries system rather than those related to a specific fish stock. Science plays a critical role in the Common Fisheries Policy (CFP), which is perhaps the most science-dependent of common policy arena within the European Union (EU). The mainstream view of science and the CFP is that the role of science is to be a neutral provider of objective advice for the overall political process of fisheries management. This is, however, a utopian view because fisheries science is not separate from politics but is itself a politically constituted process dealing with great uncertainty. So the debate over what is and is not "science", meaning the set of facts that all rational participants should accept, is constant.

We believe the value of an analysis such as this report comes from finding ways to improve the ways that policy institutions use science in a realistic rather than didactic fashion. Especially in circumstances of high uncertainty, insisting that the problem is that the science is wrong, that it is not objective, or that it suffers from "outside" influences that should be "stopped" is not helpful. Accepting the reality that when fisheries science is used to assess fish stocks to facilitate management is a part of a political process, and trying to understand what that fact means for how science is and should be done, will take us much further down the road toward developing an accurate and common knowledge base for fisheries management.

1.2. Theoretical Approach

1.2.1. What do We Mean by Institution?

Volumes have been filled defining the word "institution". Modern "institutionalist" social science uses (many contested) analytic definitions that are broader than the lay understanding of the word as a large organization (a museum) or patterns that persist over very large temporal and spatial scales (the family). These things are certainly institutions but from an analytic perspective any pattern of behaviour that persists over time does so because of some set of shared understandings, and those sets of understandings are also institutions. We might, therefore, analyse a fisheries management rule that has been operating only a year or two as an institution.

The definition of institutions offered by Scott (1995, p 33) is instructive: "Institutions consist of cognitive, normative, and regulative structures and activities that provide stability and meaning to social behaviour." Often people think only of the regulative aspects of institutions and it is true that every aspect on an institution is described by stating an "if x then y" rule. After all the point of institutions is that they pattern behaviour and rules are pattern descriptors. But institutions also have a cognitive aspect. They define the meanings of things. All the various meanings we associate, for example, with the term "brother" are integral to what a family is, and when they change the family changes. Finally, institutions have a normative aspect, i.e., the behaviour they pattern is seen as normal behaviour. Every aspect of a functioning institution contains cognitive, normative and regulative dimensions.

1.2.2. Epistemological Basis of research

Institutions are iterative phenomena and are always changing at the margins. An institution patterns an actor's behaviour, other people see or otherwise learn about that behaviour and this contributes just a little bit to their understanding of the institution. This has an impact on their subsequently behaviour. Hence, a critical underlying assumption of the methods we are using is that watching and asking people about interactions can illuminate the institutions that are both patterning their behaviour and being reproduced by their behaviour. In modern society in general, and in reference to this study in particular, the critical behaviours for the iterative reproduction of institutions are the behaviours of communication, what people say / write / send to each other and when they do so. What scientists do is gather information, calculate things on the basis of what they have gathered, talk with each other about what they have calculated, and then report what they found. Only the calculation bit is not a communicative behaviour, though it is strongly influenced by the needs of communications. This is not to say that physical behaviours are not important. Discarding fish, for example, is a critical physical behaviour in our field. But on the scales with which we are concerned, how the discards are reported has much greater impact on the institutions and subsequent behaviour than the actual discarding.

The underlying theoretical framework of this study is communicative systems theory (Habermas 1987), which address how institutions shape and are shaped by communicative behaviours. A more general description is available elsewhere (Wilson 2003, Wilson and Delaney 2005). What is important here is that the theory analyses institutions in terms of the functional requirements of communications that allow individuals to coordinate their behaviour within the institutional patterns. The focus is on how the social "machine" works, especially when it seeks to adapt to changes in its natural and social environments. Causality is understood as institutional forms resulting from a combination of the functional requirements of coordinating behaviour and the relative power of the different groups involved. Non-sociologist readers should be warned that this kind of functionalist causality is not mainstream sociology. In sociology functionalism is often disapproved of because asking why something works the way it works begs the question of why someone wants it to work that way. While recognizing the validity of this criticism we still think that functional demands on communications to coordinate actions place limits on social systems that need to be understood.

From this communicative systems theory perspective what allows science as an institution to be sensitive to factual truth is the practice that any truth claim about nature can be raised and impartially evaluated. It depends entirely on communications oriented toward convincing people through arguments that something is true. In other words, if science is to be successful at uncovering the truth, then the fundamental requirement on it as a communicative system is that claims are not blocked by dogma, prejudices, communicative distortions, or the use of social power to silence or ignore the claim. Clearly this ideal is not always met, just as clearly it is often approached or science would make no progress. So our institutional analysis of science begins with this idea as the central shared meaning around which participants understand the institution of science. We analyse the institution of science partly in terms of the circumstances under which is succeeds or fails to meet this functional requirement. This ideal of science has been articulated in a number ways. We turn next now to some of these articulations as we try to translate this epistemological approach into a research strategy.

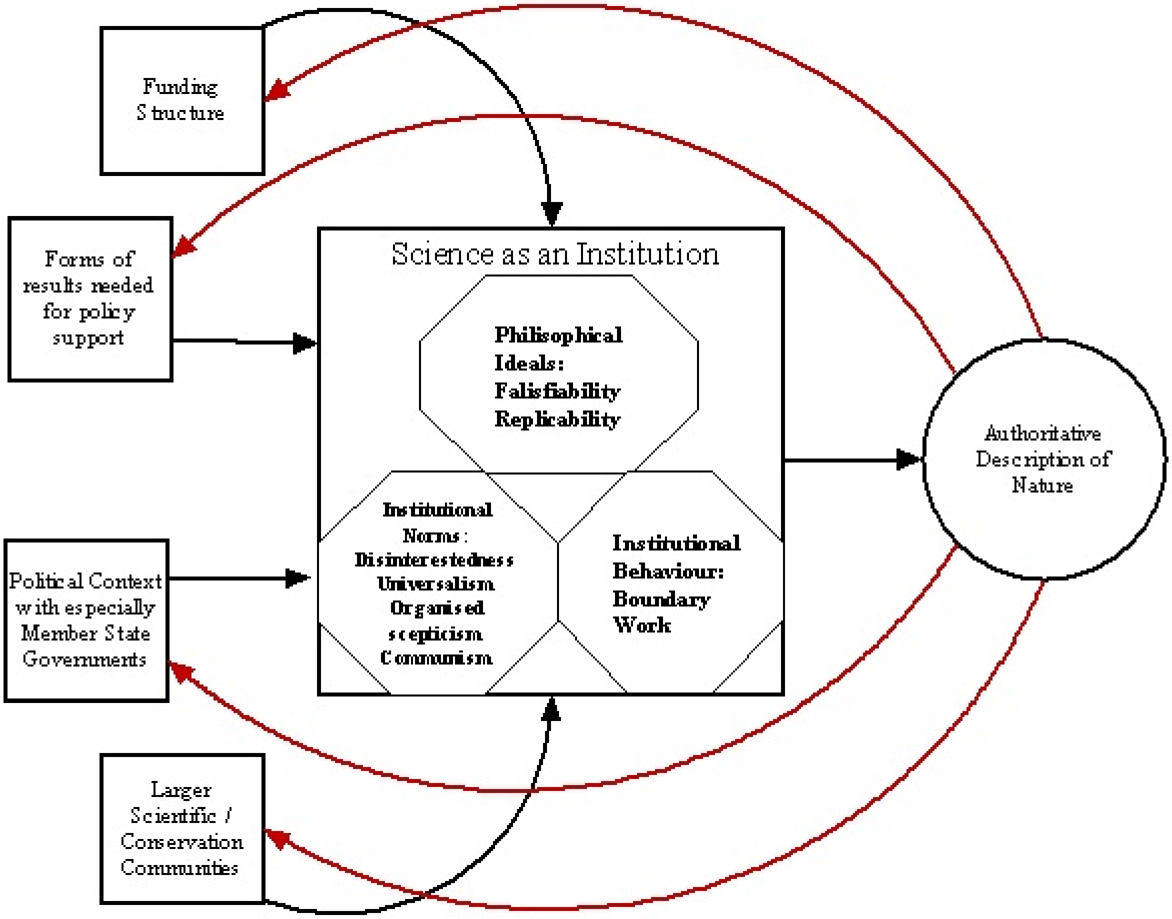

Figure 1: A Theoretical Understanding of Science in Support of the CFP

1.3. Developing a Research Strategy

1.3.1. Shared Understandings of Science

Developing a research strategy based on the approach described in the previous section begins with outlining how this functional ideal of science is articulated as a common understanding among scientists. This is the central box in Figure 1. It contains one set of images out of philosophy and two out of different kinds of sociology of science. The first two of them are what Habermas would call "partly counterfactual critical devices" meaning regulatory ideals that are not expected by anyone to be fully met in the real world, but which must be met to some degree if people are to have a mutual understanding that a situation is one in which, for example, "science" is taking place. So "paradoxically" (Habermas and Nielsen 1990, p 105) while the ideals are never actually met they are still a critical part of the makeup of social reality. The last one is a description of institutional behaviour which gives expression to these counterfactual ideals.

Most natural scientists if they had to articulate the "ideal" of science would likely begin with Karl Popper's arguments that science is never about proving something true but only about showing that something is not false. For real science to happen a hypothesis must be articulated that is falsifiable, an experiment designed that is able to prove the hypothesis wrong, and the best result is that the hypothesis is not disconfirmed and left intact to be tested another day. The experiment must also be replicable, in other words it must reach the ultimate level of transparency where other participants are so fully informed about what is being claimed that they are able to do the exact same activities and reach the same result. Quantification is closely related to this ideal because only numbers can communicate the natural world precisely enough to reach this kind of transparency. This philosophical ideal, in other words, is an abstract methodological prescription for ensuring the communicative ideal that any truth claim about nature can be raised and receive an impartial evaluation. Any claim can try to run this gauntlet.

The second set of images in the central box in Figure 1 come from the work of Robert Merton (1968). He did a survey of scientists and asked them what characteristics of scientific institutions they thought necessary. He distilled their responses into four of what he called institutional norms. The first is *scepticism*, scientists greet all new ideas skeptically, placing the entire burden of proof on the idea. They accept nothing because it sounds good. The second is *universalism*, the tests that the new idea must face are universally recognized criteria, i.e. the Popperian methodological ideal just described. Third, scientists are *disinterested*, they do not try believe and argue for something because of personal interest but because it is scientific truth. Now we can well imagine any scientist reading this is smiling at this one, it is a rare scientist who can avoid the temptation to argue for his or her own ideas even after the evidence is moving away from them. This norm has a long pedigree, though. In the 17^{th} century as modern science was just being formed it was felt that only a gentleman could be a scientist because only someone who did not rely on work for an income could be truly disinterested (Shapin 1994). The last ideal was *communism*. Scientists had to be able to have access to each others results so that science could progress. Merton (1968) himself pointed out that these norms are rarely honoured, but they do operate as real counter-factual critical devices. That drug companies pay for drug tests is pointed out every day as a way to undermine their credibility. All four norms are closely related to maintaining the functional ideal of science as treating all truth claims openly and evenly.

The last aspect of the common shared understanding of science is the concept of “boundary work”. This idea has been a critical element of the sociology of science at least since Gieryn’s (1983) seminal paper on the subject. This is not an ideal of science but the form of behaviour that seeks to express this ideal. Boundary work is the constant, ongoing discussions and activities around what is and is not science and who is and is not a scientist. Observing boundary work is perhaps the most essential activity in the sociology of science, not just because it is the concrete expression of the shared understanding defining science as an institution, but also because it is in boundary work that most if not all of the conflicts and structural patterns of science are revealed. The concept of the science boundary plays a central role in the present analysis.

1.3.2. Research Strategy

Figure 1 should be understood as the “theoretical” figure - the starting point of the analysis. Around the central box we have been discussing are the basic causal processes that the research explores. To the right is the product that the scientific institution, as it exists in support of the CFP, is supposed to produce: an authoritative description of nature that can be used as the basis of fisheries policy. The ideal here is objectivity. This is supposed to be the objective truth that the system can take up, see how many fish can be taken from the sea, and use this number as the basis of the negotiations over how the fishing opportunities will be allocated. This ideal depends on approaching the scientific ideals, so objectivity is also difficult to fully achieve.

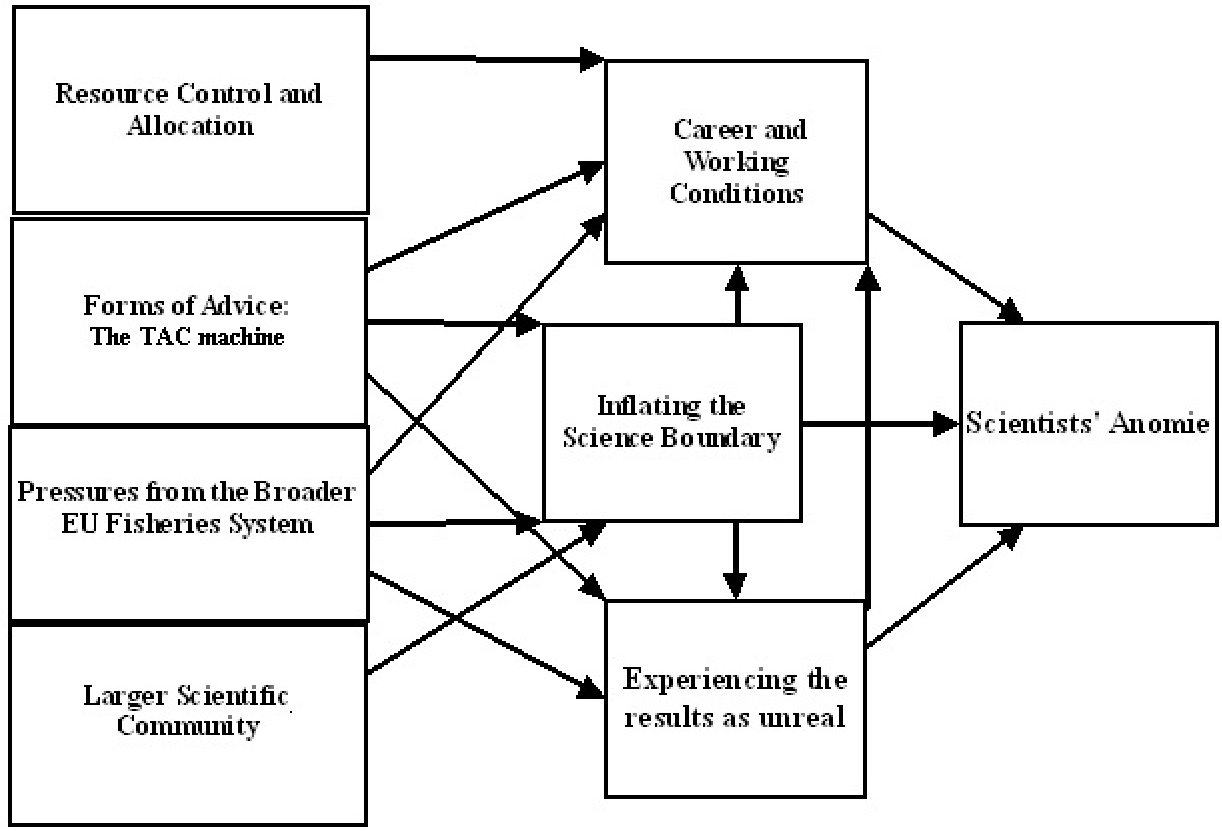

Figure 2: Some Empirical Relationships Uncovered by the Research

The product, the science on which management can be based, however, is still a critical element in the overall fisheries system (ICES 2002). This importance is why the other four boxes on the left hand of the figure become involved. Many environmental influences have an impact on the scientific institutions. We have organized them into four categories. The structure of how science is funded is clearly the beginning point of any analysis. The second box, however, is driven directly by our theoretical perspective. Various kinds of functional needs within the overall fisheries system demand that scientific results are formulated and communicated in particular ways. Third, there are a number of different kinds of political pressures that have an influence on how science is carried out. Fourth, the larger scientific community influences the ways that scientists understand and carry out science in support of the CFP.

