TOWARDS AN EXPLANATION OF THE NORTH SEA COD CRISIS

- The Perspective of Knowledge

By

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Chapter 1: Introduction

Why it is important to investigate the current crisis of the cod fishery in the North Sea is self-evident. The crisis represents a threat to the livelihood of thousands of people dependent on fishery in coastal areas adjacent to the North Sea. The crisis of the cod stock is not only affecting the cod fishery but also other demersal fisheries, which currently have to be regulated with regard to the cod stock. Further, it is obviously not only the fishermen, who are affected but also the processing facilities and the marketing and distribution channels. Consequently, such a crisis, on a longer time scale, represents a threat to fragile coastal communities, where occupations unrelated to fishing are few.

It is only through identifying and describing what led to the crisis that one can hope to provide improvements - both with respect to the current state - and as regards possible arrangements for avoiding reoccurrence of such a crisis.

A credo

This thesis could be taken to be critical - and it is so. But I stress that it is not intended to be critical to any individuals. First, I think there is no reason to criticise individuals. I basically think everyone is trying their best - subjected to the constraints they each perceive. The scientist tries to give the best possible advice given his constraints, the manager tries to decide on the fishery in the way he finds to be best subjected to the constraints he perceives, and the fisherman tries to abide by the regulations until he finds it not possible to do so.

Secondly, there would not be much *point* in criticising individuals. What is much more interesting, is to analyse, and hopefully improve, the *fishery system* or the relations between the institutions involved in advising, managing and using the resource. If the constraints the different stakeholders perceive are barriers to improvements, we must understand the nature of these in order to come up with something better.

In this work different perspectives are explored and give different although, I think, complementary answers. Therefore I ask you to read the whole thesis or not to read it at all. Otherwise the impression you will get is very likely to be different from what I want to say.

Why the knowledge perspective?

Knowledge is a necessary condition for the development of a perception of the crisis but at the same time the lack of knowledge can be an important factor in explaining it. Further, management can be thought of as the use of knowledge in a fishery system. The issues of trust and legitimacy (and compliance to the policy) are also related to the knowledge perspective. Knowledge is, therefore, a central issue with respect to the performance of a fishery system, and therefore also in explaining a crisis in such a system. I will state a further limitation of my work in that I will focus only on the perspective of *scientific knowledge*, which is most central to the current management.

Explanations and explaining

A simple account of what an explanation is that it is an answer to a (explanation seeking) why-question (Hempel, 1965: 334). It is essential to consider what is to be explained (explanandum) and what is doing the explaining (explanans). My intention is to try to explain why we have the North Sea cod crisis. Therefore I must first establish the explanandum; that we *actually* have a cod crisis - and what it means.

How is the explaining done? With the classic covering law model of explanations (Hempel, 1965: 331- 425), something is explained when it is shown that given some specified initial conditions and some causal laws (nomological or statistical laws), the explanandum was either necessary or highly likely. These models have later been criticised by strong counterexamples. However, I find that the basic idea is sound. Something is explained to the degree that you understand that it actually was a likely outcome, given the context. For a phenomenon as complex as the one in question an ideal or "rigid" explanation can probably never be provided. In such cases, the term explanation sketch is often used (Hempel, 1965: 423-425). As a consequence of the complexity of the issue and the limits of empirical evidence, the explanation will be short of the ideal explanation of the covering law model, and the explanans is then limited to *suggest* the explanandum.

Another property of the explanation is its level of resolution. Say, that it is established that the crisis was due to over fishing. A simple explanation is then: “The crisis was a result of too much fishing”. The explanation would be true but not particularly informative. On the other hand, an explanation of a complex phenomenon can loose itself in details. The proper task is to make the explanation both relevant and informative. I think a useful aim of explanation of such a complex phenomenon would be a pragmatic aim. Such an aim of the explanation could be that an “informed person” would say: “Yes, now I understand – it is highly likely to have happened such as you describe it”.

Method and theory

I have used no formal method or theory. Rather my approach has, broadly, been to pose *ad hoc* research questions, which either are “closed” or followed up by further *ad hoc* questions. I have used all sorts of literature that seemed promising towards shedding light on the general question. The approach thus comes close to that of an anarchistic “anything goes” approach. Given the limitations, I cannot perform "new" science. What I do is mainly to make a sort of selective patchwork of previous studies, resulting in a sort of “informed interpretation”. I compare and analyse previously published information from different sources.

The proper evaluation of such a somewhat indefinite method is by its result; If you find the following informative, I take method to be *in situ* justified – and *vice versa*. Specifically, I have used quite simple data analysis in chapter 2, the methods of which I will describe briefly as I proceed.

The bulk of the thesis consists of three different histories of the scientific advice: A quantitative history (chapter 2), a qualitatively history (chapter 3) and a discursive history - i.e. an analysis of the relation between scientists and managers (chapters 4.1, 4.2 and 4.3). I generally start out with the perspective of biology and I generally end up with the perspective of social science. The main reason for this structure is the logic of the *ad hoc* questions I set, which at the same time can be said to reflect my own cognitive journey in trying to understand and explain the crisis. It is a complex story to tell and I found that it was perhaps safest to tell it in more or less the same way as I experienced it.

Establishing the crisis as a fishery system crisis

Why do I term the current situation a “crisis”? I find the word crisis suitable since I think it adequately covers *what it is* and *how we got there*. I think of a "crisis" as in the context of illness. A body is in crisis when you, for example, do not know whether it will die from a disease or not. A “collapse” is beyond the crisis - it is when a body has already succumbed. Further, a collapse indicates that the transition from the healthy to the unhealthy state was sudden or instant, which was not the case for the cod stock in the North Sea. We could, then, call it a “decline” - it certainly is - but that is a neutral description, whereas “crisis” has the advantage of indicating the very undesirable consequences of the decline. Further "decline" refers to the process of stock change, whereas "crisis" here refers to a certain state after that decline, where the state has the properties as mentioned above. I will argue that the cod stock is in a state balancing between the path to recovery and the path to, possibly, an irretrievable depletion, and that it therefore is justified to refer to a “cod crisis”.

In the metaphor of the body, the disease that is causing the crisis can spread to more organs and lead to different types of symptoms as it develops. Similarly, the crisis can be evident in other parts of *the fishery system* than in the part that the resource state represents. I will present a simplified view of the fishery system and describe symptoms of crisis for each of its parts. But be aware that it at this stage is too early to say what is only a symptom and what is causing disease.

The fishery system

A modern, industrial fishery system, such as the one in question, can be thought to be a complex of four interacting subsystems: The resource system, the user system, the knowledge system and the management system.

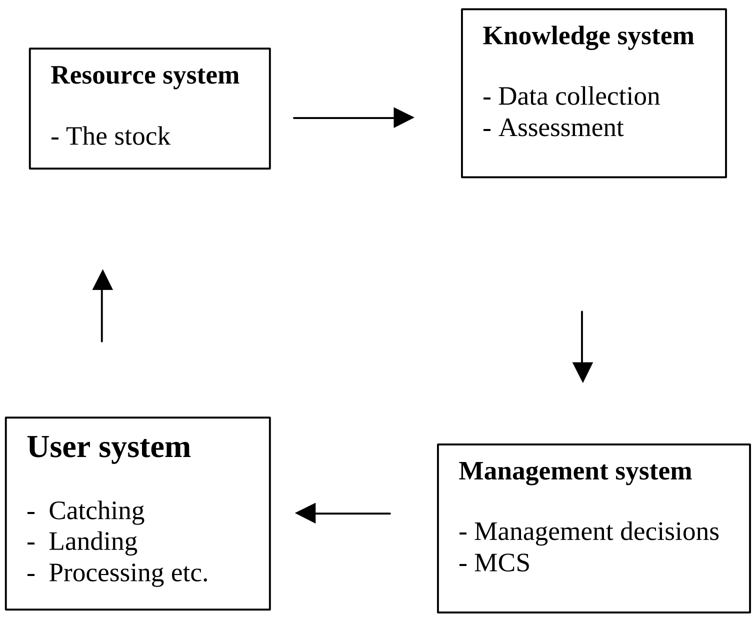

Figure 1. Simplified representation of a fishery system (modified from CM 2000/D:02: 15).

The arrows indicate, somewhat vaguely, main processes flows. From the resource system to the knowledge system, the flow consists of collected information. The knowledge system will process this information and deliver a "perceived state" to the management system. The management system will respond to the "perceived state", but its output is not only information (i.e. decisions), but also physical (e.g. Monitoring Control and Surveillance). The arrows, however, only indicate the *main* processes and influences. The subsystems are in reality intrinsically and complexly linked, which soon will become apparent. For example the user system will influence the management system through lobbyism.

“Truth”, epistemology and ontology

The heart of the problem is of course the stock level of the cod. If there were enough cod there would be no crisis. This could be thought of as the ontological side of the problem, whereas the knowledge system deals with the epistemological side (understood such that interactions between the two are allowed).

I will present the latest "perceived state" of the stock, which is provided by the Advisory Committee of Fishery Management (ACFM), the scientific body responsible for scientific advice on the stock. The question is then: Can we trust this "picture"? Of course we cannot. This is exactly one of the central problems in fishery management; we will never access the ontology - it will remain a “ding an sich”. However, the latest picture is the picture that we have best *reasons* to believe since we had more information for its production, than we had for the previous pictures. I will return to this issue later when reviewing elements of the methods used for stock assessment of this stock (chapter 4.2). In the following, the latest assessment is, at least indirectly, referred to as “true”. I stress that "truth" here should be considered in the limited sense that the latest assessment is the most reliable for the time being. Truth will then only be a sort of abbreviation for the current, relatively strongest, confidence in the assessment due to a process of *justification*. Unlike Finlayson (1994), we must however assume truth, since there otherwise will be no point in our analysis. For if there is no truth it is not true that there was a cod crisis to explain and so on.

Crisis in the resource state

The following graphs represent what is the "currently best justified" stock history of the cod, namely from the latest assessment of ACFM, which includes the detailed data that exist from 1963 and onwards.

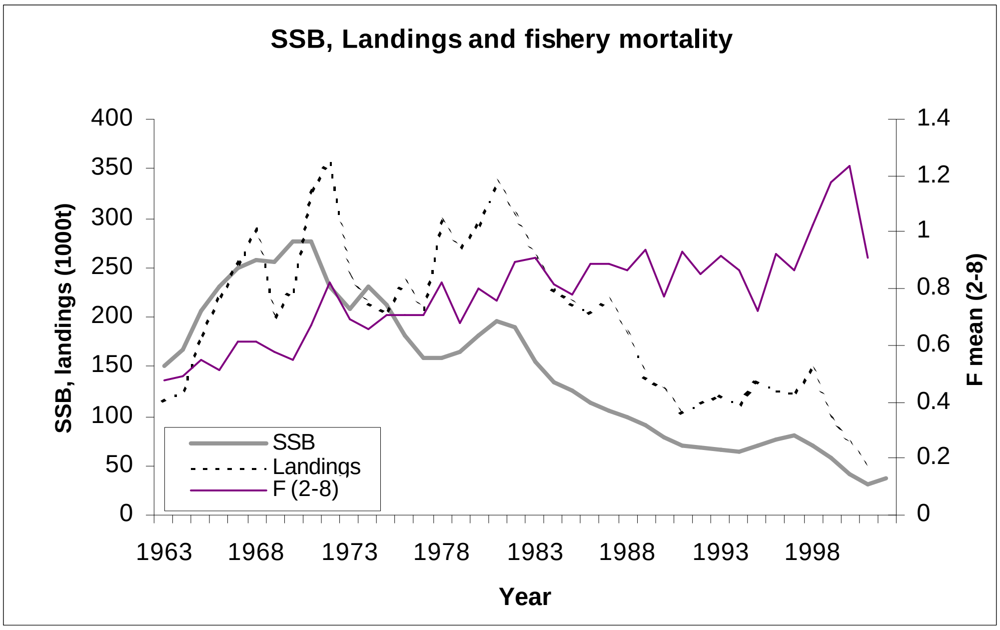

Figure 2. Graph of Spawning Stock Biomass (SSB), Fishing mortality F and landings from cod in ICES subarea IV (North Sea), Division VIId (Eastern English Channel), and Division IIIa (Skagerrak). The data is from ACFM 2002: 45. For ICES areas: See appendix 1.

From figure 2 it is evident that the spawning stock biomass has declined quite steadily from a (historical) maximum level of 271.000 tonnes in 1971 to a (historical) minimum in the last years (30.000t in 2001 and 38.000t in 2002). The maximum was reached following a recovery from around 150.000t in 1963. There was a moderate recovery in the late 1970s to the early 1980s and a very modest recovery in the late 1990s.

The fishery mortality - the rate of cod death from fishery - was building up considerably from the 1960s until the late 1970s. Then it levelled out until the late 1990s where there was a dramatic increase followed by, apparently, a sharp decline in F in 2001. As will be explained later, the assessment of the latest year is nevertheless always the least certain. The trend in landings has roughly followed the trend in SSB. Note that the SSB recovered somewhat in the late 1970s in spite of some of the highest landings in the record. This was because of a very high recruitment in this period.

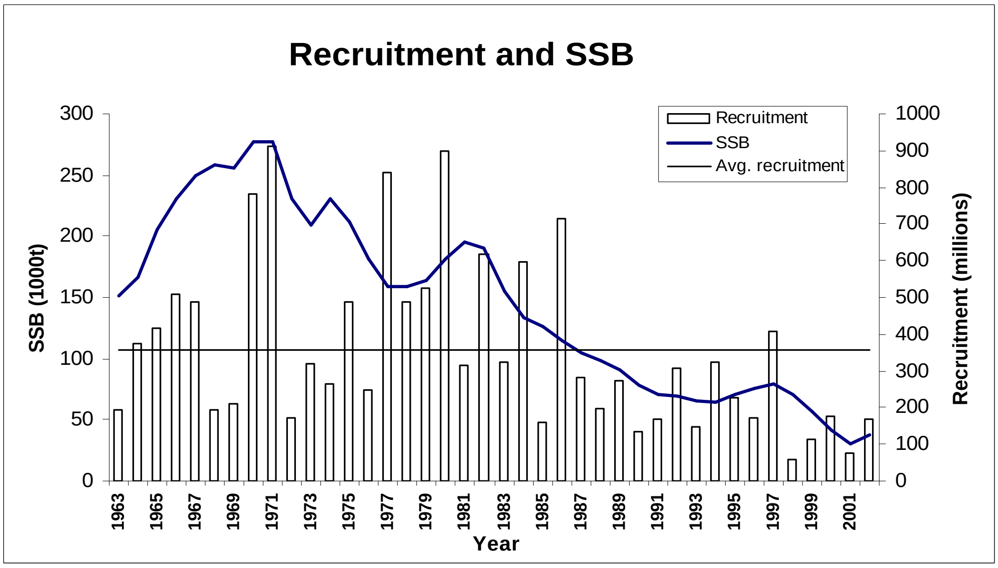

Figure 3. Average recruitment to age one and spawning stock biomass for the period 1963 to 2002 in ICES subarea IV, VIId and IIIa. The average recruitment level is indicated by the straight horizontal line (= 357 millions). The data is from ACFM, 2002: 45.

From figure 3 it is evident that the recruitment is highly variable. The recruitment reached very high levels from the mid 1960s to the mid 1980s ("the gadiod outburst"). Since 1986, 1997 has been the only year with the recruitment being above the average of the depicted period.

Let us consider what perceived state of the resource ACFM is transmitting to the managers. In the latest report by ACFM the stock was said to be “outside safe biological limits”. The spawning stock had been below the precautionary reference point (B_{pa} = 150,000 tonnes) since 1984 and within the region of the historical limit reference point (B_{lim} = 70.000t) since 1990. The fishing mortality was said to have remained about the historic high, and above it its precautionary reference point (F_{pa} = 0.65) since 1980, and also to exceed F_{lim} = 0.86, which is “the fishing mortality estimated to lead to impaired recruitment”. Further, the recruitment was said to have been below average for all years since 1987, where the 1997 and 2000 year classes have been the poorest on record (ACFM, 2002: 37). In the 2001 report, ACFM said that the risk of stock collapse was high. SSB was at a historic low of 55.000t (CRR 246, 2001: 230-232). The next year, the 2001 SSB estimate was revised down to about half - only 30.300t and the 2002 estimate was 37.600t. In the 2001 report it was noted that the reduction in recruitment in the later years could be explained by the low SSB although the effect of observed hydrographical changes could not be ruled out.

In its latest assessment ACFM summarized its advice on management as follows:

Given the very low stock size, the recent poor recruitments, and continued high fishing mortality despite management efforts to promote stock recovery, ICES recommends a closure of all fisheries for cod as targeted species or bycatch (ACFM, 2002: 37).

I find that the notion of crisis to be the most adequate for this situation.

Crisis in the knowledge system

Since the assessment is mainly based on the landing data, the knowledge system is sensitive to a crisis in the user system, since the latter is the source of these data. ACFM stated that there was “reason to believe, that the landings for 2001 were under-reported”. The landings were less than the TAC, which “implied a reduction in fishing mortality of the order of 50%”. However, the fishing mortality in 2001 actually increased considerably. Further: “The results of a time-series analysis indicated predicted removals in 2001 almost double the reported landings"(ACFM, 2002: 39-40). In the report it was stated, that the fishing mortality had "consistently been underestimated and stock size overestimated in previous assessments, and the current assessment suffers from the same problem”. Assessments since 2000 were believed to have improved due to exclusion of CPUE data, which were inducing biases. Nevertheless, the latest assessment also showed retrospective bias, “possibly because of a decrease in the quality in landings data in 2001.” Finally: “The current SSB is so far below historic stocks sizes that both the biological dynamics of the stock and the behaviour of the fleets are unknown, and therefore historic experience and data are not considered a reliable basis for medium term forecasts of stock dynamics under various rebuilding scenarios” (ACFM, 2002: 39-40).

I find that the notion of a knowledge crisis adequately sums up the significance of these statements. Further, the commission of the economic communities recently (CEC, 2002, EU-PR, 2002) presented a strategy to improve the quality and timeliness of the scientific advice. The knowledge problems are thus also evident within the management system.

Management system crisis

What indications would there be that the management system is in crisis? To answer that the management system is in crisis because it apparently does not function is not adequate. The inadequacy of the system management is a potential cause of the fishery system crisis. If the inadequacy of the management system at an earlier stage had led to development of perception of its crisis, a *fishery system crisis* might have been avoided.

The criteria for state of crisis are different for something physical and something "mental". Whereas a body or a cod population can be in crisis without our knowledge thereof, the management system can only be in a state of crisis when we think it is in that state, since the system is a construct of mind and social actions. Further, there is a difference between claiming something dysfunctional and claiming it to be in crisis. The crisis of the mental and social domain is a result of changing perceptions; e.g. “we don’t think this system is good anymore, *so we have to revise it*”.

Now I hope it is clearer what I want to argue; that the perception that the management system really should change is developing, which is equivalent with the notion of crisis. From the following quote it is apparent that the Commission of the Economic Communities recognises a crisis:

> **The CFP has reached a turning-point. The challenges are urgent and serious. The current poor sustainability performance of the CFP proves that many of the instruments applied over the last twenty years have reached their limits. In this state of crisis there is a need for major change. Reform of the objectives, principles, priorities and instruments of the CFP is more than ever necessary to deliver sustainable development and to ensure that the European fishing industry has a secure future** (CEC-COM 181, 2002).

The Council of Ministers has adopted some of the proposed revisions suggested by the CEC - however not always to the full extent (EU-CM, 2002). For example the Council this year, in the view of a crisis, introduced a fishing days limit for the roundfish fishery in the North Sea. I will later explain why this change is important, even though it was only introduced on a provisionally basis.

The fishery

Cod is mainly caught in a mixed, demersal roundfish fishery (some cod is also taken as bycatch in flatfish fisheries). The fishery is carried out with different trawl types, seines and gillnets (CRR 246, 2001:221). Gillnetting is the most selective fishery, and it is the fishery that is most directly targets cod.

The North Sea is a multinational fishing area, subjected to the interest of many nations. The cod fishery is accordingly managed as a joint EU-Norway stock, where EU is allocated 83% of the Total Allowable Catch (TAC) and Norway 17%. UK is the main fishing nation followed by Denmark and Norway. The others are in order of decreasing importance: The Netherlands, Germany, France and Belgium – and to a very limited extent: Poland and Sweden.

Fishermen's crisis

The latest adopted TAC was 27.300 (No. Dep. of Fisheries, 2002). This is about half of the TAC in 2002, one third of the TAC for 2000 and less than one fifth of the average TAC since the first TAC was given in 1975. Given the notorious problem of over capacity it goes without saying that the fishermen face serious economic problems. That is of course why fishermen have been very much opposed to the recent and drastic reductions in TACs. Note, however, that the cod is mostly taken in a mixed roundfish fishery - together with whiting and haddock. This amends the crisis impact somewhat to the degree that the fishermen can rely on the catches of the other species. The quotas for the other gadoids have nevertheless also been reduced in order to protect the cod - so the fishermen with mixed catches will still face serious problems. Obviously, however, the fisheries most directed towards cod are going to be most affected by the TAC reductions.

Let us consider the Danish case as an example. The “gillnet and hook” fleet will be the most sensitive fleet to the dramatic cut in cod quotas, since it is largely targeted for cod. In this fleet (about 450 vessels) the revenue is expected to decrease by between 30% and 40% in 2003, which implies that a large part of this fleet will not be able to cover the variable costs - they will loose money even when fishing. In addition, the economic prognosis was carried out without considering the effect of the restriction of fishing days, which in turn is expected to lead to a worse situation than indicated by the prognosis (FOI, 2003).