Where Figure 1 refers to our theoretical starting point, Figure 2 outlines the main empirical relationships we found. The four categories from on the right hand side remain. The other four boxes categorize what we found in terms of the impacts of these environmental influences on the way that science is carried out. In the center, as discussed above, we found a number of impacts on the science boundary. We refer to these impacts as "inflations" of the science boundary because the main outside pressures are toward including more and more kinds of claims under the heading of "science". Scientists are resisting these pressures in various ways. We also outline a number of ways that doing science in support of the CFP is having an impact on the working conditions of scientists. At the bottom we come to perhaps the most serious pattern that has emerged, this is a growing belief among scientists that the activities they are doing in support of the CFP are so far from their understand of science that many of them feel that they are no longer really doing science.

The last box is the impact that all of this is having on the scientists. We thought a long time about what to call this experience and finally settled on a word coined by Emil Durkheim, one of the 19th century founders of modern sociology. The word we chose is anomie, which the American Heritage Dictionary (2000) defines as "alienation and purposelessness experienced by a person or a class as a result of a lack of standards, values, or ideals". The anomie 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 context that produced it, in other words from the background understandings that make that product, for the scientists, "science". A parallel can be found between the experience of the scientists in fisheries management and the fishers. The demands of fisheries management are in many ways alien to the cultures of both groups. This should not be pushed too far. Fisheries scientists in Northern Europe for the most part have good jobs that they enjoy doing, they certainly do not suffer the kinds of hardships from the current crisis in fishing that the fishers do. However, there are serious problems with the institutional framework of science in support of the CFP and these problems are having a negative impact on many of the people involved. One important part of this is a dampening of the creativity of the people who are perhaps in the best position of all to contribute to the reform of fisheries management and the recovery of healthy fisheries.

1.3.3. Research Methods

The PKFM project is designed around the analysis of the role that claims about nature are playing in the fisheries policy process, using the management of North Sea cod as a case study. The North Sea cod case dictated the meetings that we observed and the discussions that we focussed on during those meetings. To a lesser extent it also dictated the selection of interview respondents and the countries in which we carried out our random sample survey of fisheries scientists. However, we decided very early that our objective was to do an institutional analysis of aspects of science in support of the CFP and that the role of the North Sea cod case was to enable and focus, but not necessarily limit, that enquiry. So we discussed, recorded and report on here many things that have implications for the CFP and fisheries management in general beyond simply the North Sea cod case.

1.3.3.1. Qualitative Research

Research activities were carried out under the auspices of the International Council for the Exploration of the Sea's (ICES) Working Group on Fisheries Systems (WGFS). We[1] observed in detail five scientific deliberations within the ICES system and two meetings overseeing such deliberations: 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 Study Group on Industry Information (SGFI); the September 2004 meeting of the ICES Consultative Committee; and the September 2004 meeting of the Management Committee on the Advisory Process (MCAP). We also observed two meetings of the Scientific, Technical and Economic Committee for Fisheries (STECF). We carried out 29 formal and numerous informal interviews with fisheries scientists or close observers of the fisheries science process. Publicly available documents such as ICES and STECF reports and the Memorandum of Understanding

[1] These interviews and observations were carried out by the broader team working on various aspects of the PKFM project. In addition to interviews and observations conducted by the authors we draw on interviews conducted by Alyne Delaney, Petter Holm, Jesper Raakjær Nielsen and Kåre Nielsen.

between ICES and the Directorate-General for Fisheries and Maritime Affairs (commonly known as DG Fisheries) were also analysed. Notes from observations, informal interviews, and original documents were analysed using NUD*IST textual data analysis software.

1.3.3.2 Random Sample Survey of Fisheries Scientists

We carried out 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 Faeroe Islands. A total of 465 valid responses were received. The sample size was 900, which indicates a response rate of 51.7 percent - 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 organization 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 e-mail in February 2005. The e-mail contained, on one hand, our definition of a valid respondent together with instructions and information about the survey and, on the other hand, a link to a website with a blank questionnaire connected to a unique ID-number. After receiving our e-mail 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 e-mail altogether; or d) follow the link and submit the questionnaire. Reminders and e-mails to replacements for those who replied that they did not fit the definition were send out in three subsequent rounds with approximately two weeks apart during March and April of 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 percent of all valid responses, submitted their questionnaire over the internet.

The choice to use e-mails and internet based questionnaires contributed most likely to the relatively high response rate. It is our belief that the use of e-mails in itself did not introduce any bias; almost the entire population of fisheries scientists in western Europe are users of e-mail and internet.

However, some problems with the use of the e-mail procedure were also experienced. As expected a substantial number of e-mails bounced. The bounced e-mails fell basically in three categories. The first category was scientists, who had changed place of work. In these cases a search for the new place of work and e-mail address was carried out through the internet. If that search was not successfully, which it usually was, the respondent was put on a list of scientists to receive a paper version of the questionnaire on the last known address. The second category was scientists employed in institutions, which had recently changed the format of their e-mail addresses. This problem was usually easy to solve and the e-mail could then be re-sent. The third and final category was the smallest. This was scientists with restrictive spam-filters that did not allow our e-mail to get through. These scientists were - if an alternative e-mail address was not available - added to the list of scientists to receive a paper version. Another problem was e-mails that simply disappeared into cyberspace. It was in some cases a problem that the different browsers used to go on the internet 'read' the webpages differently, which affected the appearance of the questionnaire. This meant that we got responses from a few scientists, who indicated that they could not read the questions in their browser. A paper version of the questionnaire was send to those. 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 e-mail stage have 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 relative “costlessness” of sending electronic mail, regard a traditional letter as being more serious and worthy of attention, something that would contribute to a higher response rate. These 'e-mail-survey-sceptics' would not be included without the use of traditional letters. The use of paper versions worked also as an extra safeguard against possible unrecorded problems related to the use of e-mails and internet (for instance e-mails, which possibly just disappeared into cyberspace). In total 67 valid respondents submitted their questionnaire by post (14 percent of all valid responses). The response rate for the 488 scientists, who were sent a paper version, was low (under 14 percent) but this was not surprising since the vast majority of them had probably already received our e-mails but chosen not to respond to them.

The use of scales to measure attitudes is standard in social science. We use here a seven point scale, which allows 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 on 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 purely categorical. Long experience and a great deal of theoretical work with these kinds of scales have found them to be 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 yields 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 from 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 his or her self. What we chose to do was to use as a proxy variable the type of working group the respondent last attended. We classified as a stock assessment working 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 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, alternatively a scientist that spends a lot of time on assessment could have attended another group. However, fisheries scientists are fairly specialized and we think that this variable did a reasonably good job of measuring the difference we were after.

2. The Institutional Environment of Scientific Advice for the CFP

Scientific advice for the CFP is mainly developed through the ICES system. ICES, the International Council for the Exploration of the Sea, was founded in the late 19th Century to investigate both natural and man-made causes for fluctuations in fisheries stocks. ICES is an inter-governmental organization that coordinates and promotes marine research in the North Atlantic, including adjacent seas such as the Baltic and North Seas. With more than 1600 marine scientists from nineteen countries around the North Atlantic, scientists working through ICES gather information about the marine ecosystem. This information is used to fill gaps in existing knowledge; it is also developed into unbiased, non-political advice. ICES advice is used by the nineteen member countries to help manage the North Atlantic Ocean and adjacent seas. ICES has three advisory committees that provide advice on marine ecosystem issues. The committee of greatest interest here is the one which provides advice on fish and shellfish stocks, the Advisory Committee on Fishery Management (ACFM).

Hence, the ACFM is the official scientific body providing advice to the Commission of the European Union (CEC; Commission) represented by the DG Fisheries. Other actors in relation to the direct formulation of the Common Fisheries Policy - and thereby also consumers of knowledge - are the legislating body of the European Union's Agriculture and Fisheries Council, where the member states are represented by the minister responsible for fisheries. A less prominent role is played by the European Parliament (EP) and its Committee for Fisheries.

Beyond ICES, DG Fisheries has its own advisory committee, the Scientific, Technical, and Economic Committee for Fisheries (STECF), which often consists of many of the same members as ACFM working groups. Unless otherwise specified, where we refer to scientific advice in support of the CFP we are referring to the advice from ACFM and/or STECF scientists to DG Fisheries.

2.1. Resource Control and Allocation

The level of resources for the provision of scientific advice in the EU system is an ongoing constraint. The Commission has said publically that there are not enough scientists to provide the analyses and advice which the Community needs to operate the CFP and that the demand for scientific advice for fisheries management has exceeded ICES' capacity to supply it (CEC 2003a). One indicator they point to is an increase in the number of special requests made to ICES by the Commission (Tables 4 and 5). Special requests in this case meaning requests that are not part of the annual cycle of advice for stocks under ongoing management. In respect to ICES in particular, the Commission’s response is to demand that ICES prioritize its work to focus on the most depleted fish stocks and to speed up the time it takes to deliver advice (CEC 2003a). On at least two occasions ACFM scientists have expressed discomfort to us with the degree of Commission influence on ACFM, given what is supposed to be a somewhat distant relationship.

The Commission’s frustration in trying to get the fisheries science advice it needs both through ICES and more directly though STECF is clearly related to the fact that real financial control in the system is mainly held at the member-state level through the national fisheries institutes (NFI) and the other organizations that employ the scientists directly. Beyond its relatively small secretariate, ICES’ scientists are employed by other bodies. Scientists view themselves as doing ‘volunteer’ work when engaged in the ACFM and ICES working groups. At least one scientist suggested that this means that the Commission should hold more distance than it currently does from the ICES fisheries advice process. A scientist in a leadership position at ICES told us that he sees this "we are volunteers" interpretation to be fairly wide spread.

The degree of commitment the NFI actually have for the ICES system is perceived in various ways. One scientist at the WGNSSK meeting reported that the working group work is seen by his institute as being as "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 working 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 working groups. The average among 169 employees of national fisheries institutes who had attended such groups 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. Scientists whose most recent working group had been directly involved in producing stock assessments for management advice (including STECF) rated this encouragement higher. On the whole, NFI employees perceive their institutes as being supportive of working group participation, and this support is seen to be slightly greater in working groups related to stock assessments (Table 1).

Table One: "Does your employer encourage participation with working groups" scale by Type of Working Group

Type of Last Working Group Attended	Mean	N	p
All Scientists who Attended Working Groups			
Not directly related to stock assessment	5.06	160	0
Directly related to stock assessment	5.72	123	
Total	5.35	283	
NFI Employees who Attended Working Groups			
Not directly related to stock assessment	5.65	81	0.11
Directly related to stock assessment	5.92	88	
Total	5.79	169	

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

One scientist who works for an NFI described changes in how resources are being distributed within his institution:

> Q1. When I first came here, because 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. We don't seem to have that ability anymore, simply because within the overall contract, which has to be signed and agreed every year. We have to say, "look, we'll give you this, this, this, this and this. So we're much more contractually bound on paper.[2]

[2] We follow these conventions in the presentation of quotes: italics indicate a change of speaker; italic at the end indicate, where necessary, the source of the quote; brackets indicate a comment made added by the authors; and, bold is used to highlight specific parts of the quote.

In addition to going through ICES, the Commission also has the option of going straight to the scientists' employers to recruit scientists to address fisheries management problems directly through the STECF. When scientists attend STECF meetings, the Commission pays for travel expenses but they remain employed by their home organizations even though they are working on assignments put to them directly by the Commission. The Commission does not find this arrangement adequate because it prevents the expansion of STECF tasks beyond what the goodwill of NFI and other employers allows. In indirect contrast to the perceptions of the NFI employees reported in Table 1, the Commission does not believe this goodwill is adequate to meet its need for direct advice (CEC 2003a).

The Commission are not the only people that see problems with the STECF system. In one interview with two ACFM scientists a story was related that we have heard mentioned in several other contexts among ICES scientists. The general story is outlined below in section 2.2.2, but the reason for mentioning it here is that reactions to these events illuminate an STECF - ICES tension. It relates that after the ACFM had refused to do mixed fishery assessments because of a lack of data on discards that they felt made the analysis impossible, the Commission 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. The scientists provided the results of the Commission had asked for and the ACFM scientists now believed that some the Commission employees were claiming that this showed 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. The event has come up in meetings, one scientist got a laugh at an ACFM meeting by joking that "the Commission is better than ICES because they are able to do more work with less data."

Another scientist who is very active in STECF, however, believes that ICES is simply not pleased to have another committee that second guesses it's advice. But, while this scientist believes that STECF should be seen as another opportunity for the Commission to get needed work done, he also argues that at STECF there is "scope for people to be rather naughty". Members of ACFM have been known to go back to the members of their administration and report that while they do not agree with the result in the ACFM report, they will have a chance get the advice changed at STECF. Another stock assessment scientist told us that the Commission organizes 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 "the Commission exploits scientists" because it is so much less structured and finally harder to work with than the ICES system.

These tensions between ICES, the Commission / STECF and the national fisheries institutes have important influences on how science in support of the CFP is carried out. Understanding the basic distinctions between these three institutional complexes begins with understanding the financial realities.

2.2.Forms of Advice

A helpful way to understand the institutional and historical development of forms of scientific advice for the Common Fisheries Policy is what Holm and Nielsen (2004) have termed the TAC Machine. They outline the co-evolution of the problems that managers and politicians needed the advice to solve, mainly the need for total allowable catches (TAC) in order to apportion the fish among the various polities and stakeholders, and the age-structured family of stock assessment models, which includes the XSA model often mentioned in this paper, that has been developed to solve these problems. This section describes key aspects of how the TAC machine currently operates within EU fisheries management, including attempts to make incremental changes and reforms. Holm and Nielsen's (2004) historical analysis, however, raises important questions about whether such reforms can make much difference to a system which they accurately describe as "leaking cod".

2.2.1. The Commission's Basic Requirements for Scientific Advice for Fisheries

The discussion over the structure and performance of the system that provides scientific advice for fisheries management in Europe has in the latest years emerged as an increasingly important policy issue. This development is to a large extent related to the ongoing reform of the CFP and to changing perceptions of best fisheries management practices in general.

From the point of view of the managers in the Commission 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. Perhaps the critical issue is the need for the operational units reflected in the advice to match those of the fishing activities managed. Fisheries scientists traditionally worry about units defined by nature. This is mainly single fish stocks, or increasingly biological and ecological interactions between stocks and between the fish and their environment. All of these things have, in principle, natural referents that exist beyond their social construction. 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 refer only to a social construction (Wilson and Delaney 2005).

The Commission needs scientific advice that they can apply to the management of fisheries, i.e. fisheries-based, multi-species advice. They feel considerable pressure here. To quote one example related to us by an ICES official quoting the Commission, "when ICES advises a closure for cod, haddock, and whiting and not for plaice, sole, and nephrops, there is a perception in the whitefish sector that the flatfish sector is not taking up its share of the conservation burden."[3]

Other important concerns of the Commission are described in a recent paper laying out their recommendations for improving scientific and technical advice (CEC 2003a) and in communications between the Commission and ICES around the negotiation of their Memorandum of Understanding in 2004. Critical concerns include transparency, timing, make sure advice is flexible enough, and not allowing the advice to leave room for interpretation. A desire for ICES to increase the scope of advice to respond to stakeholders' concerns about the impacts of environmental changes, pollution and previously implemented management measures on the fish stocks is also mentioned. Responding to these stakeholder concerns, which are closely related to an important general discourse in management circles in support of an "ecosystem approach to fisheries management", also finds strong support within both ICES and the Commission.

In terms of transparency the Commission argues that no matter how the advice is generated, through either a formal analysis of data or more general expert opinions, procedures must be public and subject to review. In general advice should "be prepared in a spirit of openness to public scrutiny" (CEC 2003a, p 2).

In relation to timing, the problem the Commission sees is that the advisory system is not designed to respond to urgent requests (CEC 2003a). The Commission is considering both making greater use of short-term contacts and more stringent prioritization of matters to be addressed. They see both options requiring an increased number of capable scientists. This is a separate issue than the one often raised by the fishing industry about the year or greater lag time between when data are

---

[3] A more detailed discussion of the general demand for mixed-fishery advice can be found in Wilson and Delaney (2005).

gathered and when they have an impact on the size of TACs, which the scientists in our survey rated relatively low (Table 2). The problem the Commission sees is that particular issues arise, in the EU political arena or in international negotiations for example, which are in need of a quick scientific assessment. Because the ICES system is geared toward addressing the ongoing yearly rhythm of stock assessments and generating advice for TACs it is difficult to organize resources to address such requests.

The flexibility problem arises from the mainly positive, from the Commission's viewpoint (CEC 2003a), development of standardized criteria that has greatly improved consistency of advice across fisheries, but certain issues have been lost as a result of this standardization and this has made it at times more difficult to adapt the advice in relation to specific fisheries.

Table Two: Prioritization of Problems with Respect to the Process of Producing Scientific Advice for Management

Last Working Group Attended / Problem	No Working Group	Non-assessment Working Group	Assessment working 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 prioritize 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 a working 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 working group or not; 69 who chose not to answer the question; and 29 who failed to rank according to instructions.

Finally, problems arise when advice is seen as leaving room for interpretation (CEC 2003a). They want the advice to be clear and understandable. The Commission would like ICES to account for uncertainty without making the advice uncertain, a tension which generates continuous discussion and debate in the advice giving process. This concern is reflected in the increasing use of harvest control rules (HCR), meaning forms of advice that apply over several years and specify in advance specific actions under specific circumstances. These HCRs have a number of benefits in terms of longer term planning and less expensive management for both the managers and the fishing industry.

2.2.2. Challenges in Formulating Advice for Mixed Fisheries

The quota allocation problem that characterizes 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 precise answers to this question are generated by quantitative forecasts of the future state of fish stocks under various conditions, particularly levels of exploitation. One debate is over when it is appropriate to make such forecasts because such forecasts are close to the heart of some scientists understanding of what a stock assessment is. The following exchange took place at the ACFM meeting mentioned below in respect to their decision to decline to make a short-term forecast for the North Sea cod:

> Q2. *Scientist One*: We don't reject the assessment but we reject it as a basis for a forecast. *Scientist Two*: Different words same meanings. *(From observer notes at the ACFM meeting in October 2003)*

A second closely related debate arises from the Commission's need for 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. The kinds of uncertainty and the resulting debates about forms of advice are illustrated by the following history about advice for the management of North Sea cod as one stock among many caught by various gears in the North Sea.

The discussion in Quote 3 took place at a stock assessment working group meeting in respect to the assessment of the North Sea cod stock. In general, these scientists trust their models much less when they are dealing with historically very low stock sizes, as is the case here. They had what they considered strong information from the fisheries independent sources indicating that their catch data was wrong, mainly as a result of discards and misreported landings. They believed that their model (the XSA) accurately described the level fishing mortality (F), because this would not by biassed as long as the misreporting is constant, but that the level of the spawning stock biomass (SSB) would be biassed low. This biassed SSB could perhaps be used appropriately as a conservative estimate of the stock size. One scientist felt that the information they had would be appropriate for making medium term (several year) projections, but not for the short term forecasts on which the TAC advice would be based. They also knew that the age class about to be recruited into the fishery was particularly small leaving little margin for error.

> Q3. *Scientist One*: It is a difficult precedent in ICES to follow this reasoning, there has been constant comment that when a biomass is this low we cannot do medium term forecast. I don't know how ICES will buy this. *Scientist Two*: We want to show what is happening, we want to illustrate the effects of the low recruitment, medium term forecasts never show what is happening. *Scientist One*: So where do we go? I sympathize very much with not presenting a short term forecast. We can make it but it should not go in the report. Is that acceptable? *Scientist Two*: You want it done but not reported? *Scientist Three*: Could you clarify why? *Scientist One*: From the results I have seen there is a strong indication that the XSA does not pick up the true level of the stock. It takes the stock from a level of biomass that we think is wrong. *Scientist Three*: So we should not accept it as a final assessment as well. *Scientist Four*: It is a final assessment of the level of exploitation but not of the stock. *(From observer notes at the WGNSSK meeting in Sept. 2003)*

The conclusion was that they did not do a forecast but left open the possibility of doing so when more data was available. They were concerned that the decision not to do a forecast would not be well received at ACFM, but this did not prove to be the case. The scientists at ACFM knew that the advice for North Sea cod would be a zero catch. So they were able to make a principled stand about not pulling a number out of uncertainty while at the same time not confronting a demand for a particular number to set a quota from. Instead the following appeared in the subsequent ACFM report:

> Q4. The absolute level of the recent stock size (SSB) and fishing mortality cannot be determined due to uncertainty in recent catch figures. However, conclusions about the state of the stock are not sensitive to this uncertainty. Although the current SSB is uncertain, it has been reduced to a level at which the biological dynamics of the stock are difficult to predict and productivity is impaired.......Scenarios for 2003: Due to the uncertainties in recent catches, no deterministic forecast is presented for North Sea cod. In order to illustrate the possible dynamics of the stock under management measures introduced in 2003, a number of scenarios were carried out. Scenarios assumed a range of possibilities for 2003 fishing mortality, from recent F (2000-2003 average), to the F expected if catch equals the TAC." *(ICES 2003a, p 235)*

These kinds of considerations were also applied to the setting of the Blim, the lower limit management reference point for the spawning stock biomass. This number had been set at 70,000 tonnes in 1996 under an old system and many scientist felt that it was much too low. A new system placed the Blim as high as 160,000 tonnes (ICES 2003c) based on a segmented regression of stock biomass on recruitment that identified a break point where the production of new recruits clearly began to fall after a stock was reduced to that point. The old method, rather than placing the lower limit reference point at this break point had placed the Bpa at this point. The Bpa is the biomass used as a target in order to severely reduce the chance that Blim is reached even when there is high uncertainty. As such it must be set much higher than the Blim.

The basis of both the old and new methods, however, is itself controversial among scientists because for many fish species this break point is not clear, with cod happening to be one of the few species where the method does give a clear result. Scientists at both WGNSSK and ACFM were reluctant to make a change in the official Blim because this would mean using a method that many were not comfortable with to move, in effect, the goal posts for the management of a critical stock without making any practical difference in the final result for the near future - that fishing for cod need to stop. As the scientists themselves discussed it:

> Q5. *Scientist One*: So the point of seeing recovery is the ICES definition of Blim...... *Scientist Two*: We should take away [from the text under discussion] the 150,000 tonnes. We are talking about closing the fishery until we reach that level, if we aim the advice to a lower level. *Scientist One*: 150,000 is Bpa, we should aim at Blim. *Scientist Three*: We have never used Blim in advice. *Scientist Two*: We have never seen a stock so low. *Scientist Four*: Yes it is so low we won't even be at Blim in a year. *(From observers notes at the ACFM meeting in October 2003)*

Furthermore, there are other stocks that should be changed the same way and even international agreements that would have to be revisited. As one scientist described the working group report's handling of the issue "there is some soft wording that it may be more appropriate [to redefine Blim] but may give management problems." The question for which this number did matter, when the cod stock could be considered recovered, could wait, hopefully for clearer criteria. The final advice left the Blim at 70,000, while commenting that the reference points "are consistent with the precautionary approach, provided they are used as upper bounds on F and lower bounds on SSB, and not as targets" (ICES 2003a, p 53).

The advice for cod was further complicated by the rising demand to provide mixed-fishery advice. The idea of giving advice for mixed fisheries is critical one because fleets frequently fish for more than one fish stock. For example, in the North Sea boats that fish for nephrops catch cod as bycatch. If capturing cod was completely banned, these nephrops fishers would be unable to fish, even though the condition of the nephrops stock would support such fishing. Single-stock advice also does not take account of biological interactions between different fish species and the fact that species are often caught together. In 2003 the Commission requested that ICES begin to implement multi-fleet, multi-species short-term forecasts.

Managers want models for mixed fisheries to make simultaneous predictions about many fleets fishing for several fish stocks. At the WGNSSK the scientists defined the fleets based on gear and mesh size and sought to model the fishery interactions of 83 fleets for which they had some data. There were a number of challenges. The fleet definitions were controversial, the necessary data was hard to get and what they had was only for one year. Some of the needed data existed, but was not available for scientific use. Even when available, the data was from many sources and considerable work was required to make it compatible in terms of both the type of data and the levels of aggregation.

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 line between the political and the scientific. Hence the form that the report takes becomes more controversial:

> Q6. *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 the 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. (*From observer notes at the WGNSSK meeting in Sept. 2003)*

ACFM took the challenge of producing multi-species 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. Nevertheless, the ACFM agreed with the WGNSSK that the data was insufficient for forecasting the impacts of various combinations of fleet effort. They did indicate that if reliable landings and discards data were available by fleets, 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 2003a, p 222).

The Commission was not in a position to be able to carry on with these outcomes from ACFM. As a Commission scientist described this issue in a public lecture:

> Q7. 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 in safe biological limits. So this means no cod catch and would require the closing the demersal fisheries. So basically the advice is no fishing. 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 rhetorical 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 short term forecast for North Sea cod that the WGNSSK and ACFM had specifically declined to make. The STECF recognized the validity of ACFM's reasoning but also believe that "despite its numerous limitations, it would be more appropriate to provide advice based on evidence for the mixedspecies 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). Their key response was to point to the distinction between using the 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 was not sufficient (one of several reasons for their refusal to make the short term North Sea cod forecasts) and 2) the definitions of 'fisheries' were too coarse. STECF agreed that these would be a fatal flaws for the mixed-*fishery* advice but they did not believe that it would be so for the mixed-*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, highlighted in Quote 6 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 with out 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) can be understood as 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 in respect to fish species, but not to fisheries, a social unit for which data is 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. This 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).

2.2.3. Transparency: Validity Includes Providing a Clear Explanation

The scientists we observed and interviewed are mainly supportive of increased transparency. One suggested that the XSA model has become like "a great big black box" that very few people, even among scientists, really understand inside and out. He believes that some scientists are satisfied with this arrangement but that the fishing industry is getting more critical of these black boxes, that the distrust between the two groups is intensified by the opaqueness of the methods being used.

One aspect of the movement toward increased transparency is that ICES is opening up their 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 the Commission and the stakeholder groups who have been working in various ways for such access. We did short interviews with all of the observers that 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 mis-reporting 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". A third scientist said that he was aware of trying to articulate things in a less technical fashion, but suggested that this may also have positive aspects.

In working sessions scientists are very conscious that their work will undergo scrutiny and there is an increased appreciation that this scrutiny will go beyond the ACFM and even beyond the client groups such as the Commission. This awareness is reenforced by these clients, as in following short exchange:

Q8. *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. *(From observer notes at the ACFM meeting in October 2003)*

One impact of this expectation of public scrutiny is that explainability becomes a criterium for the validity of results. This is evident in Quote 6 above 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 recognizes that lay people are going to read the report but at the same time feels that they should be writing their report for other experts, i.e. ACFM, and not be constrained to write with lay people, even managers, in mind. Nevertheless, toward the end of the meeting as the report is being finalized the following exchange took place that showed considerable awareness of the possibility of a politically oriented lay audience.

Q9. *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 understand 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. (*From observer notes at the WGNSSK meeting in Sept. 2003)*

Increasingly transparency is based on the ability of fisheries stakeholders, including scientists, to be able to explain how they know what they know. This requirement is being pushed deeper into the workings of the scientific deliberations than it has before. Studies of science in legal contexts have shown that there is a point at which science cannot live up to legal expectations of objectivity, especially when it finds its full expression within the unremitting incredulity of an advocacy-based legal proceeding (Smith and Wynne 1989). Science depends on a background of internal trust among scientists, because almost all results depend on a series of skilful acts and expert judgements, especially in the face of high complexity and ongoing uncertainty (Barnes et al. 1996). Mechanisms for increasing the transparency of European fisheries science will have to shield that science from being pulled too directly into legal confrontations.