A complete fishery system crisis

If you are now accepting that each part of the fishery system is in the state of crisis, it follows that the complete fishery system is in crisis. This point is, however not so important. The intention was rather to show the interconnectedness of the sub-systems: The causes and symptoms spread through the fishery system as the crisis develops. The main points are that there is crisis in the resource state, which implies a crisis for the resource users. Further, the knowledge system is in crisis as a consequence of the pressure on the other sub-systems. This points to the need for revisions in the management system and there is some indication that these are initiated (i.e. a management system crisis). Before we proceed with these issues, it must be justified that the stock crisis actually is a fishery problem – otherwise it would make no sense to treat the crisis as a management problem. The latter is not as obvious as one could think. Again the question of knowledge and lack of knowledge becomes focal.

Explanans: possible causes of the stock decline

What can explain a complex phenomenon as a stock decline? When trying to allocate explanatory importance to different candidates for causes, the best approach would be to proceed from evidence that makes it possible to conclude that some issue either was important or that it was not important. However, this may not be feasible and thus a “second best” approach must be used: To conclude that there is *no strong evidence*, that the issue was important and then proceed to the next possible cause, which hopefully is more fruitful to investigate. Given the nature of the evidence in question, this latter approach has to be followed.

To the degree that the distinction can be made, it may be useful to distinguish between anthropogenic and non-anthropogenic causes. However, I have found no evidence for viewing the stock problem to any large extent as due to non-anthropogenic causes or evidence for not doing so - with the question of recruitment being one possible exception (we do not yet know if temperature effects are anthropogenic). This is a weak point - at least so far - but allow me by "the second best approach" to proceed with the explanatory potential of anthropogenic causes. The recruitment issue will be discussed separately in the following since it remains uncertain if the observed changes in recruitment patterns are due to anthropogenic causes or not.

Anthropogenic causes could similarly be dichotomously divided into those unrelated to fishing and those related to fishing. What non-fishing causes would be good candidates? I have only come across two obvious ones: Pollution and eutrophication.

Parret (1998), provided a major review of the literature, that “identifies the most current and relevant information relating to known and perceived effects of pollutant on fish” in the North Sea. This study concluded that there is circumstantial evidence of biological effects of chemical contaminants on the sub-organismal and individual level of biological organisation (examples are liver tumours and disturbances of immune functions) of fish and shellfish. However, “there is not currently any clear evidence in the literature that chemical contamination is impacting on populations or fish stocks”. Parret (1998) emphasises that the former should not be taken to mean that population effects are not taking place or that they will not take place in the future. To me, the conclusion, however, is a prima facie reason to proceed with the “second best approach”.

The eutrophication case is somewhat similar. There is some evidence of toxic algal blooms and oxygen depletion in coastal regions affecting fish abundance directly or indirectly. But:

Conclusive evidence that anthropogenically derived nutrient inputs are responsible for an alteration in fisheries abundance, however, is lacking as large scale environmental changes and fisheries practices are also implicated (Parret, 1998).

The North Sea is a vast water body with a considerable exchange of water. Negative effects of eutrophication have been limited to affect the shallower, estuarine and coastal areas in a relatively narrow zone. Eutrophication may however affect recruitment of cod, since growth of its juveniles to a large extend takes place in the near coast areas (Boddeke and Hagel, 1991). However, I will proceed with the potential for fishery as an explanatory factor before I return to the recruitment issue.

The potential of a fishery explanation: The significance of F’s

The literature concerning assessment of the North Sea cod stock generally leaves little doubt that the decline of the stock is somehow due to its intensive fishery. On the condition that the estimated Fs are just very roughly within the range of their true values for the cod stock in the North Sea this is not surprising. Let us briefly turn to basic population biology to see why.

The percentage of fish death in a time interval t_1 to t_2 $= 100 \cdot (1-e^{-Z(t1-t2)})$, where Z is the coefficient of total instantaneous mortality. Z = F +M, Where F is the fishing mortality and M is the rate of natural (non-fishing) mortality. Let us assume the natural mortality M is 0.20, a value that is often used - for example by the WG on the North Sea cod stock for ages of 4 years and older (CM 2000, ACFM:7). The mean F for ages 2-8 has been estimated to be about 1 in recent years. It can now easily be calculated what fishing at such an F value implies. It means that about 70% of each year class of 2 year old and older cod will die per year and that 90% of these deaths are because of fishing. If we consider the mean value of F in the period 1963 to 2001 (F= 0.78), it implies an annual death rate of 63%, of which 87% of the deaths are due to fishing. That is why the fishery is considered to be important in explaining the stock decline.

M=	F=	Z=(M+F)	Z (%)	F / Z
0.20	0.20	0.40	0.33	0.50
0.20	0.36	0.56	0.43	0.64
0.20	0.78	0.98	0.62	0.80
0.20	1.00	1.20	0.70	0.83
0.20	1.20	1.40	0.75	0.86

Table 1. Distribution of mortality. Calculated examples for illustration.

One complication is that F and M are interdependent. If you fish less, more fish will die from "natural reasons" thus the M will increase. But it will still be true, that the fishing will explain half of the mortality if F is equal to M. In other words: to say that fishery was not the most important in explaining the stock decline would imply to say that M was seriously underestimated or/and that F was seriously overestimated. If M was really the double, and F was really only half of the current estimates, it would still be true, that fishery was a main cause of the stock decline.

Fishery science is, notoriously, an uncertain science. Fish population models are often very flexible to very different interpretations, as will be described later. Could it be that the whole series of F and M are considerably and consistently wrong?

Could it be that we do not really have the potential of controllability, since the stock levels are very much less dependent on fishery than we are used to think?

The sceptic will always win the argument: We do not *know*. Yet, the sceptic will never allow himself to explain anything. And there is some quite convincing evidence for the contrary; e.g. evidence for potential of controllability.

The axiom of controllability

In "Changes in the North Sea cod stock during the twentieth century", Daan et. al. (1994), Pope and Macer, (1996) tried to extend the series stock history information backwards; beyond the period from which there is reliable data (1963 and on). The limitations of data, however, were such that the results must be interpreted more in a qualitative than a quantitative way. The writers found that the landing data were the most reliable source of information. Estimates of fishing mortalities based on CPUE data were the second most reliable information source since data from independent fleets essentially gave the same results. The general approach was then "an attempt to capture the main trends in stock sizes and recruitment to match the realised catches and estimated fishing mortalities".

A very significant result from this study is that the fishery mortality (not surprisingly) dropped to a very low during the Second World War. F was estimated to be around 0.5 in the decades before the war, 0.10 during 1940-1945 and to have increased to 0.40 after the 1945 (for then to increase from the 1960's and until today). Further, the average recruitment from 1935 to 1940 and 1940 to 1945 was estimated to have been the lowest for the whole series from 1910 to 1994. Now, in spite of this very low recruitment, the SSB and TSB (total stock biomass) showed the strongest increase seen in the same series. The SSB increased from less than 100.000t to almost 300.000t during the war. And when the fishing picked up again, the SSB steadily declined until the onset of the "gadoid outburst" in the mid 1960s.

I admit some circularity of argument here: Fishery mortalities are used to evaluate themselves. However, with the proper academic reservations; this is as good evidence for the severity of impact of the fishery as we can get. Personally I find no strong reasons to *suspect it to be wrong all together*. It is reasonably to view fishery as a main cause of the low stock levels of recent time. Let us now address the issue of recruitment, which the former discussion really cannot be seen in isolation from. Will it complement or contradict this interpretation?

Limits to controllability: Recruitment and the “gadoid outburst”

In a review of the available literature with respect to changes in the North Sea cod in the 20th century, Niels Daan (1978) noted that the most significant of these changes appeared to be the recruitment (the gadoid outburst), which resulted in the dramatic increase in landings of the 1960s. Especially when seen in a longer time perspective, the gadoid outburst was very marked (Daan *et al.*, 1994).

Various hypotheses have been proposed for these changes in recruitment but none of them has been conclusive (Hislop, 1996). Cushing (1984) argued that the elevated recruitment was because of a change in abundance and timing of zooplancton species, which are prey to pelagic stages of gadoid larvae. This conclusion was however later doubted (Daan *et al*., 1994). Pope and Mazer (1996) examined if changes in predator stock levels could explain recruitment variation. But since the gadoid stocks themselves are important predators, the study concluded that inclusion of predator effects only revealed that the recruitment must have been even more favourable. The elevated predating stock levels following the high recruitment would tend to decrease the recruitment and thus not be a source of explanation of the outburst - on the contrary, there is even more to explain.

Daan *et al*. (1994) stated that the overexploitation of the pelagic system (herring, mackerel) could "not be discounted" as a major causal factor for explanation of the gadoid outburst (predation release on juveniles or larvae, competition release on prey). However, this hypothesis is weakened by the fact that the timing and areas of the decline of these pelagic stocks does not mach the increases in recruitment in the gadoid stocks.

O'Brien *et al*. (2000) argued that decline in cod recruitment had "paralleled" warming of the North Sea for the last decade. Weak year classes, however, also occurred in cold years, when the stock biomass was low. The (possible) temperature effect is therefore minor when SSB is low. The study was not conclusive but describes a correlation. It was argued that the combination of diminished stock combined with possible adverse warm conditions would be a threat to the long-term persistence of the North Sea cod.

Boddecke and Hagel (1991) claimed the eutrophication of the North Sea continental zone to be a "Blessing in disguise". The increased productivity in near coast and shallow areas was taken to explain the increase in recruitment of fish, for which these areas serve as nursery grounds (cod and whiting). The period of elevated nutrient discharge matches the onset of the gadoid outburst in the early 1960s and with its fade-out about two decades later, when nutrient discharges were reduced. Further they claimed that the landings of fish species not dependent on estuarine nursery grounds did not increase. The latter is probably wrong, however, since haddock apparently was one of the species who benefited most from the gadoid outburst, although the variation in haddock recruitment was much higher than it was for the other gadoids (see fig. 5 in Pope and Macer, 1996: 1162).

To me this does not exclude eutrophication as a potential cause. Nielsen and Richardson (1996) related the increased yield of Kattegats fisheries (an area adjacent to the North Sea management area) to eutrophication. The evidence was however not sufficient for a causal relation. For the North Sea, the eutrophication could also have played a role for haddock, since it lives off various "leftovers" at the bottom. However, further work would be required to establish a possible link between eutrofication and recruitment.

Redirecting the explanation

I therefore agree with Hislop (1996): We don't know why the "gadoid outburst happened. This confusion might call for “a third best approach”: To give up or start all over again. I will try another strategy. Do we really need to understand the reason of recruitment changes in order to explain the cod crisis of today? In one way we do: The crisis could be seen as a consequence of a return to normal recruitment. In another way we do not. The gadoid outburst allowed the fishery mortality to continue to increase, without the SSB going down. Therefore, the gadoid outburst was really *postponing* the crisis. Or it may also explain how the fishery was able to expand that much, and thus explain both the postponing of crisis and the severity of the crisis (i.e. it allowed the effort to increase considerable). If I move the focus from the time perspective from the last 30 years to that of the last century, it allows me, so to speak, to move the gadoid outburst from the explanandum to the explanans.