This increased demand for transparency is closely related to the Commission's other concern with consistency and flexibility. Consistency has become an important operating norm for the scientists. Despite the fact that in and of itself consistency is not a scientific virtue, the perception of fairness in how the different stocks are treated is politically very important. A leader at ACFM, for example, began a plenary editing session by announcing that "we did not make a good rationale for one species as opposed to another and while that it not our job, we need to respond to this criticism" (from observers notes ACFM October 2003). A scientist who is the chair of one of the stock assessment groups told us in an interview that standardizing stock assessment approaches is appealing because everyone is "singing from the same sheet" and ACFM will be able to more easily evaluate the work. However, he argues that these stocks are so different that it is unlikely that one method will be valid for all stocks and circumstances. So what is happening now is that each stock has its own method and has an assessor that knows that inside and out. This second approach is scientifically more appealing in the sense that it leads to a more accurate description of the natural word, but once again it is more heavily reliant on a background culture of internal trust, particularly trust in the certain individuals who are relied upon to know the stock "inside and out".

2.2.4. The Unambiguous Interpretation of Uncertain Advice: The Role of Text

The tension between the need for advice to be clear, in the sense that it cannot be interpreted in different ways by different stakeholders, and the need to account for the uncertainty underlying that advice arises in very practical ways within the political milieu surrounding the implementation of the advice. One relevant exchange was related to us by an ICES official in respect to general discussions with the Commission about how the advice should be communicated. The Commission's request was that ICES "account for uncertainty when providing its advice but it should not be done by making the advice uncertain." ICES agreed that the advice should be clear and said that they would continue to work on the problem "along with the Commission observers at the ACFM meetings". For the ICES scientists, this particular problem in deciding how to articulate "official scientific advice" was something that the scientists and the managers need to work out together.

The Commission sees part of the problem as being the way ICES presents results in a deterministic fashion. In their view, lay people, in particular the fishing industry, may then 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 the time of the negotiation our respondent described, the 2002 North Sea cod assessment had included an indication of 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. A short-term catch option table had been interpreted by stakeholders as saying that a small reduction in TAC could result in a significant increase in biomass. Hence, the results seem to show that the stronger measures that both the Commission and ICES thought necessary, were not. The Commission believed that the uncertainty underlying these results in particular had not been effectively communicated. They wanted the advice to be placed in the form of harvest control rules with expression of uncertainties around the assessment numbers. They did not specify what these expressions of uncertainties would consists of and our respondent felt that this decision was, somewhat unfairly, being left up to ICES. Their concern was that there was no consensus among scientists about what the best way of calculating the precision of assessments, and even more that the real uncertainties arose from the fact that the population dynamics of the cod stock at such historically low levels were not at all understood. They also noted that the recent assessments had been overly optimistic about the rebuilding of the cod stock, which they attributed to discarding and changes in fishing patterns that are the pattern in mixed fisheries governed by a number of separate quotas. These were the main things that ICES felt needed to be communicated to the industry about the cod situation.

This discussion reflects the ongoing underlying issue of quantitative tabular data and 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?" for which the Commission, at the end of the day, wants to answer with a precise number that will allow these fish to be divided among the member states. On the other hand, as the concerns of ICES in the last paragraph reflect, the uncertainties underlying the results are complex and need to be explained with words and phrases such as "stock dynamics at low levels are not understood", "discards" and "changing fishing patterns". Whatever the Commission 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.

The following exchange took place at the ACFM meeting after the discussion related above between the Commission and ICES. It is a long excerpt but very illustrative of the problems that they are facing and 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" recognizing that there will be discards of cod from fishing boats targeting other species, and the refers the reader to the section mixed fishery advice.

> Q10. *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 we need to. ..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 a 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 prioritize 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 '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 recognize what they are doing. *Scientist Two*: We could have an opening statement saying the catch should be 0 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. *(From observer notes at the ACFM meeting in October 2003)*

Not only must the advice for cod within the mixed-fisheries context be communicated using text, 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 the 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. While this is a particularly dramatic example, comments indicating that scientists need feedback from managers to help them make needed decisions or trying to anticipate how managers will interpret statements that the scientists are making are very common at both the ACFM and assessment working group level. It is in these kinds of comments that we can see how the line between the science and management has become truly reflexive. In this particular comment such reflexivity is perhaps especially ironic because this scientist is referring to "take account of the mixed fisheries" when the Commission is asking for tools to do just that. Tools that in a few days the ACFM is going to decline to provide (see Section 2.2.2 above).

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, was 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 to all this trouble to define what they mean readers may not 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 "Category One" be inserted instead so that people would be required to look up exactly what they meant.

Not only does the ACFM take the text of the advice very seriously in terms of seeking to limit how advice will be used. This is also true of scientists at the working groups that feed into the ACFM. Consider the following exchange:

> Q11. *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. (*From observer notes at the WGNSSK meeting in Sept. 2003)*

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 organized skepticism (Merton 1968) 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 the ecosystem approach to fisheries" "If you give loose, non quantitative advice to managers 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". This was part of the question of how to develop ecosystem advice where relevant quantitative methods are much less available.

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:

> Q12. *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 to 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 the Commission may be the numbers found in table, when these numbers are thrown into a political process the text that the ACFM is struggling with in such detail does, according to one person who is both deeply involved and financially interested in the outcomes, have an important effect on how the numbers will be used. The textual aspects of the advice push up against the limits of its role as a scientific recommendation. This is nicely illustrated by the desire of the scientist in Quote 10 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 'politicize' science by making it available for public scrutiny and input can promote the interests of both science and democracy.

2.3. Pressures from the Broader European Fisheries System

The third category of institutional influences on the production of scientific advice is a more amorphous one than the previous two. It is also closely related to aspects of both resource control and the forms of advice, and indeed to the larger peer communities discussed in the next session. As contribution to a political process, scientific advice for the CFP is meant to play a political role. That role is evolving in ways that have implications for both the overall network of fisheries policy institutions and for the way that science is done.

2.3.1. The "Mainstream" View of Science in the Policy Process

What could accurately be called the mainstream view of the role of science within the policy process is articulated as follows by the Commission: "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, p15). 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 the Commission is able to point to scientific conclusions to justify particular management measures. This need is only intensified by the move toward 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.

A related question in our survey asked the extent to which scientists in working groups suspected other scientists of arguing in a way that was consciously biassed by that scientist's national interest (Table 3). The overall mean is higher than the mainstream view just described would like it to be at just below the middle point of the scale. Mean answers for those scientists whose last group was directly involved in stock assessment was higher than for the others. Scientists have also expressed on several occasions feelings 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 the same if not a higher level.

Attempts are made to enforce this mainstream view of the role of science. One way of doing this is to discourage scientists from articulating policy opinions. One role of the scientists is to provide support to negotiators and one of our respondents related that he was present at the negotiations when the 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 real role contradiction because on the one hand he was a part of the negotiating process and he should support it, but scientifically he could not. This incident shows both the legitimate pull of both these roles and the willingness of the scientists superiors to at least a limited degree of enforcement.

Table Three: When participating in working or study groups how often have you suspected that a scientist was arguing in a way that was consciously biassed by his or her national interests?

never = 1 7 = very frequently

Last group was not a stock assessment group	Mean	3.51
	N	161
	Std. Deviation	1.768
Last group was a stock assessment group	Mean	3.89
	N	123
	Std. Deviation	1.624
Total	Mean	3.68
	N	284
	Std. Deviation	1.715

Relationship is significant at .06. Excluded to reach N of 284: 148 respondents who indicated not having participated in a working 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 working group or not; and seven who did not answer the question on bias.

Another respondent articulated this tension this way:

> Q13. 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[4] looking at them. The scientist in Quote 13 mentioned in another part of his interview:

> Q14. *Interviewer*: When you're doing the assessment, you think "this will be scrutinized"...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 toward 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 scrutinized, with little of the "distance" than brings "credibility and influence" in the words of the Commission (CEC 2003a, p15).

2.3.2. Accountability Arising from the Fishing Industry

One of the most critical sources of political pressure on scientific institutions supporting the CFP is from the demands of other stakeholders. This is mainly the fishing industry but also conservation NGOs are to a large and growing extent involved in scientific accountability. This takes a number of forms. Stakeholders will often involve politicians in challenges to particular findings or measures based on those findings. A rising trend is for stakeholders to involve their own scientific advocates in debates. This can mean hiring trained scientists or, in some cases, finding scientists who for various reasons take issue with the mainstream findings as represented by ICES and STECF. In some cases stakeholders have managed to convince governments to bring in prestigious scientific bodies to examine fisheries science, such as the Royal Society of Edinburgh's examination of ICES described in Section 2.4. Fisheries management's legitimacy, of course, depends on it being based on accurate science to science is a critical entry point in management debates. Many of the attacks on stock assessments would not stand up in an open debate and the issues pointed to are very often things, like inadequate date, that scientists readily acknowledge.

What makes pressure from the fishing industry particularly salient is that, unlike most scientific specialties involved in environmental regulation, in fisheries science the fishers also have a broad, experienced based knowledge (EBK) of the subject, albeit one with very different qualities than the research based knowledge (RBK) of the scientists. A basic problem is that fishers view the fishery in ways that are somewhat alien to the perspective reflected in stock assessment models. They do not see fisheries as mathematically tractable systems at all (Smith 1990). Fishers emphasize habitat considerations (Pinkerton 1989) and view the resource on smaller temporal and spatial scale levels than do the stock assessment models (Smith 1995).

Wilson and Delaney (2005) examine in some detail the impacts of accountability from stakeholders on ICES science. They argue that stakeholder pressure is the ultimate source of much of that which shapes the Commission's requirements about forms of advice. In particular, the need to respond to stakeholder accountability is visible in the demands described in the last section that advice shift from the fish stock to the fishery and that advice not be open to various interpretations. This has led

---

[4] We adopt here an analytical definition of stakeholder: anyone who is organized, has interests in policy outcomes and can influence on either the content or implementation of policy is a stakeholder in that policy.

scientists to be concerned with consistency between stocks in ways that are beyond what is required simply by good science. The industry is also behind the demand to examine the impacts of current fisheries management measures when preparing advice. The main impact of stakeholder accountability is the pushing of fisheries scientists to consider more and more extra-biological questions (Wilson and Delaney 2005).

2.3.3. Special Requests

The Commission had raised as one indicator of demands on the scientific advice system the increasing number of special requests that ICES is receiving. Special requests have a number of different sources and some of them reflect political pressures on the system. Issues are raised within debates among stakeholders that need scientific input to help resolve. These requests take two forms. The first (Table 4) are fast track requests that need immediate attention. Most of the ones listed in Table 4 had time frames from three to six months, DG Fish's first request was needed in six weeks. Upon receiving such requests the ICES Secretariate must organize a team to carry the request out among appropriate scientists working in various institutions as they have no "in-house" capacity for responding to such requests.

ICES is not the only part of the advice system that feels the strain of an increased number of special request. A scientist at an NFI tells related in an interview that in a period of two weeks his lab received 78 written questions from their ministry that came from questions from members of the national legislator. This was an unanticipated task which alone took up four person weeks and for which there was no particular budget line.

Table Four: Fast track requests to ICES in 2005	
CUSTOMER	**REQUEST – Fast track**
DG Fish	1. Compile status list of EU Fish stocks
	2. Sole in IIIa – 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 Fish and Norway	1. Long-term management advice
DG Env	1. Influence of sonar on marine mammals and fish
NEAFC	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
IBSFC	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 North East 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	Catch of NEA cod and haddock for 2006

Thanks to ICES for providing this information.

The other type of special request is simply requests that are not a part of the recurrent stock assessment and other regular advice activities. They do not require particularly urgent attention.

Table Five: Non-recurrent Requests to ICES in 2005	
IBSFC	1. Keep IBSFC updated on progress with revising estimates of smolt production potential in wild salmon rivers
	2. Information on the development of fishing practices for salmon in the gulf of Finland and assessment of the consequences of such development of catches of wild and reared salmon
OSPAR	1. Identification of suitable biological effects monitoring techniques for CEMP, and integration of biological effects measurements with chemical monitoring
	2. Guidelines on frequency and spatial coverage of monitoring for nutrients and eutrophication parameters
	3. Advice on threats to, or decline in the OSPAR area of, seamounts
	4. Scientific aspects of risk management of ballast water
	5. Review of the outcome of the ICES/OSPAR Workshop on the development of guidelines for integrated chemical and biological effects monitoring, and finalising the guidelines
	6. Assessment of the long term impact of oil spills on marine and coastal life
	7. Consideration of the current developments within OECD/EU regarding endocrine disruptors and whether this is adequate for the marine environment, and advice on any further work considered necessary to address issues specific to the marine environment
HELCOM	1. Include the Baltic Sea in a marine habitat classification and mapping
	2. To evaluate every second year the populations of seals and harbour porpoise in the Baltic marine area
	3. To review and revise the quality assurance section of the PLC Guidelines
Thanks to ICES for providing this information.	

2.3.4. The Politics of Data

Data for fisheries advice is not available in either the quality or quantity the Commission would like to see. They have initiated both a major data collection program with funding (CEC 2000) and created a series of regulations (CEC 2001) with both required and optional data to be gathered. According to one of our respondents from the Commission, 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. The point of these initiatives was to make some investments to change this situation. The impact of stock assessment work on scientists' careers and working conditions is an important theme in Section 3.1 below.

Data gathering is uneven and often politically charged. Some ICES delegates have demanded that this is something ICES needs to police and be aware of so that it can be fixed. The problem begins at the level of the fishing industry. 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. The issue is reflected at the national level as well, some countries in the area have a rule that they can only use official data, so the scientists created a category of "unallocated" data to keep it anonymous. When this decision was reviewed these scientists received comments back that this category was not acceptable. The issue was that this kind of treatment means that the data cannot be evaluated. It creates "black boxes" in the system and undermines scientific credibility even up the ICES level of the scientists involved. One scientist complained that if one identifies a specific country the reaction will just make it worse, including to the scientists while another from the region said that they can never mention the countries for these reasons. 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 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 collected discard data as part of the EC 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.

Fisheries data also raises questions of confidentiality and control. An ICES official described the outlines of an agreement between the Commission and ICES about the use of community fisheries inspections data for scientific purposes is long and complex. It specifies exactly what the Commission is willing to provide and requires that ICES not publish the data and restrict access to members of relevant working 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 vessel monitoring system (VMS) satellite tracking data.

Scotland has been a particular place of contention in respect to discard information. Scotland was the earliest place to begin a regular observer programme that gathered such data. Once the data was made available it 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 was being used against them in a way that penalized them more than other fishers who had not provided the data. In at least one case this led one of the fishers organization 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 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 negotiations and the end of the 2002 and setting quotas for 2003 seemed to have been a watershed event. At that point it became clear that fisher's organizations no longer wanted to provide cooperation at previous levels. It was not the discards issue, per se, but a general attitude toward government establishments that contribute towards quotas and management. The data goes through the labs and the fishers feel that they are providing information that is going to 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 has taken place in Northern Ireland where fisheries officers were kept from sampling landings through threats of violence. A scientist involved in the working group says that this activity has had an effect on the Irish Sea stock assessments.

2.4. The Larger Scientific Community

2.4.1. The Importance of the Larger Scientific Community

The larger science community is an important reference group for scientists and hence an institutional influence on how science is carried out in support of the CFP. Related but separate groups are pointed to here. The most critical one is, of course, the "scientific community" writ large. This includes general understandings of what scientists are and what they do, norms pointing at what it means to be a "good scientist" who does "good science", understandings of how one's career should progress, the importance of being published in peer reviewed outlets, etc. Many of these things prove to be of great importance in Section 3.1 on institutional changes in fisheries scientists careers and working conditions.

Reference to the broader scientific community can at times be brought directly into the politics around the science being used in support of the CFP. 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:

> Q15. 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. (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:

> Q16. 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 realized 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, an activity that he had been willing in the past to spend a good deal of time on but having limited personal rewards and some substantial professional costs in terms of chances to do 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 sufficiently defend their work 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 as just one of six reviews, albeit the review by the most prestigious outside group. Another detail worth pointing out is again the importance of the text.

While the Memorandum of Understanding may insist on numbers, it is these numbers presented in the midst of complex textual caveats about their uncertainty and recommendations to reduce effort that represent to him the real advice for which he is willing to be held accountable for as a scientist by other scientists.

Other sub-groups of scientists are also important, for example scientists 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 are sets of common understandings reproduced through things like organizations, task-oriented professional networks, conferences, and journals that form loose communities with shared ways of looking at problems. The importance of such 'epistemic communities' in environmental policy has been recognized for years (Haas 1989). Their particular importance in fisheries science is reflected in many ways but two prevalent general attitudes emerge from these larger communities and have a strong and perhaps growing influence on the fisheries scientists working in Europe. These two attitudes are the general support for the ecosystem approach to fisheries management and the precautionary approach to environmental risks.

2.4.2. The Ecosystem Approach to Fisheries

That fisheries management should move in the direction of an "ecosystem approach to fisheries", to use the FAO's term (Garcia et al. 2003), can be described as a broad but shallow consensus among fisheries scientists. The consensus is broad in that nearly everyone agrees that it is a good idea, 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" in itself is a reaction to the idea implicit in the earlier idea of "marine ecosystem management" that it is possible to model and manage an ecosystem as a unit. A number of the activities around developing the ecosystem approach, e.g. the REGNES group within ICES that is trying to build an ecosystem model of the North Sea, have at least as an initial perspective the idea of managing to an ecosystem model. 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 term "ecosystem approach" is also often used as a way to justify the inclusion within fisheries management policy of a sub-set of important related issues, e.g. the impacts of fishing on the sea bottom, bycatch of marine mammals and birds, and many others, that need to be addressed but are outside of the traditional fish stock focus of management. In the last few years it has also become associated with the movement toward greater use of marine protected areas.

The ICES leadership is trying to balance both approaches while incorporating an even broader set of issues. At an ICES Management Committee on the Advisory Process meeting in 2004 scientific advice related to the ecosystem approach was discussed in terms of being "adaptive" and having a "social balance" and "focus on the process". They were trying to wrestle directly with the institutional implication of an ecosystem approach. From a scientific perspective they described groups like REGNES as one thrust, the long term integration of the fish stock assessment groups as a second, and groups dealing with specific issues such as seabird ecology as a third.

> Q17. *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 system level. To start in a humble way. *(Observers notes at the MCAP meeting, September 2004)*

Our survey asked three scale questions related to different aspects of the implementation of the ecosystem approach. While responses to the three were correlated, this correlation was not high enough to indicate an underlying attitude toward ecosystem management in general. The same result was found in similar research in the United States (Wilson et al. 2002). Average scores on all three of these scales were in the direction of greater support for the ecosystem approach (Table 6). When scores were compared by the type of last attended working group no difference was found about the basic idea of moving way from species by species management. More complex statements, however, showed systematic differences. On both a scale dealing with the ecosystem approach and bureaucratic reorganization and on a scale dealing with using the ecosystem approach on large geographical scales, scientists whose last working group had been an assessment working group, while still on the ecosystem approach side of the centre of the scale (4), were less supportive than the other scientists (Table 6).

Table Six: Responses to Attitude Scales about Ecosystem Management by Type of Last Working Group

strongly disagree = 1.....7 = strongly agree		Type of last Working Group: None	Non-Assessment	Assessment Working Group	Total	P
It is critical that fisheries management retain species by species management as its basic approach	Mean	3.72	3.44	3.71	3.61	0.31
	N	146	166	121	433	
The requirements of implementing an ecosystem approach to fisheries should be a central principle when reorganising national and international fisheries management agencies	Mean	5.69	5.46	4.93	5.39	0
	N	144	166	123	433	
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	3.14	3.02	3.63	3.25	0.01
	N	144	165	121	430	

Excluded in all questions are 20 who did indicate participating in a working group but who did not make clear which kind of working group and six who failed to answer whether they had been in working 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.

When the same scales were broken by type of employer systematic differences were found for the first two, while the third scale revealed differences between two categories and all the other categories combined (Table 7). For all three scales, 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 Seven : Responses to Attitude Scales about Ecosystem Management by Type of Employer										
strongly disagree = 1.... ....7 = strongly agree		Type of Employer								
		NFI	Academia	NGO	EU	Other Private	Other Governm.	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.

Tables 6 and 7 tell almost exactly the same story found in the research in the United States. 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 7 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 organized and managed.

The scientific advice issues that arise in relation to the implementation of ecosystem management are illustrated by the following discussion at ACFM:

> Q18. *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 in to 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 an 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 standardize these things. *Scientist Five*: ACE...has identified some steps ICES can take to go 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. *(From observer notes ACFM Sept. 2004)*

This is a group of people who have a vague idea of the direction they want to go, but a real challenge figuring out how they are going to go there. The discussion wraps together a complex mixture of questions such as: modelling *vs* quantitative descriptions; the respective roles and interactions between scientific advice and political realities; the quality of advice *vs* the available resources; and the respective roles of ACFM *vs* Advisory Committee on Ecosystems (ACE).

2.4.3. The Precautionary Approach

The precautionary approach was introduced in the Rio Declaration (UN 1992). The United Nations Conference on Straddling Fish Stocks and Highly Migratory Fish Stocks (UN 1995) first articulated the "Precautionary Principle" for fisheries with the following definition: "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 implementation of the precautionary approach in management is a constant subject of debate. In Europe, a conceptual framework has been developed and used for the inclusion of the precautionary approach in ICES advice that uses the identification of precautionary reference points (e.g. the Blim and Bpa often referred to in this paper). This conceptual framework reflects what Degnbol (2003) describes as stochastic predictability "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 the as a supplementary consideration within the standard and traditional framework of stock assessment models and their associated management techniques. A constant tension in the discussions among ICES scientists in the preparation of advice is the movement between precautionarity as it is expressed in through stochastic predictability and the underlying issues of uncertainty that resist being expressed as error terms in stock assessment models.

That the precautionary approach is a good idea is widely accepted among fisheries scientists. ICES is publicly committed to the precautionary approach (e.g. ICES 2002). In the US research, 80% of the marine scientists surveyed agreed with the statement "It is critical that fisheries management be risk-averse and choose lower fishing pressure when stock condition is uncertain" and 44% strongly agreed. The same statement on a seven point scale has a mean of 5.66 (Table 8), hence US support seems slightly higher but we have not yet tested that directly.

Table Eight: Responses to Attitude Scales about the Precautionary Approach by Type of Employer									
		Type of Employer							
		NFI	Academia	NGO	EU	Other Private	Other Governm.	Total	P
It is critical that fisheries management be risk-averse and choose lower fishing pressure when stock condition is uncertain. strongly disagree = 1.... ....7 = strongly agree	Mean	5.71	5.83	6.3	5.3	5.44	5.00*	5.66	0.12
	N	220	105	10	33	54	17	439	
To what degree should judgements made in preparing scientific advice be influenced by the precautionary approach? never = 1.... ....7 = always	Mean	5.94	6.21***	6.60*	5.39***	5.49***	6	5.92	0
	N	219	104	10	33	55	17	438	

* 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 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.

Table 8 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 working 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. In a similar pattern to the ecosystem management variables, scientists working for the European Commission were still above the centre of the scale on this question, but below all other categories. The highest scorers where the NGO scientists, six of whom chose seven, with the other four choosing six.

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 with and the pressures and temptations that they struggle with in the implementation of precaution:

> Q19. *Scientist One:* Would it be better to highlight that the estimate is close to Blim and if 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 notes, ACFM October 2004)*

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 culture of the scientific community. Precaution is an area where they have very strong prescriptive opinions and this tension is very difficult and sometime 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 stock condition represents. The scientists themselves, particularly when discussing it in the terms given them by Degnbol's (2003) "stochastic predictability" approach often seem to treat it as an objective outcome rather than a political judgement. Some of their observers, however, can be very critical of the degree to which the scientists lean toward the precautionary approach. We asked one biologist employed by the fishing industry who was observing ACFM what surprised him and he replied:

Q20. I am surprised by the reluctance to increasing quota. 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 next two quotes illustrate how this form of pressure on the science boundary once again leads to a desire for a reflexive interaction between the advice givers and the advice takers.

Q21. *Scientist One:* Another point is about the precautionary approach being the responsibility of managers. Many do not agree, but the precautionary approach is about uncertainty and the managers don't know what this is. So they can talk about different ways to respond to uncertainty as if this is the space where they can manoeuver. We need to be able to react more tightly. *Scientist Two*: The characterisation of uncertainty is so difficult, the document should be so clear that the managers can take their responsibilities. They do not need scientists in order to take higher risks. *(Observers notes, Study Group on Industry Information, 2004)*

Scientist one is arguing that managers do not really understand the precautionary approach because they see uncertainty as an area where their options are expanding, while precaution would dictate that uncertainty is where options are reduced. Therefore the scientists have got to find ways, seemingly, to make their advice every tighter in response to greater uncertainty. They are faced with a paradox, the less they know the stronger their opinion should be. They recognize as well that it is the managers who are responsible for acting on the opinion. The scientists are also faced with some direct and even strongly felt disagreements about the precautionary approach:

Q22. *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 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 notes, MCAP meeting, September 2004)*

In this discussion the desired reflexivity almost become a form of bargaining. "We will advise X unless you promise Y" seems to be the way Scientist One wants it, and he calls upon the precautionary approach for his justification. Scientists Four's response to such an idea is ready and blunt. It is not ACFM's job to check later if the managers used their advice in the a way consistent with the precautionary approach. Scientist One remains unconvinced.

A small number of scientists, among them perhaps are the five percent of our sample who scored three or less on the statement "To what degree should judgements made in preparing scientific advice be influenced by the precautionary approach" are extremely critical of the precautionary approach. As we might expect from Table 8, the following quote comes from a scientist who works for the European Commission:

> Q23. *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 90s - fisheries science - than we are today.

Here we see a frustration with what the precautionary approach has done to fisheries science, not simply in respect to advice, but as a discipline. On the one hand he explains that when the precautionary approach started it stalled some initial attempts in the early 90s 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 90s 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 its energy on the sterile pursuit of precautionary reference points while issues like multispecies interactions were neglected. If this scientist is correct in his historical interpretation, the way that the precautionary approach as stochastic predictability - strongly stated the incorporation of scientist's political positions directly into stock assessment models - contributed to inflating the science boundary had important implications for fisheries science as a whole and not just for the advice process.

3. Institutional Changes in How Scientists Work

3.1. Career and Working Conditions

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). In this section we examine links between perception of working and career conditions and, respectively, forms of advice, resources, and pressures from the EU system (Figure 2).

As mentioned earlier, we divided the scientists who responded to our survey in the following categories based on present employer: NFI, 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 9 describes the composition of the surveyed scientists in relation to employment and working group participation.

There are significant differences between the employment categories of scientists when it comes to working group participation. NFI scientists attended working group meetings more than others. This is especially true for assessment working groups. Almost three-quarters of the surveyed scientists, who confirmed having attended an assessment working group, work in NFI.

In contrast to NFI scientists, less than half of the scientists employed in academic institutions have attended a working group meeting in the last five years. When scientists employed in academic institutions confirm attending in a working group meeting it is rarely an assessment meeting. There is consequently an important difference between our two largest categories of scientists, NFI and academia, when it comes to working group participation and especially assessment working group participation. NGO scientists stand out as none of them had attended a working group meeting in the last five years (Table 9).

3.1.1. Assessment work

In our survey we asked the following of our respondents: “Please rate your overall job satisfaction.” The results of the question related to place of employment are reported in Table 10.

Table Nine: Composition of Survey Sample - Employer and Working Group Attendance									
		NFI	Academia	NGO	EU	Other Private	Other Gov.	Total
Not attended working group meeting	Row percentages	27	39	6	8	15	5	100
	Column percentages	18	54	100	34	40	47	34
Attended working 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
Attended assessment working group meeting	Row percentages	73	6	0	9	12	0	100
	Column percentages	42	7	0	34	27	0	29
Total		50	24	2	8	12	4	N = 418
		100	100	100	100	100	100	

Percentages are rounded and do not always add up to 100. Excluded: 20 who did indicate participating in a working 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 working group or not; and 21who did not identify their employer.

Table Ten: Means of Scales of Perceptions of Working Conditions by Type of Employer

		NFI	Acad.	NGO	EU	Other Private	Other Gov.	Total Mean	Total N	Total P
Please rate your level of overall job satisfaction. 1 = I am not at all satisfied, 7 = I am very well satisfied	Mean	5.12***	5.35	6.10*	5.15	5.67**	5.65	5.29	439	0.02
	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 included. 1 = decreased a great deal, 7 = increased a great deal	Mean	5.3	5.59**	5	5.70*	5.13	5.08	5.37	345	0.08
	N	179	81	8	27	38	12			
To what extent does your employer encourage your participation in externally funded research? 1 = it is strongly discouraged, 7 = it is strongly encouraged	Mean	5.71	6.13***	4.30***	5.52	5.52	4.76***	5.7	434	0
	N	218	104	10	33	52	17			
Approximately how many days job related travel did you do 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? 1 = I would enjoy travelling more, 7 = this is far too much travel	Mean	4.23**	3.95	3.8	4.27	3.85	3.82	4.1	433	0.25
	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? 1 = severely hinders, 7 = strongly encourages	Mean	3.82**	4.90***	3.22	3.61	3.71	3.5	4.03	431	0
	N	217	104	9	33	52	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 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.