Is this a rhetoric trick? Of course it is. But, importantly, it is more than that: It will make the explanation *interesting*. For the interesting explanation must be both *possible* and *useful*, whereas the explanation with to many loose speculations will be neither. If we simply do not have the adequate knowledge on recruitment, the relevant explanation must take that into account. In addition, the usefulness of the explanation is also, partly, related to what we can change. And if the recruitment – at least at the current stage is not in our hands - this will accordingly point towards the appropriateness of latter form of explanation. I hope this all will become clearer as we go on.

Why then didn't I start out with this long-term perspective? Any way you view there is a crisis, but the background on which the crisis becomes apparent changes considerable when the history is extended backwards. Interestingly, the long-term perspective is weakly, if at all, transmitted to the managers. Therefore the crisis is generally *perceived* within in the four decades perspective, which makes the exploration of this perspective unavoidable. What I suggest, is, that a useful *explanation* of the state of crisis will benefit from including the perspective of a longer time horizon.

Returning to the role of the gadoid outburst

Daan *et al*., 1994 and Hislop (1996), argue that the increased fishery mortality was the reason why the SSB did not increase very much in the 1970s. According to the former authors, the impact the elevated recruitment had on TSB during the gadiod outburst was much less than the impact the cessation of fishery had during World War II. "Nevertheless, the net effect of the recruitment must certainly have been that the biomass remained relatively high even at extremely high levels of fishing mortality." (Daan *et al*. 1994).

In recent years the recruitment has returned to normal levels like those prior to the early 1960s (Daan *et al*., 1994). The fishing mortality has increased steadily and the spawning stock biomass has reached an all-time historic low (Daan *et al*., Hislop) and. The recent spawning stock level cannot be considered "normal" (Hislop, 1996). Consider that the SSB now has declined to about half the level of what it was considered to be at the time of the production of papers referred to here.

It could be that the *decline* in recruitment in recent time is caused by the historical lows in the SSB (Pope and Macer, 1996), (Daan *et al.* 1994). This suspicion was explicated in the 2001 ACFM report, even though the causal effect of observed hydrographical changes could not be ruled out (CRR 246, 2001: 222). This unresolved question will determine the degree to which management can restore the stock to the high levels of the gadoid outburst (Hislop, 1996), (Daan *et al.* 1994). However, if recruitment over fishing currently is taking place, that will only strengthen my point: the crisis is most reasonable seen as a fishery problem.

A fishery problem

Let us summarise the findings of the possible causes of the stock crisis. We don't know why the gadoid outburst happened, but it postponed the crisis in enabling the stock to sustain a high and increasing fishing mortality. We also don't know why the gadoid outburst faded out, but there is a suspicion that it was caused, or reinforced, by the heavy exploitation, e.g. recruitment over fishing. The recruitment is currently not lower than it generally was estimated to be in the last century. But the spawning stock biomass has recently been reduced to a historic low level, a level that is much lower than the lowest level seen during the poorest recruitment periods prior to and during World War II. There is little doubt that the decline in spawning stock biomass is caused by the intensive fishery expressed in the high F levels, so we have, at least, the potential to control the stock biomass. Therefore it is proper to view the crisis as a fishery problem and thus a problem of management.

We cannot control recruitment but we can improve the exploitation of the resource subjected to a given recruitment (similar to Hislop, 1996). Management and accordingly an explanation in terms of fishery management make sense. Let us therefore turn to the management.

Chapter 2: Quantitative analysis of the history of advice and management

Analysis of recommendations TACs and landings

It is often said, that the decline in the cod stock was due the managers setting the quotas higher than recommended by the scientific advisors. This is claimed to be the case for the cod in the Barents Sea (Nakken, 1998) and this is what people with experience in fisheries told me when I asked them why the crisis in the North Sea happened. Holden (1994: 62-65, 107-108) accordingly argued that TACs for the North Sea cod, for political reasons, were set higher than scientifically recommended such that the TACs achieved nothing with respect to fishing mortality[1].

Since the quotas are the core of the management system, it is necessary to analyse what quotas were recommended, what quotas were politically agreed upon by the managers and what the associated landings were. The information needed to answer these questions is present in the assessment reports that the scientific advisors in the ACFM produce for the managers each year. The first year, in which harvest quotas were introduced on the roundfish fishery in the North Sea, was in 1975 (CRR 56, 1975). 1975 will therefore be the first year in the series to be analysed. The ACFM reports contain a recommendation for TAC for the following year and a first estimate of the landings of the year before.

As will become clear it is not entirely straightforward what values should be used for the recommendations, TACs and landings. This is because the recommendation consists of more than a single value and because the assessment is often revised, whereas the landings are updated later. One must therefore consider what the analysis should aim at exploring when considering which values to include in the analysis.

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[1] The view of Holden (1994) is complex and maybe somewhat ambiguous when it comes to the issue of the decline in the cod stock. He claims that the TACs failed because they were higher than the recommended values. But he also argues that the enforcement was highly inadequate (p159-167) and he stresses the importance of the failure to implement technical regulations (p91-99). Further, he explains that the decline in the cod stock was inevitable because of the fade-out of the gadoid outburst. Management would therefore only (potentially) be able to *delay* the consequences of the declining recruitment (151-156). As will be apparent I agree with his points except the first one (and partly the last), but his work could have benefited from some integrated evaluation and interpretation of these elements.

Recommendations

The managers receive the assessment and the recommendation by ACFM in an annually produced ACFM report (a Cooperative Research Report). ACFM bases its recommendation on a working group report, but often recalculates and revises the assessment. When analysing the recommendations, the working group reports can be ignored, since it is the ACFM report that is relevant to the managers. Until and including 1988 there was a recommendation for the North Sea cod following ACFMs first meeting in May, which was usually revised at ACFMs second meeting in October. This may sound a little pointless, but really indicates a trade-off involved. The managers need the advice as soon as possible to be properly prepared for the decision-making. The scientist would, on the other hand, need the information from the latest surveys on recruitment in order to provide the best possible basis for the assessment (CRR 106, 1981: 6-7). Since I want to investigate the actual effective advice and decision of the management system, I always chose the latest advice given to the managers - relevant for the final decision on the TAC.

Since the advice does not only consist of one recommended TAC figure, I need to make a decision on a useful interpretation on *what* is recommended in order to proceed. As it will be described more carefully later, the advisors generally recommended a decrease of the exploitation level of the roundfish stocks. The managers, however, were under pressure from the industry, which generally wanted the TACs to be high. What turned out to be most important to the managers was then what allowable catch *maximally* could be recommended. In this relatively crude, quantitative, analysis I will therefore only consider the maximum recommendable TACs, whereas I in later sections will turn towards other features of the given advices.

In a few cases it was somewhat delicate to judge what was maximally recommended. In the early 1980s the general basis of maximum recommendations from ACFM was that the TACs should aim to reduce F by 10% per year. ACFM would like the reduction of F (towards F_{max}) to be faster, but used the mentioned policy as a pragmatic way of recommending reductions, since reductions were problematic for the industry. The advice corresponding to this policy would be 220.000t for 1982 (CRR 114, 1981: 246) and 230.000t for 1985 (CRR 131, 1984: 62, calculated from a regression of catch options). However, because ACFM often reviews its previous advices it is possible to see what ACFM itself interprets its maximum recommended TACs to be. Surprisingly, the 1986 report says that these are 235.000t for 1982 and 259.000t for 1985, which corresponds to catches of status quo F. I have used the ACFM interpretation in these two cases (the numerical values do not matter too much in this case). Should we always accept the latest interpretation of ACFM in this respect?

1987 was a problematic year for ACFM. The second recommendation (given in November 1986) was a maximum TAC of 125.000t. This represented a dramatic cut compared to the previous years and it was associated with serious warnings of low SSB levels. In May 1987, new and much more favourable estimates of the recruitment revised the recommended maximum TAC to 200.000t. There was, however, not agreement within ACFM to advise the managers to increase the catches since it was an “excellent opportunity” to rebuild the stock (CM A:5, 1987). The final given advice was 125-200.000t. This advice was repeated in the CRR record until 1991, where the advice was referred to have been “<125” and finally from 1999, the recommendation was said to be 100-125.000t. It cannot be disputed, however, that 125-200 implies a maximum of 200. The catch forecast used for 1987 was that of the May meeting in 1987 since this turned out to be the basis of the management decision (the TAC was revised up from 125.000t to 175.000t).

Holden (1994: 107-108) and Karagiannakos (1996: 124-126, 133) have carried out similar comparisons of recommendations and TACs. However, they did not always choose the latest and thus effective recommendation. No TAC was recommended in the period from 1991 to 1995. Instead decreases in effort or fishery mortality were recommended. Holden (1994) and Karagiannakos (1996) used the latter recommendations to recalculate a corresponding recommended TAC. I find, however, that there would be little point in analysing some recommendation that was not given. Instead I have excluded these years here and leave them to be analysed separately later. It is exactly the fact that no TACs were given that makes these years very interesting.

TACs

It is much more straightforward to decide on the relevant TAC to include in the analysis. Sometimes the TACs that were agreed on by the managers, based on the recommendation of the May ACFM meeting, were changed because of revisions in the assessment by the November ACFM meeting. However the latest agreed TAC for a year is the one implemented, and therefore the one relevant to consider in the analysis.

Landings

The landing figures are often updated some years later than the first value (for example due to delayed reporting. I have used the latest available figure, since the best estimate of the landings is the most useful in this analysis. In the reports two sorts of landing figures are provided. The nominal landings are the landings as they are officially reported to ICES, whereas the working group landings (WG-landings or ACFM landings) are corrected for different reasons. In the case of the North Sea cod fishery, the difference between the WG-landings and the official figures are relatively small (figures Ia and Ib: appendix 2). This is because estimates of mis- and underreporting have generally not been incorporated into WG landings. An exception is the year 1998, for which the mis- and underreporting was believed to be significantly greater than for other (previous) years, even though the TAC was not taken. The unallocated landings for 1998 were, however, only about 5% of the total WG-landings.

The differences between the nominal- and WG-landings are mainly due to differences in calculation procedures (e.g. conversion factors for gutted to fresh weights etc) (ACFM: 7, 2000). The average ratio of nominal landings to WG-landings in the period 1975 to 2001 is 0.97 (table I, appendix 3) indicating that the nominal landings tend to be slightly smaller but that the difference is generally negligible. Since only the WG-landings are used for input in the assessment calculations, I will in the following base my analysis on them.

When analysing the history of quotas and landings and the assessment estimates one complication is that the assessment area changed in 1996. Until 1995 an assessment was carried out for the North Sea area separately, whereas the assessment from 1996 was carried out for an area that also included Skagerrak (IIIa) and the Eastern English Channel (VIId) (CRR 221, 1996: 80). The areas that were added to the North Sea area are of relatively minor importance. However, the effects is accounted for by using the latest available landing estimate for the North Sea only until 1995 and by using the latest available landing estimate for the combined area from 1996.