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 working groups as shown in Table 9. Indeed, NFI scientists who last participated in non-assessment working groups actually rate their overall job satisfaction somewhat higher than those who have not participated in working group meetings at all in the last five years. This pattern for NFI scientists fits with the facts that NFI scientists are relatively interested in doing research (Table 14) and that the non-assessment working groups can be thought of as "science" working groups (Section 3.2.3). Hence, participation in non-assessment working groups could be expected to be related to higher job satisfaction among NFI scientists.

Table 11 shows job satisfaction related to working group participation. Those who participate in assessment working groups rate their overall job satisfaction lower than other scientists. The assessment element is consequently pivotal, not working group meeting (assessment as well as non-assessment) activity in itself. In order to investigate if the driver was type of working group or place of employment we analysed job satisfaction for each employment category in relation to working group participation. The results for NFI scientists are reported in Table 12.

Table 12 shows that last working group being an assessment working group is related to lower overall job satisfaction. The same pattern applies to all employment categories; scientists who participate in assessment working groups rate their job satisfaction lower than other scientists from their own employment category. The conclusion that the problem is not just going to meetings is also supported by Table 12.

One scientist did not beat around bush when describing his experience with assessment work: "I moved away from assessment and advisory groups six years ago; feeling better!" Another scientist with a very low rating of job satisfaction described some experienced difficulties.

> Q24. 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!

This scientist lists several problems, which are mentioned by other respondents too. The data needed to make assessments is perceived as getting worse, which makes it impossible for the assessment working groups to deliver assessments of high quality. The pressure to deliver assessments on the basis of a shaky data places the scientists in a tradeoff situation having to chose between their scientific integrity and doing what they are instructed to do (Section 3.3). One scientist indicated in an e-mail to us that they "sell more certainty than we can warrant" as a result of these pressures.

Table Eleven: Means of Scales of Perceptions of Working Conditions by Type of Working Group					Total		
		Not attended working group meeting in last five years	Last working group did not deal with assessments	Last working group did deal with assessments	Mean	N	P
Please rate your level of overall job satisfaction. 1 = I am not at all satisfied, 7 = I am very well satisfied	Mean	5.45*	5.42	4.93***	5.29	437	0
	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. 1 = decreased a great deal, 7 = increased a great deal	Mean	5.26	5.36	5.45	5.36	343	0.46
	N	105	133	105			
Approximately how many days job related travel did you do in 2004?	Mean	39**	41	51***	43	435	0
	N	144	167	124			
How do you feel about having to travel this much? 1 = I would enjoy travelling more, 7 = this is far too much travel	Mean	3.62***	4.2*	4.38***	4.06	429	0
	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? 1 = severely hinders, 7 = strongly encourages	Mean	4.54***	4.14	3.25***	4.02	429	0
	N	143	164	122			

* 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 a working 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 working 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 the employer's support for peer reviewed publications.

Table Twelve: Means of Scales of NFI Scientists' Perceptions of Working Conditions by Type of Working Group							
		Not attended working group meeting in last five years	Last working group did not deal with assessments	Last working group did deal with assessments	Total		
					Mean	N	P
Please rate your level of overall job satisfaction. 1 = I am not at all satisfied, 7 = I am very well satisfied	Mean	5.08	5.46***	4.78***	5.11	208	0.01
	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. 1 = decreased a great deal, 7 = increased a great deal	Mean	5.14	5.18	5.44	5.3	169	0.31
	N	28	61	80			
Approximately how many days job related travel did you do in 2004?	Mean	41	41	50**	45	207	0.1
	N	36	83	88			
How do you feel about having to travel this much? 1 = I would enjoy travelling more, 7 = this is far too much travel	Mean	3.64***	4.24	4.42*	4.21	206	0.01
	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? 1 = severely hinders, 7 = strongly encourages	Mean	4.69***	4.02	3.23***	3.81	205	0
	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 a working 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 working group or not, 21 who did not identify their employer, and 209 who did not work in NFI. 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.

Work pressure related to assessment working groups is experienced as too high and increasing, as Quote 24 suggests. At least two other scientists complained in interviews about the overall amount of work in connection with assessment working groups. One of the scientists told us that he had had to take a couple of months off because of stress. Another scientist told us that the WGNSSK meeting had become increasingly chaotic in the last years and that it was impossible to stick to the planned - already long - working hours. This was underlined by a scientist who after a WGNSSK meeting approached our observer to say that the previous day's work had carried on to four a.m. This was the only time a scientist as any meeting we were observing, and all scientists at these meetings had been told what we were doing, approached us to make sure that something went into our report.

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

> Q24. 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 scientists describe a situation where he never feels that the job is done. This is of course linked to the time constraints in an already 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 what it means to have done enough in attempting to describe nature.

In our survey we asked: "In the previous three years how has the pressure you experience on the job changed?" In order 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. However, pressure can increase simply because the scientist gets more experience and thereby seniority even though he or she remains in the same position. In fact, increased pressure is not *per se* experienced as negative. One respondent indicated that his job pressure had increased substantially but commented it in this way:

> Q26. "Being a young scientist the greatest changes in my work activities have been increases in responsibility, which I consider very valuable. The diversity of activities (sampling, writing proposals, analysing, publishing, giving presentations, coaching students, etc.) has also increased.

Nevertheless, the correlation between job satisfaction and experience of changes in pressure at the job is -.23 (p=.01), suggesting that increasing pressure at work is at a general level weakly linked to lower overall job satisfaction.

The survey data, however, does not confirm that a significant relationship exists between assessment working group participation and higher rating of increase in job pressure (Table 11). The same is true if we look at NFI scientists only (Table 12). This is interesting since it could be expected that those closest to the advisory process had experienced the highest increase in job pressure since the system is viewed as being increasingly short on resources (Section 2.1).

Table 11 shows that fisheries scientists travel on average 43 days per year in connection with their work. Those whose last working groups was an assessment working group travel the most (Table 11), on average ten days more per year than others. Those in assessment working groups are more dissatisfied with the amount of traveling than others (Table 11). One scientist involved in assessment work complained in an interview about having to travel on short notice:

> Q27. For the good of your heath you can't carry on like that. Having 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.

Quote 27 highlights that the problem is not necessarily the amount of travelling as much as the circumstances around the travels. Some British scientists were of the opinion 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 time to the same extent as before .

Table 10 shows that there are large differences in the amount of travelling 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 group of other private travel most with a mean of 57 days a year. This does not, however, result in a clear pattern in the feeling about the amount of travelling as one could expect based on the pattern identified in relation to working group participation (Table 11) where more travelling was linked to greater dissatisfaction with the amount of travelling.

Participation in assessment working groups is negatively influencing scientists' opportunities to publish peer reviewed publications. In the survey we asked respondents: "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 11). However, for the group of scientists whose last attended working group dealt with assessment, the mean is 3.25. Scientists who did not attend a working 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 12).

One scientist currently chairing an assessment working 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". It seems likely that this situation will have negative impact on his ability to publish peer reviewed publications although the respondent was not specifically referring to this.

The training of scientists for assessment work is an issue that ICES is emphasizing. Assessment work requires specific skills, for instance sufficient knowledge about mathematics. This is often not sufficiently part of the educational background 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 also 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 is conducting various courses related to the training of assessment scientists but ICES cannot fund these courses and has to rely on the NFIs. However, one scientist described how he brought junior staff to meetings as a training arrangement, which indicates that the 'by accident' description may not always describe the way things work. Some training takes place through a master-apprentice type of relationship.

Overall, our data indicates that the last working group being an assessment working group has a negative impact on fisheries scientists' perception of their working and career conditions. On the most general level those who last participated in an assessment working group rate their overall job satisfaction considerably lower than other scientists (Table 11). This group of scientists is also less satisfied with how much they travel. The work they engage in seems finally to be hindering them more than others from producing peer reviewed publication. NFI scientists constitute the majority of the participants in assessment working groups and this group shows also relatively poor results in relation to job satisfaction.

3.1.2. Changes in funding structures

One of the recent years most important changes in fisheries science has been the change towards reliance on "soft" money that scientists or institutes have to apply for. This has affected the scientists' working conditions in several ways. The comment of the scientist in Quote 1 is worth repeating here:

> Q28. When I first came here, because 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, on the contrary. This particular scientist suggests that scientists were previously able to decide for themselves and produce better research results instead of being caught in fulfilling contractual demands.

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. The answers to this question fell basically in two categories. One group of scientists focussed on concrete changes in fisheries research issues, for instance the increasing focus on ecosystems. Another group, which is interesting in this context, focussed on changes in their working conditions. Two issues reoccur in many of the answers, namely funding sources and administration. The answers of several respondents included both, an example being this one:

> Q29. 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.

In order to analyse differences between those mentioning funding and/or administration and those who did not, we decided to code the answers to this question. Answers with reference to changes in funding sources were coded as such. Answers merely mentioning declining funding or similar were not included. Answers mentioning management, administration, bureaucracy or unnecessary meeting activity without value added were coded as references to administration. Two coders were used to check for bias.

Table Thirteen: Percentages Mentioning Issues of Funding and Administration by Type of Employer

Subjects mentioned in unsolicited 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."		NFI	Not NFI	Academia	Not academia	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. NFI 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 question generated 60 answers that mentioned the change in funding sources. This is a considerable number given that the responses were the result of a completely open ended question. The scientists were not asked to evaluate the changes experienced, which makes it difficult to say how they perceive them. Several mentioned that the need to write applications for funding puts additional pressure on them. Some also argued like Quote 28 that the changes in funding sources has 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 13 shows that 24 percent of the scientists in academic organizations mentioned this change while it was only 14 percent of the scientists in the other groups combined.

As discussed in Section 2.1, in our survey we asked our respondents to rate how much their employer encouraged 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 to seven, which indicates that this is on average encouraged. On average NFI scientists were close to the overall mean, whereas scientists from academic institutions came out with a significantly higher rating (Table 10).

Table 10 shows that scientists employed in academic institutions stand out as the group that feels the greatest encouragement to participate in externally funded research. Scientists employed in academic institutions are simultaneously the second highest employment goup in experiencing an increase in pressure at the job (Table 10). It seems likely that there is a connection between the feeling of increasing pressure at work and the experience of stronger emphasis on external funding at least for scientists employed in academic institutions. This seems certainly to be the case for the scientist employed in an academic institution, who stated that he felt a "pressure to do it, but no other tasks are taken away to permit this". The survey data supports that those, who mention funding as an important change, rate the increase in pressure at work higher than others (5.82 *vs* 5.38 / 7 P = 0). Scientists employed in NGOs are the ones, which rate the support of their employer towards participating in external research lowest. This is most likely linked to the fact that scientists in NGOs are not employed primarily to do research but rather to do advocacy.

Many fisheries scientists feel affected by the change in funding sources. Some scientists mention that the continuos need to apply for funding is taking time away from research and that it adds to an already high pressure at work. It is noteworthy that scientists in academic institutions mention funding issues more than other categories (Table 13). They also feel greater encouragement / pressure to participate in externally funded research than any other employment group. At the same time their perception of increased pressure at work is higher than all other employment groups besides EU scientists.

Table Fourteen: Means of Scales of Perceptions of Research and Career by Type of Employer								
	NFI	Acad.	NGO	EU	Other Private	Other Gov.	Total	
							Mean	P
How important is it to you that much of your time be 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 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 refer 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 Chronbach's Alpha of .634.

3.1.3. Administrative tasks and outside pressures

In the previous section we mentioned that the open ended question on most important changes in work activities generated many answers mentioning administration - along the lines of Quote 29. Almost one third of those who answered the question mentioned administration. Scientists employed in NFI are over represented among those mentioning the issue. Thirty-nine percent of NFI scientists mention administration whereas it is 'only' 26 percent of all other groups combined. One scientist described the development in this way: "Much greater emphasis on administration and monitoring of work targets with no improvement in work output...". The fact that shift in funding structures is mentioned in connection with increased administration by 29 respondents seems to suggest that there is a close connection between the two (Table 13).

In most institutions advancement means more administrative duties. It is in this way possible to be promoted out of research. If scientists prefer doing research this will create a further disjunction between activities and career goals.

The two first questions reported in Table 14 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, indicating that they put emphasis on doing research. Several respondents add as a comment to the question that they have already passed on an advancement possibility because of the administrative duties involved. NGO scientists score in opposition to this relatively low on these questions, which is hardly surprising. As mentioned above, NGO scientists are not primarily employed to do research. A condition they are likely aware of when they apply for the position.

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

The most interesting information in Table 14 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 ends up with the biggest difference between wishes and expectations. NGO scientists end up with a very small difference, mainly because they do not really emphasize doing research. The relative match between wishes and expectations may contribute probably to the high rating of overall job satisfaction of NGO scientists (Table 10).

Administrative matters constitute an increasing part of fisheries scientists duties; a development which is closely linked to the change in funding structures. Most scientists would very much prefer doing research. As much as one quarter of all the surveyed scientists indicate that they would definitely be willing to forego career advancement in order to spend more time doing research (counting only those who checked seven on our scale to seven; if six was also included the figure goes up to over 50 percent).

Administrative pressures are to some extent related to pressures from the industry, the EU and other elements of the environment in which the scientists navigate in. As discussed above in Section 2, this means that particularly the 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 with changes in funding structures and increasing administration these developments add to the pressure that the scientists experience. Seen from the perspective of those exerting the pressure it is clear that these pressures are meant to improve the output from the assessments. It seems, however, that these pressures are having a number of unintended, negative consequences on the lives of these scientists.

3.1.4. Gender

Fisheries science in support of the CFP has noticeable gender patterns. Participants in all of the meetings we observed were largely middle-aged white men. Twenty percent our survey respondents are women and this distribution holds fairly closely 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 percent are women. While among the most senior third, respondents who received their last degree 17 or more years ago, only five percent 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 assignments of men and women to types of working groups (Table 15). Women are over-represented among those who do not attend working groups and under-represented among those attending the more desirable non-assessment working groups. Assignments to assessment working groups show no gender pattern. This pattern is the same, but less pronounced, among the more junior third. It is statistically significant at .10 only among NFI employees (Table 15). The overall picture seems to be a slowly changing working environment where women's opportunities may be increasing, but further research would be valuable here.

Table Fifteen: Patterns in Working Group Assignments by Gender

	Type of Last Working Group			Total	P
	No WG	Non-Assessment WG	Assessment WG		
All Respondents					
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 a working group but did not make clear which kind of group, 6 who failed to answer if they had been in a working 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 NFI.

3.1.5. Summary of Working Conditions

Fisheries scientists on average score their job satisfaction above the mid-point of the scale, with NGO scientists being the most satisfied with their job and NFI scientists the least satisfied (Table 10). All groups of scientists have experienced increasing pressure at work in the last years. This is to some extent related to a change in the funding structures and a connected increase in administrative duties. This seems to be a development that has affected all categories of scientists.