Recommendations and TACs

Managers generally followed the TAC advice from of the scientists (figure 4a). This impression is in accordance with the interpretation of Karagiannakos (1996: 133), but is in conflict with the impression you get from Holden (1994 107-108, 132-158), which is a little surprising since he used almost the same data.

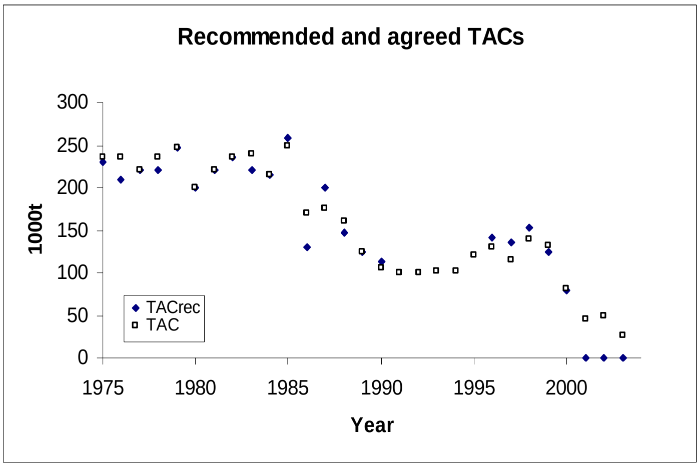

Figure 4a. Maximum recommendable TAC by ACFM and the agreed TAC. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). Data and sources: Table I, appendix 3.

I think that the conflicting interpretation of Holden largely stems from the fact that he did not always choose the latest recommendation. For 1984, 1986 and 1987 the adopted TAC was higher than the first recommendation for maximum TAC but less than the revised value. Further, the TAC adopted for the two last years in his series (1991 and 1992) were higher than the TACs that were taken to correspond to the advice of reducing effort by 30%. As mentioned, the choice of data depends on the intention of the analysis. Finally, the impression of the whole series changes when the years after 1996 are included (table I appendix 3).

Earlier in the series the managers sometimes wanted the TAC to exceed the recommendation a little (Figure 4b). Later it often was the other way around. The average of the ratio of recommended TACs to agreed TACs is almost 1 (table I, appendix 3)[2]. I use ratios here since they make more "biological sense" than absolute numbers. A difference of 20.000t means little to a large stock but may be crucial to a small one. Note, however, that the "recommendation" is in terms of the *maximum*

[2] Years from 1991 to 1996 (no TAC recommendation given) and the TAC = 0 in 2001 excluded (see later explanation).

TAC recommendable. In other words the managers generally "take what they can get", but usually not much more.

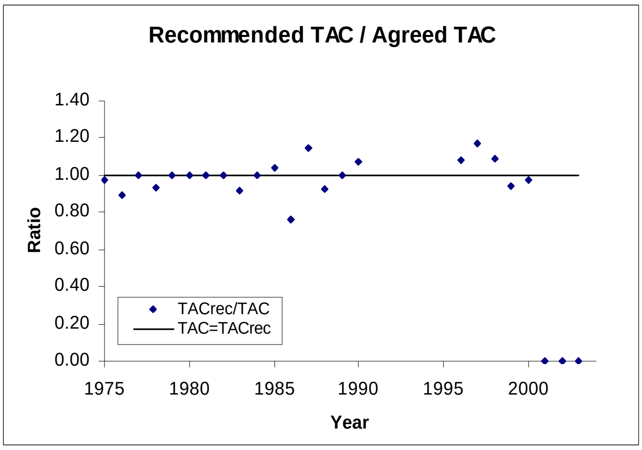

Figure 4b. Ratio of Maximum recommendable TAC to the agreed TAC. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrack (IIIa) and Eastern English Channel (VIId). Data and sources: Table I, appendix 3.

An important exception is of course the last three years. Since 2001 the scientist have recommended a TAC = 0, whereas the managers have agreed on relatively small quotas. However, the SSB was already extremely low in 2000 – or even before that. I therefore view this period more as the culmination of the crisis than as containing its explanation. The latter does not exclude these quotas from having a potentially devastating effect on the stock – not so much because of the magnitude of the quotas, but as it turns out, because fishing was allowed to continue.

I agree with Holden (1994) insofar that the general advice of reducing Fs was not followed effectively. But in terms of TAC recommendations, they generally were. The failure to reduce F was not because of the size of the TACs - compared to what was recommended - rather but in spite of them.[3]

[3] Holden (1994: 191-208) recognised the inefficiency of the TACs, but perhaps not the magnitude of the problem , which is understandable because the magnitude of the problem only escalated from the mid 1990's. However, Holden (1994: 151-159) explicates that the management can not alone be blamed for the decline in the cod fishery - the important role of the out-fading gadoid outburst must not be left out. I agree - but as qualified in the introduction. There is evidence that the stock biomass potentially can be controlled - at least so some extent, but the effect of the SSB on recruitment is not

TACs and landings

If the scientific advices were followed by the managers it could suggest that the decline in stock was because the fishermen did not respect the quotas. The latter, however, seems not to be evident from the data on landings (figure 5a).

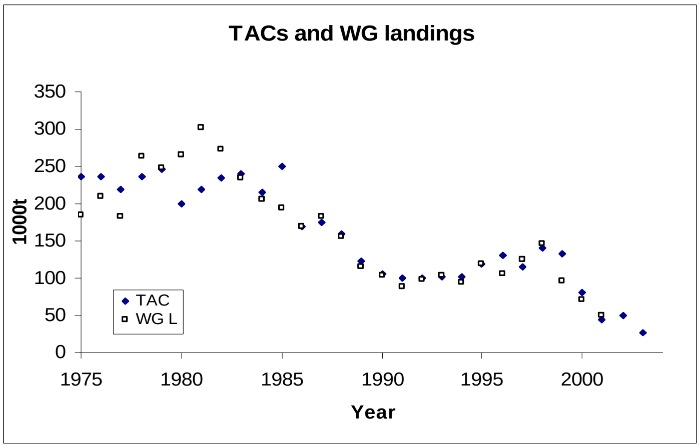

Figure 5a. Agreed TACs and Working Group landings. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). Data and sources: Table 3, appendix 1.

The first quotas (1975-1978) seemed not to be restrictive since they were not taken. In the early 1980s the quotas were exceeded considerably, indicating a highly insufficient enforcement.

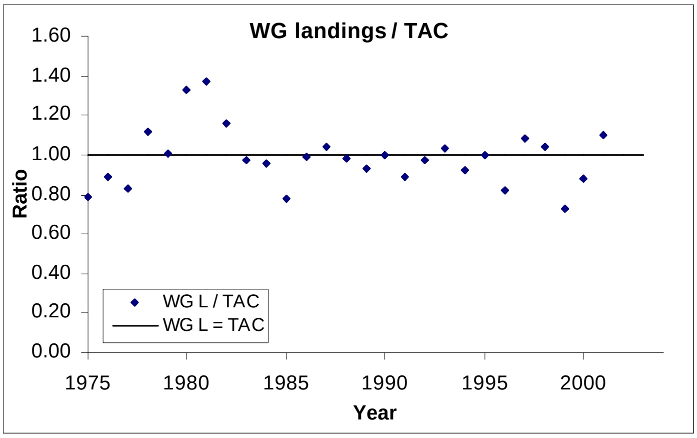

Figure 5b. Ratio of the Working Group landings to the agreed TACs. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). Data and sources: Table I, appendix 3.

known. We can apparently not control the number of fish to enter the fishery, but we can, somewhat, decide when to harvest them.

From the mid 1980s to the mid 1990s, the deviations between the TACs and the landings are relatively small and equally spread around the line of equality (fig. 5b). From 1996, the fishermen have apparently not been able to fish the quota half of the times. Note, however, that estimates of mis- and under reporting were generally not included in the landing figures. 1998 was an exception from the latter - but here WG landings only exceeded the nominal landing by 5% anyway. At first sight it would make sense to assume that there is little mis- and under reporting in the years where the TAC is not taken. But that is not necessarily true, since there could be differences between regions or vessels. Whereas some possibly are not able to land their quotas others would land the excess illegally.

Recommendations and landings

In the figures 6a and 6b the recommendations given by ACFM are compared to the landings as used by the working group. There is more spread around the line of equality here than in the other plots, which is expectable since the link between scientists and fishermen is indirect compared to the scientists-managers link and the managers-fishermen link. The average ratio of the recommended TAC to the working group landings is 1.04, indicating that the advised TAC tends to be a little *larger* than the landings (Table I, appendix 3).

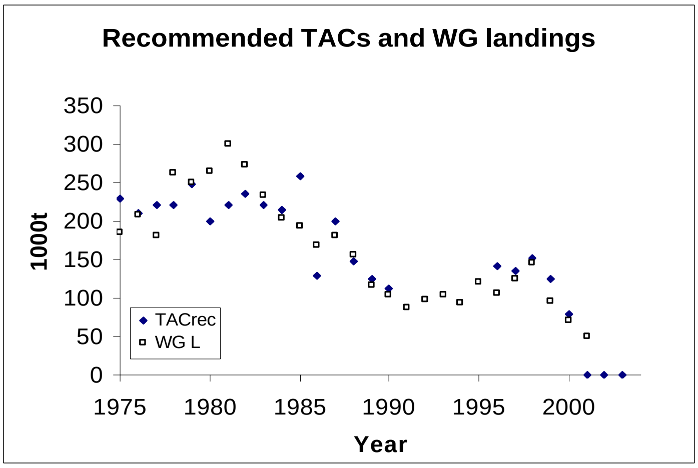

Figure 6a. Recommended TACs and Working Group landings. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). Data and sources: Table I, appendix 3.

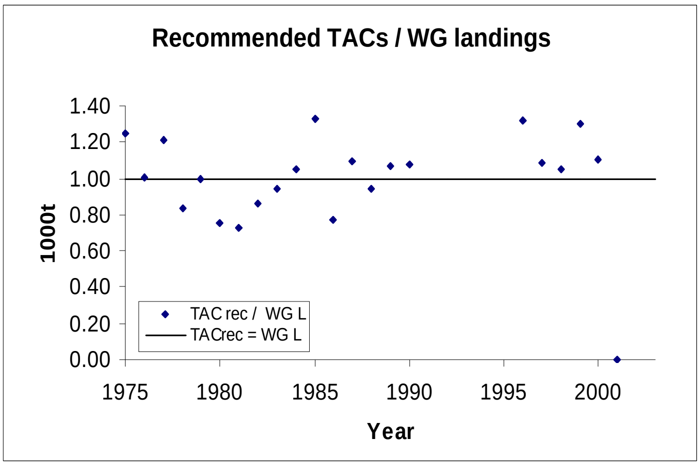

Figure 6b. Ratio of recommended TACs to the Working Group landings. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrack (IIIa) and Eastern English Channel (VIId). Data and sources: Table I, appendix 3.

To summarise, the analysis of recommendations and implementation of TACs and the landings (nominal or as used by the working group) seem to shed little light on why the stock declined. Instead we are left with two new questions: If the landings in general were not greater than recommended by the scientists, why did the SSB continuously decrease? And if the quotas were set with the aim of first stabilising and then reducing the fishing mortality, why did it continue rising? These questions are so intimately related that they can be rephrased in the following question: Were the quotas really effective in regulating the fishery?

Now, let us address the period from 1991 to 1995. Why was there no recommendation for TAC for these years? In its report of 1990 (CRR: 173), ACFM clarified problems of TAC regulations in this fishery. The TACs were ineffective and the explanation given by ACFM can be summarised as follows. In order to be effective the TACs must be below catching capacity, but since there is no limit on effort, the catches are in excess of TACs and the excess is either discarded or landed illegally. In other words, the TACs control the official landings but *not the mortality inflicted by fishing on the stock*. As will be discussed later, these views were not entirely new. However, they had not been emphasised as strongly before in the ACFM reports. For the year 1991 ACFM accordingly abstained from giving a TAC recommendation. The advice was instead to cut effort by 30%, which ACFM claimed was the only effective way of reducing F.

The assessments: Retrospective analysis of SSB and F

Before we proceed with the reasons mentioned by ACFM above for the TACs not being effective, I want to consider another quite obvious possibility: What if the TACs were not really below the catching capacity? Could it be the case that the TACs were ineffective simply because they were set too high? As it turns out, it may be difficult to separate these possible causes of the failure of the TACs. Since the TAC recommendations are dependent on the perception of the stock condition it is necessary to examine the assessment history. As time passes, the stock history is revised. There is consequently often a discrepancy between what condition the stock was thought to be in a certain year, and what condition the stock is now believed to have been in that year - as seen by the latest assessment. Obviously, only the historical assessment is relevant to the management decisions, but the stock crisis can develop unnoticed and thus only be seen in retrospect. To the degree that the development of the crisis was not noticed, it is obvious that the management system would be incapable of avoiding it.

In this section I will try to analyse the changes in the perception of the stock condition. Ironically, the assessment is here the subject of a trial of which it is also the judge, which underscores the ambivalent role of knowledge in explaining the crisis.

It is relatively easy to carry out such a retrospective analysis. It is simply a question of comparing the first estimates of the stock parameters of a given year with later estimates concerning the same year. Again all the information needed is present in the ACFM reports. The same type of data is also present in the working group reports - but the ACFM is the final advisor and it is therefore its advice that it is most relevant to analyse.

Historical and current SSB estimates

It is evident from fig 7a, that the SSB estimates have been quite accurate except for two periods: The early 1980s and from the mid 1990s and on. But in these two periods the SSB was considerably overestimated.

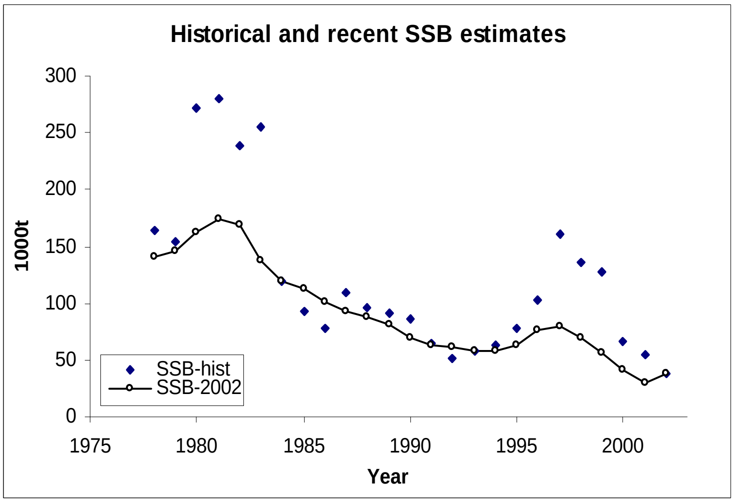

Figure 7a. Historical and recent (2002) SSB estimates for Cod in the North Sea. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrack (IIIa) and Eastern English Channel (VIId). The 2002 assessment was rescaled to cover only the North Sea for the years before 1996 (see appendix 2). Data and sources: Table II, appendix 4.

When would you start to worry seriously about the stock? In 1983 the SSB estimate was 255 (CRR: 128, 1983), in 1984 it was 120 (CRR: 131, 1984). This could be called a "collapse", but as will be described, it was a *virtual* collapse: a collapse that never happened. Further, this collapse did not worry scientists too much. Yet, from the mid 1980s the continued decline of the stock was correctly perceived, which did worry the scientists.

In 1992, the SSB was estimated to be only 51.000t and the state of the cod stock was termed "critical" (CRR 193, 1992: 77-80). 1992 was the culmination of the *first* crisis. Then the SSB was perceived to increase again. The optimism topped in 1997, when the SSB was believed to be back on the right side of the precautionary reference point of 150.000t - for the first time since 1983.[4] As seen by the latest assessment, the stock did recover a little from the mid 1990s but the recovery was severely inflated in the historic assessments. From 1997 to 1999 a moderate decline was perceived - but the situation was actually much worse, since the SSBs of 1997, 1998 and 1999 were overestimated by about 100%. The second virtual collapse was in 2000. Now the seriousness of the stock state was realised - even though the SSB estimates of 2000 and 2001 assessments were also inflated considerably (by 38% and 45% respectively).

[4] There were, however, some dubious comments by ACFM in the 1997 report (CRR 223, 1997) with respect to whether SSB was on the right side of the MBAL (as it will be apparent in the next chapter).

As before it may be useful to analyse the ratio of the historical SSB estimate to the 2002 estimate, since the impact of overestimation is relative to the stock size.

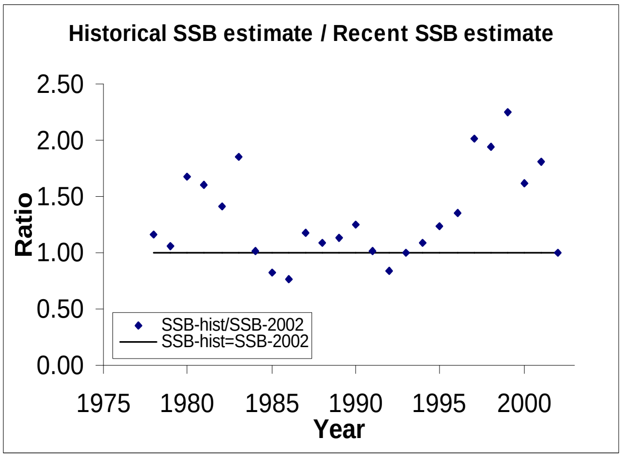

Figure 7b. Ratio of Historical SSB estimates to recent (2002) SSB estimates for Cod in the North Sea. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). The 2002 assessment was rescaled to cover only the North Sea for the years before 1996 (see appendix 4). Data and sources: Table II, appendix 4.

From figure 7b it is clear, that even the latest over-estimations were not the largest in absolute terms, they were more serious in relative terms. From 1984 to 1996, the SSB estimates were quite *accurate* (the mean was unbiased as compared to the 2002 estimates) but had low *precision* (there is considerable deviation from the mean) [5]. Figure 7b may inspire the idea that SSB is overestimated following a recovery period (when things are improving, you predict them to continue improving). There is however a better explanation for the extreme estimates of the early 1980s.

As mentioned the 1983 SSB estimate was 255 (CRR 128, 1983) whereas the 1984 value was 120 (CRR 131, 1984) - a decline of 113%. In one year the SSB went from one of its highest recorded levels to below what later was defined as the lowest desirable level (MBAL of 150). However the 2002 estimates, rescaled to cover only the North Sea, are 138 for 1983 and 119 for 1984, which is a decline of 16%. How was this possible? Let us look in greater detail at the assessments of those years. A more detailed picture is available when we use the data from the working group reports, where the estimates of the stock parameters of previous years are present (later these were also included in first graphs and tables in the ACFM reports).

[5] The trend in estimates from 1992 to 1997 could be taken to indicate an initiated and increasing bias.

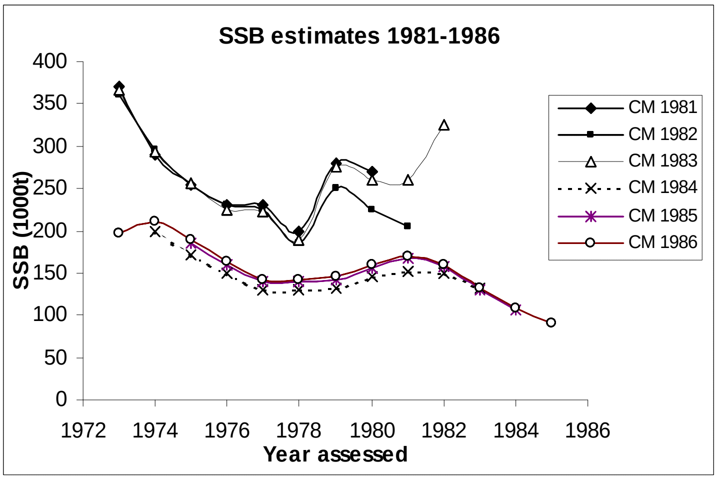

Figure 8. Series of SSB estimates for Cod in the North Sea as assessed from 1981 to 1986. Data and sources: Table III, appendix 5.

Figure 8 is a somewhat complex, but its elements are quite straightforward. Each line represents the perceived stock history with respect to SSB since 1972 - as seen from the assessment of a given year between 1981 and 1986. The 1983 assessment is obviously quite optimistic, which explains much of the difference between the 1983 and the 1984 assessments. But more importantly, there is clearly a systematic difference between the assessments prior to 1983 and those of 1984 and later.

As it is evident from figure 9, the systematic difference was due to two different ways of dealing with sexual maturation of the cod. Until 1983 the assumption of knife-edge at first maturity was used. This means that you assume that all cod are immature until a certain age after which they are all assumed to be mature[6]. From 1984 and onwards, the working group found it preferable to use a more detailed (and realistic) maturity ogive, where a proportion of each year class is assumed to be mature (C.M. 1984/Assess:10: 17).

[6] Actually is not apparent from the 1984 working group report how the knife-edge assumption was specifically used. It could for example be applied such that all from a certain year class are mature or such that 50% of a certain year class are mature.