Scientists whose last working group dealt with stock assessment have, however, at the same time faced a range of other pressures, which clearly impinge on working conditions and job satisfaction. These additional pressures is most likely the explanation for the lower rating of job satisfaction of this group. They travel more than other categories of scientists. They have also a more negative perception of how their job affect their possibilities to produce peer reviewed publications. That NFI scientists are the employment group that has the lowest rating of job satisfaction. As we showed this is to a large extent connected to their greater participation in assessment working groups. NFI scientists, whose last working group was not a stock assessment group have a better rating of the job satisfaction (Table 12).

3.2. Challenges to the Science Boundary

The second set of impacts that can be traced to the institutional environment of science in support of the CFP deals with the content of the science itself. The concept of "boundary work" is described in the description of our theoretical approach in Section 1.1 above. A central assertion we are making, which we hope to justify in this section, is that there are systematic pressures within the institutional environment of science in support of the CFP to "inflate the science boundary". The inflation metaphor means that the pressures seek to define more and more kinds statements as scientific ones. Even while there is this pressure to define more things as science, the institutional environment places a number of many kinds of non-scientific constraints on how the science is to be carried out.

3.2.1.A Theoretical Aside: Why seek to Inflate the Science Boundary?

A theoretical aside may be of interest here to clarify why we consider these pressures be systemic rather than simply the outcome of particular decisions or policies. From the sociological "big picture" perspective, these pressures are in response to institutional imperatives to have certain things stamped "science" in order to enable institutional functioning. These imperatives are rooted in the fact that particular types of institutional coordination require what communicative systems theory calls "empirically motivated ties" rather than "rationally motivated trust" (Habermas 1987). In the former, the mechanisms for the coordination of social behaviour - mainly those used by markets and bureaucracies - involve the use of calculable values to exert pressure on people without having to convince them of anything beyond the fact that they are under real pressure. This is strongly related to a distinction that has a long history in social theory. Burt (1982) cites Festinger (1950) as drawing a distinction between "facts" and "social attitudes" based on whether or not it is possible in principle to check the veracity of a communication against some objective reality. The empirical ties that allow the pressure to be exerted have to refer to some objective reality.

The bottom line is that certain institutions, particularly in our case institutions using bureaucratic authority to coordinate behaviour, have to be able to make operational references to "facts". The more questions that can be defined as issues of fact with objectively true answers the easier it is for such institutions to function. Therefore such institutions 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. In Habermas' term to "technicize" these arenas. It is, of course, not only people directly concerned with making bureaucratic management work who seek to technicize these arenas. When scientists are folding the precautionary principle into scientific findings they are doing so that their vision of how risk should be distributed becomes a technical fact written into the functioning of the management bureaucracy.

Scientists, however, have their own institutional imperatives to resist this inflation of the science boundary and will seek to drive the boundary back towards which can be the most clearly demonstrated. From the communicative systems theory perspective (Habermas 1987, p 182-184) this is because science is one of the institutions most dependent on "rationally motivated trust." As we mentioned above in Section 3.2.1, science is completely dependent on the activity of scientists seeking to convince other scientists that something is true. This is what makes science as an institution sensitive to factual truth in the first place (Wilson 2003, Wilson and Delaney 2005). The success of this activity is directly dependent on science being "differentiated out" (Habermas 1987, p 184) so that it is deals exclusively with questions that are amenable to rationally motivated factual truth. These are truths that in principle can be reached through processes of consensus characterized by disinterest and skepticism and oriented around universal criteria such as precise definitions, the falsification of hypotheses and replicability. Only this narrowness of focus makes possible the internal trust of both people and results that makes scientific inference possible (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.

Our schema (Figure 2) suggests three ways that this extra pressure is articulated within the operation of the CFP: demands related to the forms that the advice takes; political pressures; and, the values of the larger scientific community.

3.2.2. The Science Boundary and the Provision of Advice

The discussion about North Sea cod and multi-species advice contains examples of how the inflation of the science boundary creeps into advice requests. The untenable advice that cod catches be 0 led to the wording that cod catches should be kept to a "minimum". But as one ACFM scientist put it during the Oct. 2003 meeting "I think really we need to say there is not science-based way of establishing what minimum means". The clearest example was when they referred to the difficulty of defining fisheries as a justification for refusing to provide the mixed-fishery forecasts. This was an act of resistance to the use of science to solve the political problem of the distribution of fishing effort among various fleets. STECF then justified their provision of the advice that ACFM had declined to provide by the fact that they were providing mixed-species advice, not mixed-fisheries advice. Focussing on the biological unit of the species rather than the fluctuating half-social, half-technical unit of the fishery, pushed the arena of discussion where a scientific process could, in principle, uncover factual truth.

The category of political pressures covers a number of different challenges to the science boundary. One is the demand that scientists, like the one is Section 2.3.1 who got his "wrist slapped" stay in their place when it comes to the other aspects of setting fisheries policy. Struggles over the science boundary apply as much to people as they do to results. When someone has been stamped a "scientist[5]" this power to designate a fact is to be used at the behest of the bureaucracy, not the individual scientist. 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 the Commission (CEC 2003a, p 15) if the scientist wants "credibility and influence" they must keep their "distance". From the scientists' perspective the distance may make the promise of influence look somewhat empty.

The level of public interest and oversight also brings our non-scientific institutional norms within science. Concern with the consistency of methodological approaches among different species is greater than would be justified by simple concern with consistent quality of analysis (Wilson and Delaney 2005). The possibility of misinterpretation and even misuse of results by other stakeholders pushes scientists to articulate maximum clarity in the midst of great uncertainty in contrast to the basic scientific norm of organized skepticism. Public pressure also leads them to spend time examining issues, for example the impact of certain technical measures (Wilson and Delaney 2005) that they see as less important than other issues. Finally, data is subject to a number of non-scientific controls that protect confidentiality and very often directly reflect political sensitivities.

Finally, the greater scientific community has a great deal of influence on the inflation of the science boundary through the ways that the precautionary principle has been folded into the

[5] One interesting pattern is that within fisheries this designation is often based on employment rather than on education. A scientist in terms of playing this kind of role in the mainstream understanding of science and fisheries policy is something you are hired to do, not trained to be, though obviously the training makes the hiring possible. One scientist, however, told us that not all members of working groups have university degrees.

advice process (see also Wilson and Degnbol 2002). The strong consensus that judgements made in preparing scientific advice (Table 8) should be influenced by the precautionary approach has been the basis of the importation of a political judgement into the heart of the advice process. It is also justified by political mandates and international agreements that require a precautionary stance. These commitments of both belief and law make their way into the science itself. In one way they do so as an "ideology" among the scientists that advice should be as conservative as can be justified. The other way has been a long effort to develop the precautionary reference points. These reference points have been an attempt to create an objective, or an least quantitative, basis for the expression of this commitment to precaution. This approach has led, however, to some rather strained science such as the setting of reference points through the use of break points in the stock recruitment relationship described in Section 2.2.2. This method is meant to develop a *consistent* approach among stocks but the break points are only readily identifiable in a limited number of stocks. Worse, the use of these reference points have deflected attention - in both the creation of advice and the development of methodologies - from many of the most serious sources of uncertainty.

The scientist who had his 'wrist slapped' in Section 2.3.1 reflects this way on the science boundary:

> Q30. 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 Section 3.3 below, however, 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.

No one involved in the production of scientific advice in support of the CFP would suggest that the drawing of the science boundary is not problematic. The Commission sees the problem in the context of the mainstream view of science and policy discussed above in Section 2.3.1. They casts the problem in terms of the clear statements of management objectives. This is a phrase very often heard in critiques of policy by natural scientists. The Commission describes the problem in detail as follows:

> Q31. 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, yield 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).

This is an extension of the mainstream view of the role of science. 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 (Quotes 6, 10, 13, 22) and the Commission (Quote 7) calling for greater reflexivity, conditional advice and ongoing interactions across the science boundary. 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 and fishing activities. The topic under discussion is how to develop a management strategy approach to management:

> Q32. *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 but we have to address the problem of scientific input and the availability of knowledge, a feed back process. *(From observer notes, meeting cannot be identified as he was the only Commission scientist present)*

And in the following quote, from the same Commission scientist in response to the Oct. 2003 ACFM decision about multi-fishery advice. The last sentence borders on lamentation.

> Q33. What we have now is in some ways weak advice, we say you do this, you do that, there is no data except one table offered about interactions. It leaves managers to decide how they can define fisheries and take bycatch into considerations. We have the same situation as last year, we are on our own in Brussels. *(From observer notes, again the forum cannot be identified as he was the only Commission scientist present)*

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 also very abstract and become unclear as soon as they begin to be operationalized. 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. Arguably, a management approach based on *real* "clear objectives" would be very nearly the opposite of an adaptive, ecosystem approach to fisheries.

3.2.3.The Science Boundary and ICES

These pressures on the science boundary have direct implications for ICES. ICES plays a number of roles in European fisheries science. In this paper we have focussed almost exclusively on its role in formulating advice for fisheries management. At the same time ICES is the main professional organization for European fisheries scientists. While fisheries science is in many ways the quintessential applied science, it was created by the recruitment of biologists to help manage fisheries, it is also an academic discipline with an interest in knowledge for it own sake. Many of ICES' working and study groups are only distantly related to the advice giving process. The issue can be very serious. There is a perception among some ICES scientists that the reason that few large scientific research projects are not managed out of ICES is because the national fisheries institutes have in some places very soft support for ICES as a science body because they want to see it as basically a advisory body. The opinions of the NFI about ICES' role is, of course, critical or in some cases determinative because they are the employers of a great many ICES scientists.

The relationship between science and advice within ICES is being raised directly in the context of the ecosystem approach to fisheries, and indirectly in respect to how the precautionary approach is being implemented. An ecosystem approach applies that a greater number of ICES working groups will be involved in fisheries management advice. Some of these groups are anxious to get involved because they feel they have an important contribution to make. Yet this is not always easy:

Q34. *Scientist One*: The organization of an Annual Meeting of Working Group chairs will be seen as a further removal of advice from science and the inability of the advisory chairs to see that as part of the problem. *Scientist Two*: It is going to make the ACE [Advisory Committee on Ecosystems] life a lot easier. *Scientist Three*: It will be down to the level of what are you supposed to write in section X.....*Scientist Three*: It is a good thing and practical. *Scientist One*: The groups that for a long time have felt frustrated about having an impact on the nature and content of fisheries advice will not be there.

(Observers notes at an ICES Consultative Committee meeting, Sept. 2004)

Scientist One sees ICES advice and ICES science as two very different things, but that it is a problem that not enough of the ICES science has been brought to bear on advice. Indeed, he sees them as moving away from one another. This is another understanding of the science boundary that draws the line within ICES itself. 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 working groups that want to have an impact on the "nature and content" of the advice and have felt frustrated that this has not happened. In this case it is simply the practical complexities of the increasingly complex advice system, represented here by the need for the chairs of advisory committees to have greater coordination, that is a block to this greater participation.

Table Sixteen: 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 never 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 are in both questions 20 who did indicate participating in a working group but did not make clear which kind of group and therefor did not fit in any of the three categories and six who failed to answer whether they had been in working 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 16 provides some mixed evidence that Scientist One was not speaking just for himself. The first question relates to the degree that ICES should advocate for fisheries conservation. Regardless of working group, average scores are fairly close to the centre of the scale suggesting at most a slight preference toward ICES increasing its direct advocacy of conservation. But there are significant differences between the three groups. Those who are not active in working groups are the strongest advocate of this position, while those involved directly in stock assessment are its weakest advocates. The middle group are 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 would like to have an impact on the "nature and content" of the advice, moving it toward a stronger conservation posture. However, the same pattern is found in 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 16 suggests that while Scientist One was not speaking only for himself, the scientists are broadly satisfied with ICES' current balance in respect to these two issues.

Internal challenges in respect to the science boundary take other forms. ICES has in recent years become interested in involving social scientists. This has arisen recently in particular in regard to an internal ICES debate around creating management "strategies" that builds on the idea of harvest control rules but tries to place them in a broader strategic and adaptive context. Many ICES scientists see this as requiring input from social scientists. This has not proved to be easy. Non-economic social scientists, economists and natural scientists have very different conceptual styles and they often get financial support in very different ways. It is another and in many ways unfamiliar form of the science boundary, and while many ICES scientists are very open to working with social scientists others are understandably reluctant.

Even the participation of biological scientists in ICES working groups can be problematic. ICES is struggling with guidelines for participation. There has been some experience with "heterodox" fisheries scientists claiming they are being excluded. At the moment participation in advisory working groups is formally determined by the national "delegate", the scientist who is the official government representative to ICES from a member country. The chair of the particular group has a great deal to say about who participates, but in practice this has meant that any scientist who is interested in a group can call the chair and will very likely be able to participate. These things are both controversial. Two related issues are involved here. One is simply funding, the funds for participation in working groups are controlled by the delegates, so a working group chair can nominate but cannot fund someone's participation. The other is that there needs to be workable number of scientists who are able to contribute and ICES' fear is that these limits will led them to be accused of excluding people for politics rather than merit. Of particular concern is the politics among the various EU member states.

> Q35. *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 would 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 are able to contribute. *(Observer notes at an ICES Consultative Committee meeting, Sept. 2004)*

Thus a practical solution is reached that will last until someone decides to make an issue out of someone else's participation in a working group.

The scientific boundary work related to science in support of the CFP involves more than simply determining the what sort of facts and results will be stamped "science" and which one will not. As is often found in other scientific arenas, it also involves the determination of who is a scientist and who is not. Beyond this, the data that the scientists will have available to them, the ways that they will present their work, and even the ways that they will behave as they play their scientific roles are in dispute. Science boundary work is also having a direct impact on ICES self-understanding as an organization and even on how it organizes itself. Practical questions about what ICES science is and who is going to do it arise constantly and lead to ongoing tensions.

3.3. The Implications of not "Doing What You Might Call Science"

As our research progressed it became clearer that both the pressure on the science boundary and the constant demand to produce answers for the next cycle of the TAC machine are having a serious impact on moral. Many scientists are experiencing a real discontinuity between what they see as the science they want to do and their own activities. As one put it:

> Q36. 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 has two 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" that work on developing these methods. This was discussed in some detail above in Section 3.1. The second is a feeling that the advice being produced 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 Quote 11. As the respondent we quote above about number manipulation and giving advice clarified at another point in the interview:

> Q37. 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.

This lack of faith in results arises from the way the entire system handles uncertainty. As another respondent said:

> Q38. 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:

> Q39. There are a lot of people who are directly or indirectly involved in the advisory process because we have to rely on people to do data extraction, so we can manipulate the numbers.

This third respondent points to the cyclical growth of bias and uncertainty that he suggests is built in to 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.

> Q40. 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 noncompliance, which then screws up your basis for setting TACs in the future. So there are 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 I say "no we can't..."? I think with a lot of stops 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:

> Q41. 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 and this survey thing I 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 in respect to the CFP. Several of the statements we have reported on here are emblematic of this. The most striking one in some ways is the plea by the scientist in Quote 11 that they stop pretending they know how many fish are in the sea. This demand was in a voice with no sound of uncertainty at a plenary session in front of 25 of 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 precisely what was going to happen to the results in the larger world after they finished fiddling with the details.

As the evidence presented in Section 3.1 demonstrates, this anomie is happening to the scientists working for the national fisheries institutes directly in stock assessment process. These are the scientists who have the largest gap between their desires to do research and their perceptions of their future chances to do research (Table 14), who are feeling somewhat more pressure from administrative work than other scientists (Table 13), who have the lowest job satisfaction (still above the mean of the scale) and are the least satisfied with their opportunities to do publication.

As section 3.4 demonstrates, discomfort about the science they are doing has a negative impact on their job satisfaction that is independent of these other working conditions.

3.4. Examining Perceptions of these Relationships Using Survey Data

One way to evaluate the coherence of Figure 2 is to examine relationships among scientists attitudes using survey data. Figure 2 is meant to summarize the results of both the qualitative and quantitative methods. As such not all of these relationships can be examined using the attitude scale data. While the relationships depicted in the survey cannot be directly measured, indirect measures for some of the variables can be derived from responses to attitude scales. Using the survey data to examine the coherence of Figure 1 requires some arguments about how we might "operationalise" the boxes in the figure as survey variables. The dependent variable needs to reflect the box on the right hand side "Scientists'Anomie". As a proxy measure for scientists' Anomie we used responses to the following question as the dependent variable in our analysis:

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.

Using this dependent variable opens up possibilities for examining mainly the relationships on the right hand side of Figure 2. Attitudes towards the demands of mandated science, fishers, the precautionary approach, and ecosystem management can be, and were, measured, but we can hardly expect scientists attitudes toward these things having much influence their overall job satisfaction one way or another. Indeed, no such correlations exist. This is also true of any questions that might be related to perceptions of the science boundary, something we did not ask about directly because it is simply too complicated a concept to reduce to direct questions. We must rely mainly on the qualitative research, as well as the inferences that we have made above about attitude differences between scientists involved in stock assessment and other scientists, to make up the bulk of the empirical support for Figure 2. Our use of the attitude survey is restricted to the relationships between working conditions, the experience of unreal results, and scientists' anomie.

For "Working and Career Conditions" we used responses to the following questions:

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?

My job

severely hinders = 1___ 2___ 3___ 4___ 5___ 6___ 7___ = strongly encourages

my production of the level of peer reviewed publications I would like to be producing.

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.

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.

As a proxy measure for experiencing results as unreal we used the following two questions:

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

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 being entered into an analysis model these two questions have to be evaluated as to whether or not they are measuring the same underlying attitude or two different ones. If this is mis-specified then we run the danger of either covering up important differences on the one hand or specifying a collinear model on the other. Several approaches to this question exist. Simple face validity would suggest that given their wording and context they are pointing at the same kind of experience. Responses to the two questions have a simple correlation of .47 just shy of the rule-of-thumb test of .5 that the two scales are measuring the same thing. Hence we cannot conclude that the scales measure a single underlying experience. However, given their high intercorrelation and the fast that these statements are reports of similar experiences rather measures of an underlying attitudes, we think that their combination is a better measure of this experience of unreal results than they would be separately. So we have decided to create a scale of the average of the responses from the two questions as a measure of the scientists experience of the unreality of results.

Table 17 reports the results of a linear regression of the model just described.

Table Seventeen: Regression on Attitude Scales Related to Figure Two

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	Impossible questions and create certainty scale	-0.1	-0.1	-2.6	0

N=270 AR^2=.29 Exclusions: 148 respondents who indicated not having participated in a working group in the last five years, and six who failed to answer whether they had been in working group or not. Sixteen respondents 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.

The results indicate that, as expected, the Working and Career Condition questions had a direct impact on job satisfaction. The key result, however, is clearly that controlling for the impact of differences in working conditions, the more scientist feel that they are being asked to create certainty and answer impossible questions the less satisfied they feel with their job.

4. Conclusion

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:

> Q40: "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 I think. I 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 seeking to provide scientific advice is support of the CFP this is not an untypical experience. We hope the present report has shed some light on the aspects of the fisheries system that this scientist is experiencing as a source of "manipulation".

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 protect the counterfactual devices that allow their institution to function. These are the things that we have variously described here in terms of openness to new claims, the fair evaluation of arguments based on rigorous and prescribed methodology, a background assumption of trust for other scientists, and disinterestedness and skepticism in respect to results. In this respect the production of scientific advice for the CFP can be seen as a process of cultural alienation. In a way similar to the knowledge of fishers, the knowledge of scientists is disembedded from the knowledge culture that created it and 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 precautionary approach is also going to continue to create difficulties for how science is done. Many of the real sources of uncertainty in fisheries simply cannot be reduced to probabilities, so the precautionary approach will never be encompassed through stochastic predictability (Degnbol 2003). 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.

Several of the quotes we have offered point to one basic way that the problems presented by the inflation of the science boundary can be ameliorated, as it will almost certainly not be resolved. This is the desire expressed by scientists throughout the advice system for increased reflexivity across the science boundary. This means getting at what scientists are really after when they want to condition their advice on what managers are going to do with it. The scientific advice needs to be produced in a much more interactive manner with managers and other stakeholders. This means that this utopian mainstream view of the role of science needs to be modified. If we continue to demand that scientists offer up to a political process the "objectively real" for all to see then we will continue to force them to create something alien in which they do not believe. And the evidence we offer here in respect to job satisfaction suggest that we will loose some of them in the process.

Perhaps rather than asking the scientists to be less than what they understand "scientist" to mean, we need to ask them to be scientists even more than they are now. Of all the institutions in the fisheries system it is science that has built itself on radical transparency. Scientists are the transparency experts, they know what it really means to explain how they know what they know. Transparency, and the accountability it makes possible, is the key to making multiple stakeholder, adaptive management of the marine environment a possibility. Nothing else can make the needed information available in a sufficiently timely manner. We should think about how scientists can move in a new direction toward facilitating interactions between stakeholders in respect to building an accurate common picture of the marine environment rather than being the ones who tell the stakeholders how it is. Such a shift would not necessarily mean a large change in scientists' activities - they would still need to carry out ongoing research - as much as a shift in the roles they play with respect to other stakeholders. Shifts would be required in both scientists' attitudes towards how their work will be used and in the attitudes of others about their expectations from scientists. Indeed, we would argue that there is a good deal of movement in this direction already both within and beyond ICES.

A basic question, then, is how this creativity can be further released into the various movements to reform fisheries management. This research has uncovered three different and competing institutional needs that at the moment are either not carried out or carried out by ICES alone. The first such role is providing scientific advice for the CFP. Many concerns exist in respect to the roles of ICES, the NFI, the Commission and STECF. We will not resolve these here, but we will say that there is an advantage to having the checks and balances that several different scientific bodies make. While it is certainly understandable that the Commission would like a more unified and efficient system, facilitating a new, multi-stakeholder role for science will require a flatter rather than a more hierarchal structure.

The other two needs are barely being addressed for fisheries scientists in Europe, in contrast to, for example, North America where the American Fisheries Society operates independently of the advice formulation process. For one thing it is clear from this research that scientists' working conditions, while still better than many professions, can stand some improvement. Less travel and more opportunities for creative research are needed, particularly among those who are involved in assessments. Scientists need an organized voice to express their needs as employees. Such a voice would need to tackle Europe-wide issues but it would likely not be appropriate for ICES to try to play a role like this while organizing the advice system. Second, scientists who are involved in analysing fisheries also need to have a way to advocate their beliefs and the needs they see for reform. Beyond just conservation and other fisheries management issues, there is a need to advocate for strengthening the profession. These are things that to some extent are being addressed through the ICES system. But they are also include things that should be separate from the advice provision system. Hence, we would recommend that fisheries scientists think about creating a Europe-wide independent professional organization. This would not only help them meet some of their individual goals, it could be a powerful advocate for more effective marine environmental management.

The fisheries scientists we have studied here are an extremely committed and creative resource for achieving important ecological, social and economic goals. We hope that this report can contribute to finding more ways to release this energy into an accelerated reform of the fisheries management system.

5. References Cited

American Heritage Dictionary of the English Language, The. Fourth Edition 2000. Boston: The Houghton Mifflin Company

Barnes, B., D. Bloor, and J. Henry 1996 Scientific Knowledge: A Sociological Analysis Chicago: University of Chicago Press

CEC (Commission of European Communities) 2000 "Council Decision of 29 June 2000 on a financial contribution from the Community towards the expenditure incurred by Member States in collecting data, and for financing studies and pilot projects for carrying out the common fisheries policy" Official Journal of the European Communities (2000/439/EC)

CEC (Commission of European Communities) 2001 "Commission Regulation (EC) No 1639/2001 of 25 July 2001 establishing the minimum and extended Community programmes for the collection of data in the fisheries sector and laying down detailed rules for the application of Council Regulation" Official Journal of the European Communities No (2000/1543/EC)

CEC (Commission of European Communities) 2003a "Communication From the Commission: Improving scientific and technical advice for Community fisheries management" Official Journal of the European Union (2003/C 47/06)

CEC (Commission of European Communities) 2003b "Commission Staff Working Paper Ad Hoc Working Group on Mixed Fisheries" Brussels 21-24 October 2003

Degnbol, Poul 2003 "Science and the user perspective: The gap co-management must address" pp 31-50 in Wilson, D.C., J.R. Nielsen and P. Degnbol (Eds) The Fisheries Co-management Experience: Accomplishments, Challenges and Prospects Dordrecht, The Netherlands: Kluwer Academic Publishers

Garcia, S.M., Zerbi, A., Aliaume, C., Do Chi, T., Lasserre, G. 2003. "The ecosystem approach to fisheries. Issues, terminology, principles, institutional foundations, implementation and outlook" FAO Fisheries Technical Paper. No. 443. Rome, FAO. 71 p.

Gieryn, T. F. 1983 "Boundary-Work and the Demarcation of Science from Non-science: Strains and Interests in Professional Ideologies of Scientists" *American Sociological Review* 48:781-795

Haas, P.M. 1989 "Do regimes matter? Epistemic communities and Mediterranean pollution control" *International Organization* 43:379-403

Habermas, J. 1987 The Theory of Communicative Action: Volume II Lifeworld and System: A Critique of Functionalist Reasoning Boston: Beacon Press

Habermas, J. and T.H. Nielsen 1990 "Jurgen Habermas: Morality, Society, and Ethics: an Interview with Torben Hviid Nielsen" *Acta Sociolgia* 33:93-114

Holm, P. and K. N. Nielsen 2004 "The TAC Machine" Appendix B, Working Document 1 in ICES 2004 Report of the Working Group for Fisheries Systems (WGFS). Annual Report. Copenhagen, Denmark.

ICES, 2002, Working Group for Fishery Systems (WGFS). Annual Report. Copenhagen, Denmark.

ICES 2002. The ICES Strategic Plan. Copenhagen, Denmark.

ICES 2003a, ACFM Annual Report. Copenhagen, Denmark.

ICES 2003b Report of the Working Group on the Assessment of Demersal Stocks in the North ICES CM 2003/ACFM:02. Copenhagen, Denmark

ICES 2003c. Study Group on Precautionary Reference Points For Advice on Fishery Management .ICES Headquarters 24–26 February Report. Copenhagen, Denmark

Jasanoff, Sheila, 2002. "Citizens at Risk: Cultures of Modernity in the US and EU" *Science as Culture* 2(3): 363-380.

Merton, R.K. 1968a "Science and the Social Order" Chapter 17, Pages 591-615 in Social Theory and Social Structure New York: The Free Press

Nunnally, J.C. 1971 Psychometric Theory New York: McGraw-Hill

Pinkerton, E. 1989. "Introduction: Attaining Better Fisheries Management through Co-Management - Prospects, Problems and Propositions" Pp. 3-36 in *Co-Operative Management of Local Fisheries.* Edited by E. Pinkerton. Vancouver: University of British Columbia Press

Royal Society of Edinburgh (RSE) 2004 Inquiry into The Future of the Scottish Fishing Industry Edinburgh: RSE Publication

Scott, F. Richard.1995. Institutions and Organizations. Sage Publications, Thousand Oaks,CA

Shapin, Steven 1994 Social History Of Truth; Civility And Science In Seventeenth-Century England University of Chicago Press

Smith, M.E. 1990 "Chaos in Fisheries Management" *Marine Anthropological Studies* 3(2):1-13

Smith, M.E. 1995 "Chaos, Consensus and Common Sense" *The Ecologist* (25):80-85

Smith, R. and B. Wynne 1989 "Introduction" pp 1 - 22 in Smith, R. and B. Wynne (Eds) Expert Evidence: Interpreting Science in the Law London: Routledge

STECF, 2003, Scientific, Technical, and Economic Committee for Fisheries, Plenary Report #17. European Commission, Brussels, Belgium.

UN. 1992. Report of the United Nations Conference on Environment and Development (Rio de Janeiro, 3-14 June 1992) Annex I Rio Declaration on Environment and Development United Nations A/CONF.151/26 (Vol. I)

UN. 1995. Agreement for the Implementation of the Provisions of The United Nations Convention on the Law of the Sea of 10 December 1982 Relating to the Conservation and Management of Straddling Fish Stocks and Highly Migratory Fish Stocks United Nations: A/CONF .164/37

Wilson, D. C. and P. Degnbol. 2002. "The Effects of Legal Mandates on Fisheries Science - Deliberations: The Case of Atlantic Bluefish in the United States" *Fisheries Research* 58:1-14.

Wilson, D. C. 2003. "Conflict and Scale: a Defence of Community Approaches in Fisheries Management" 193-209 in Wilson, D.C., J.R. Nielsen and P. Degnbol (Eds) The Fisheries Co-management Experience: Accomplishments, Challenges and Prospects Dordrecht, The Netherlands: Kluwer Academic Publishers

Wilson D. C., Bonnie J. McCay, Veronica Rowan and Barbara Grandin. 2002 "Institutional Differences among Marine Fisheries Scientists' Views of their Working Conditions, Discipline, and Fisheries Management" *Fisheries* 27(8):14-24.

Wilson, Douglas Clyde and Alyne Elizabeth Delaney. 2005. Scientific Knowledge and Participation in the Governance of Fisheries in the North Sea. in Gray, Timothy (Ed) Participation in Fisheries Governance Dordrecht, the Netherlands: Kluwer Academic Publishers