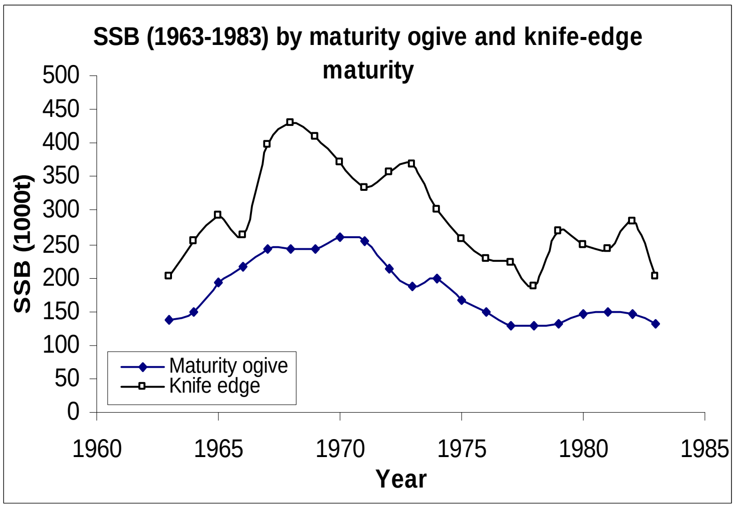

Figure 9. Series of SSB estimates for Cod in the North Sea assessed WG in 1984 by applying two different assumptions with respect to the maturation of the cod. For the upper curve a knife-edge maturity of age 3 was used. For the lower curve a maturity ogive (as given in table IV appendix 5) was applied. Source: Redrawn from Fig. 17.1, CM 1984: assesses 10.

If we return to fig. 7a and 7b, we can explain the first series of over-estimations. These were partly a result of an overoptimistic 1983 assessment but mainly reflected changes in the assumptions of maturity in the assessment. Further, the assumption of knife-edge maturity could seem to imply greater fluctuations in SSB (fig.9), which could explain some of the (seemingly) lower precision of the estimates before 1984. Now it could seem puzzling that the 1978 and 1979 are very close to the 2002 assessment values and much lower than the assessments from 1980 to 1983. This is nevertheless explained by the fact that the 1978 and 1979 SSB assessments were carried out on the assumption of a knife-edge maturity of age 4, whereas the 1980-1983 assessments were based on a knife-edge maturity of age 3.[7]

Therefore the bias in the SSB estimates of the yearly 1980s is now explained. There is no need to examine the period with no bias, since that would shed little light on why the TACs may have been to generous. Let us therefore turn to the post 1995 period, which would seem most important for an explanation of the current stock crisis. As before we can look in greater detail at the assessments by plotting the series of SSB estimates from each assessment, which is done in figure 10.

[7] It is therefore plausible that the 1978 and 1979 assessments would represent considerable over-estimations if the maturity ogive was applied to these instead of the assumption of knife-edge maturity of age 4. This could be calculated quite easily, but is not within the scope here.

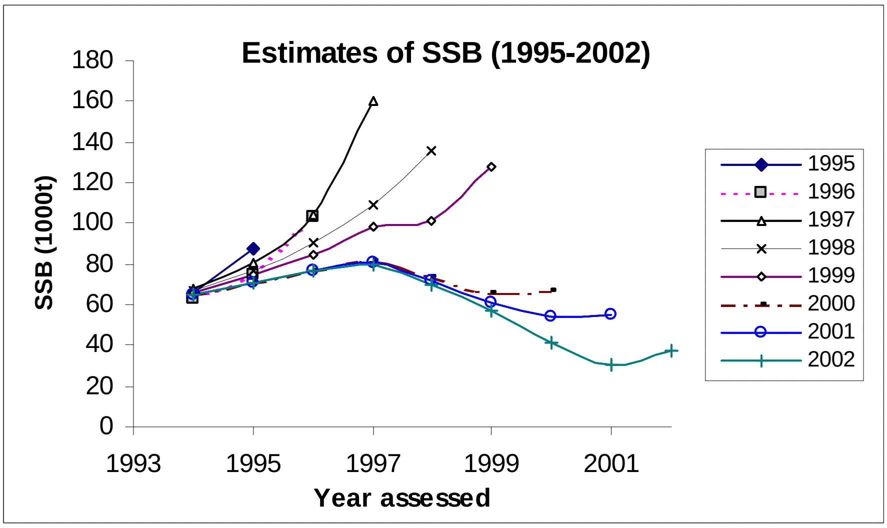

Figure 10. Series of SSB estimates for Cod in the North Sea as assessed by ACFM from 1995 to 2002. Sources of data: Table V, appendix 5.

What is important to the managers in a certain point of time is the endpoint of each graph, indicating what the contemporary SSB is perceived to be. If you only look at the SSB graphs in figure 10 as if you only see one at the time, what would you think?

From 1996 to 1997 you probably thought that the SSB was increasing rapidly - the crisis was over - you were “covered” by the B_{pa} reference point. In 1998 and 1999 it turned out that the increase in SSB was slower than you first thought - but it was still slightly increasing, so there wasn't too much to worry about. You were not at B_{pa} but you were approaching it steadily. Then suddenly in 2000 the bomb is detonated: The increase until 1997 was actually very modest - and it was over. The decline has actually been going on for two years - but the SSB, fortunately, had stabilised since last year. In 2001 you realised that the SSB did not stabilise in 2000, but fortunately it had stabilised now. In 2002 you find out that the SSB did not stabilise in 2001, but fortunately it had now stabilised. So can we trust that it has really stabilised now?

The perception of the rate of increase is consistently changed and the perception of the rate of decline is consistently changed. But the perceptions of the end states and the rates that are in accordance with the end states are not the only perceptions that change. The stock history also changes in accordance with the changes in other perceptions. Take for instance the year 1997. The SSB of 1997 was first assessed in 1997 to be 160. It was reassessed in 1998 to have been only 110, in

1999 the figure was down to 98, then in 2000 it was 81 for then to apparently stabilise on 80 in 2001 and 2002 - 50% of the first estimate. The pattern of revising down not only the current estimate but also the previous history is consistent. The last couple of years were subjected to the largest revisions. As before, the most drastic change happened between the 1999 and 2000 assessments.

Note that I don't say what comes first: The perception of the current state, the rates to achieve it or the interpretation of the precedent stock history. It probably all comes together, but that is an issue to be discussed later.

Historical and current F estimates

Since the recommendation and management considers not only the SSB but also the fishing mortality we obviously need to make an analysis for the fishing mortality similar to that of the SSB above.

An F estimate for the year of assessment is usually not given in the ACFM reports, since the last value F is the most uncertain to estimate. Therefore, the F analysis will be based on the F estimate given for the year prior to the year of assessment in this analysis. The F estimate is really a weighted mean of a range Fs for different year classes. Usually F was expressed as the weighted mean of the Fs of ages 2 to 8 (the main part of the fishable stock). For the years 1981-1985, however, the F estimates were means of ages 3-8. Therefore these Fs are not strictly comparable. The $F_{(3-8)}$ could quite straightforwardly be recalculated into an $F_{(2-8)}$ value. Yet, the latter did not seem to be necessary for the present purpose, since there seems to be no clear systematic difference between these types of Fs (table VI, appendix 6).

Since 1996 ACFM has not calculated separate Fs for the sub-areas. Yet, the 2002 assessment is compared to the assessment for the North Sea area separately as regards the assessments prior to 1996. This appears wrong. However, I find that the approach is legitimate in the case of Fs. The reason why the areas were combined was that they could not really be assessed separately because the fish move around between the areas. I want to analyse if the assessment was biased in some way. Therefore, if it was somehow biased because of the area problem, I should really not try to compensate for it in the analysis. Anyway the difference is this regard is small.[8]

[8] Differences between the F series as estimated in 2002 and 1995 (respectively) are, as was the case with the SSBs, quite small and not systematic (figures IIa and IIb, appendix 7). Generally the

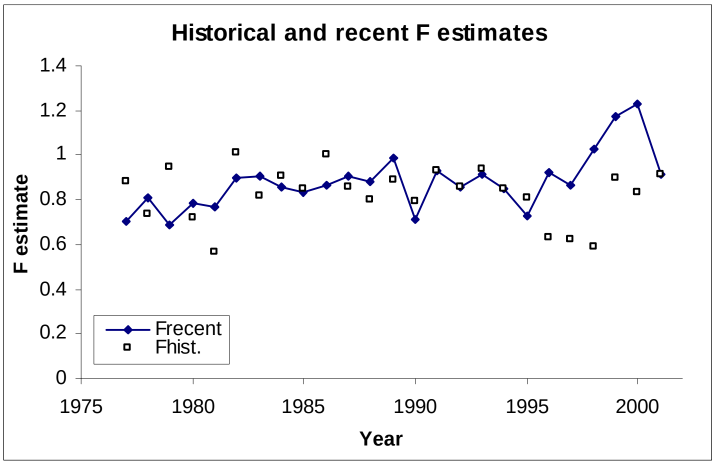

Figure 11a. Historical and recent (2002) SSB estimates for Cod in the North Sea. The historical series is for the North Sea only until 1996. From 1996 the estimates are for the combined are of North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). Data and sources: Table VI, appendix 6.

From figure 11a it is apparent that F estimates tend to fluctuate more between years than SSB estimates. The trends and levels of the historic estimates generally are the same as in the 2002 assessment - with the clear exception being the period after 1995. From 1996 to 1998 F estimates of the previous years were around 0.60. From 1999 to 2001 the level jumped to around 0.85. But in retrospect, the F was around 0.95 when you thought it was 0.60, and when you thought it was around 0.85 it was at the level of 1.20. Since the last F estimate is the least certain we still do not really know if the latest perceived decline in F in 2001 is real. There is no estimate for F in 2002 yet.

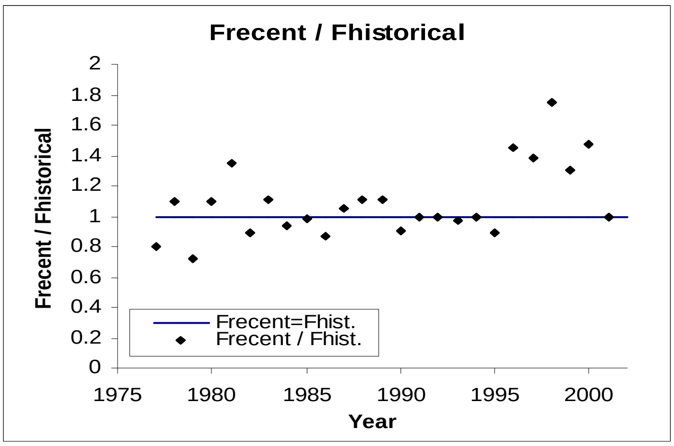

Figure 11b. Ratio of historical F estimates to the recent (2002) F estimates for Cod in the North Sea. The historical series is for the North Sea only until 1996. From 1996 the estimates are for the combined are of North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). Data and sources: Table VI, appendix 6.

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differences are within 2% to 3%, though the spread (as expected) increases with time. The trends are similar and the largest difference is 8% (i.e. for 1990).

Figure 11b shows that F was quite accurately estimated until 1995 (as viewed from the 2002 assessment). Further, the precision of the estimates increased during the period. The fact that F was quite accurately estimated prior to 1984 is in accordance with the explanation of the overestimation of SSB as a "virtual" phenomenon for that period. When SSB is biased, you would expect the F to be biased as well since F and SSB is estimated together (as will be explained in chapter 4.2).

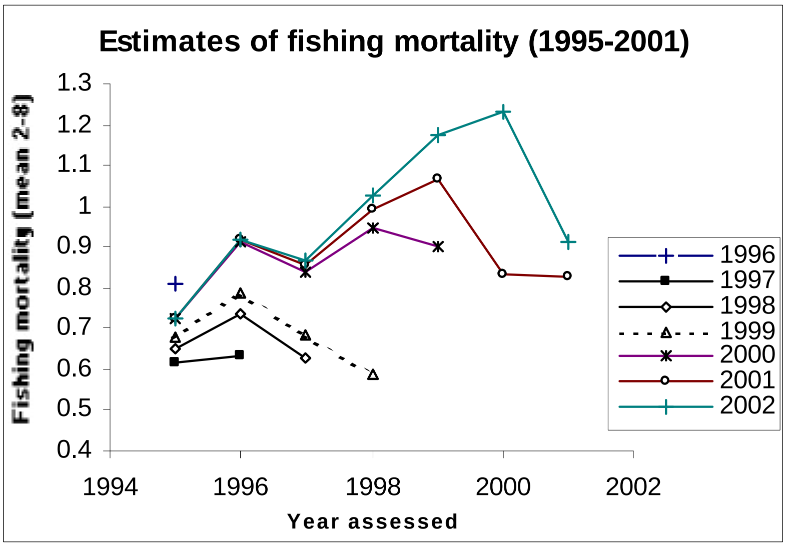

Figure 12. Series of F estimates for Cod in the North Sea as assessed by ACFM from 1996 to 2002. Sources and data: Appendix 8.

As before we can make a more detailed analysis of F estimates of the period of 1995-2001 (figure 12). Since the first F estimate given in the ACFM report usually is from the succeeding year, the series of estimates concerning these years are from 1996-2002. F in 1995 was, as the only year in the series, overestimated (in 1996). The 1995 value was revised to be an underestimate in 1997 for then to increase gradually halfway back towards the first estimate.

From 1997 to 1999, F estimates of the previous year were around 0.60. From 2000 to 2001, the level jumped to around 0.85. The most dramatic change was, as was the case with the SSBs, between the 1999 assessment and the 2000 assessment. The two last assessments are alike in claiming that the F was very high two years before but that it then decreased – a structure that resembles the one of the SSB estimates.

Let us sum up a little: We have seen that SSB and F have been quite accurately estimated until 1995, although the use of different maturity factors distorted the SSB estimates of the early 1980s. The precision F estimates increased until 1995, after which the assessments get out of hand. From 1997 to 1999 the SSB estimates are inflated by about 100% (125% in 1999). We also saw that over-estimations of SSB were linked to under-estimations of F, which is expectable from methodological reasons (as it will be explained in chapter 4.2). The 2000 and 2001 estimates were inflated by about 60% and 80% respectively. Similarly F has been underestimated considerably since 1996. For the period 1996-1997, the last F estimate is about 40% higher than the historic value. Similarly the 2002 estimates are 74% higher for 1998 and 30% and 50% higher for 1999 and 2000, respectively, as compared to the historic estimates (table VI, appendix 6).

The base of the advice: Short-term predictions

Let us now return to the issue of the effectiveness of the TACs. Obviously the manager would be inclined to set the TAC lower if he sees that the SSB is low or the F is high. The overestimation of SSB and the underestimation of F could in this way contribute to the low effectiveness of the TACs in protecting the stock. However, we saw that the managers actually opted for TACs lower or about equal to the maximum recommended by the ACFM in the period from 1996 - 2000. How would the assessment biases affect the TACs? In order to see that, we need to consider how the TACs are actually decided upon by the *catch predictions*.

As mentioned earlier, the TAC advice is not a single figure and the scientist generally recommended large reductions in F. Prior to the advice for 1981 the advisers presented the managers with very limited options. On a dialogue meeting managers argued for freedom of choice - they wanted to know the consequences for the stock of different catch levels (CRR 102, 1980). This increase in freedom was granted from the first time in 1981 advice in form of catch options (CRR 114, 1981). The catch options depict - in tables and graphs - the relation between F and yield in the year for which the quota should be decided, and what SSB will result at the end of the year, when the TAC is taken.

Beek and Pastoors (CM R04, 1999) conducted a similar evaluation of the catch forecasts of the most important demersal North Sea stocks. However, these writers used the catch forecasts as provided by the Working Group. With respect to management it is more relevant to examine the catch forecasts as provided by ACFM since only the latter are of importance to the managers. There are some differences between the catch forecasts of the WG and that of ACFM. The ACFM catch prediction tends to be slightly more conservative than that of the WG, although the difference in the later years has been quite small.

Predicted F values

Since these catch options are the base of the management decisions in the negotiations for the TACs, they are absolutely central to the management decision process. The catch option can be seen as a contingent prediction, since it predicts what yield will result from a given fishing mortality, and what the SSB accordingly will be in the beginning of the year after the TAC is taken. In order to examine the appropriateness of the advice as catch option, it is useful to turn this relation around, since we know what the yield (landings) was for a given year. Thus if we provide the catch-option relation for a given year with the actual landings for that year it is possible to determine what ACFM accordingly would have predicted the F to be.

Since the catch option table only provides a limited number of pairs of Fs and yields and because reading from graphical representation of catch options would be relatively imprecise, a regression between the yields and Fs was calculated for each years catch option table. A second order polynomial regression was found to be very suitable (all R^2s were >0.998). The polynomial was constrained by an intercept equal zero, since no yield logically implies a zero fishing mortality. Applying the actual landings to the polynomial equation resulted in a predicted value for F. These values were confirmed by the graphical representation of the catch options given in the reports (except for the 1998 prediction)[9].

Actual F values

The predicted values of F given the actual landings can now be compared to the "actual" or realised F as estimated by the latest assessment available. The latest (2002) assessment was also used to give the “actual Fs” prior to 1996, even that F was

[9] For 1998 the predicted F value was 0.49 according to my calculation, whereas it is around 0.40 in the graph. I trust my calculated value more, since the graph could be the result of one misprint, whereas the calculation would imply misprints of 5 values in the table. Further, the graphed value would *strengthen* the significance of the results. I have therefore used the calculated value.

predicted for the North Sea only until in 1996. This is justified by the same reasons for which the assessment has been carried out for the combined area since 1996. If the fish moves around quite freely between these areas, then the proper "actual F" is for the combined area. Again, there is, however, little difference between the latest, North-Sea-only estimated F series, which was given in the 1995 report (CRR 214) and the latest series of estimates for the combined area in the 2002 ACFM report. The averages of the Fs between 1981 and 1995 differ by less than 1% between these two assessments, and the trends in the series are similar. As before it consequently matters little if the one or the other series is used.

Actual vs. predicted F

As it is evident from figure 13, there is no "proper" relation between predicted F and actual F. It is tempting to compare the predictions of F to a gunshot – in the wrong direction. The slope of a linear regression (not shown on graph) is *negative* and significant on the 5% level (p<0.05, a = -1.12 +/- 1.00 for 95% confidence limits and the intercept is significantly *positive* (p<0.001, b = 1.69 +/- 0.91 for 95% confidence limits. This is of course absurd, since the negative slope, a, indicates that predicted Fs decrease with increase in actual Fs, whereas the intercept, b, implies that the predicted fishing mortality - when there is no fishing - is positive (the estimate 1.69 is higher than any F estimate recorded for this stock).

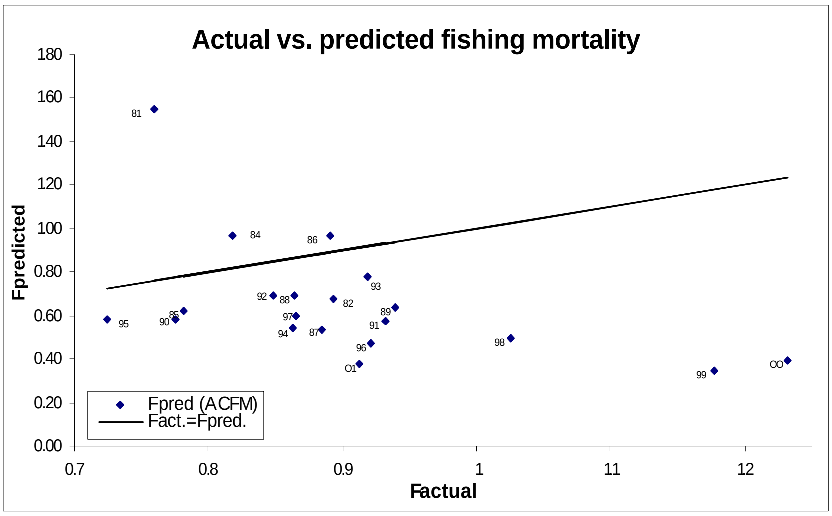

Figure 13. Predicted F (calculated from ACFM catch options as described in text) plotted against actual F from the latest assessment. The actual Fs prior to 1996 are from the 1995 assessment, which as the predicted values covers

the North Sea only. From 1996 both predicted and actual values are for the combined area. The straight line marks where the predicted F value is equal to the actual F. ACFM did not predict any F for 1983 (due to problems of assessing the stock).

Earlier predictions, however, tend to be more accurate than later predictions and are also less biased since some of the predicted values in the 1980s are higher than the actual values (1981 is a lonely extreme outlier with a predicted F being much higher than the actual). Later in the series, the predicted values are considerably biased such that the predicted F is much smaller than the actual F. Since 1996 there has been a large and increasing discrepancy between the predicted and actual F.

A very important observation is thus that catch predictions are *useless as predictions*. The scientist would have much higher success by saying that the F next year will be similar to the F last year (regardless of the quota), than by use of the catch forecast. Beek and Pastoors (CM R04, 1999) basically obtained a similar conclusion when they examined the catch forecasts of WG for the cod stock.

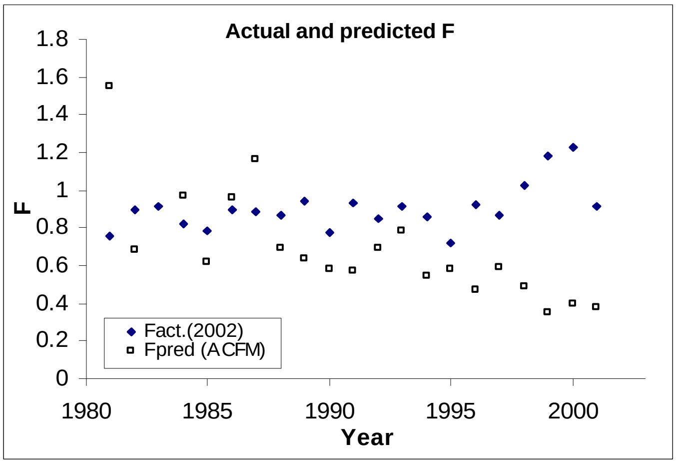

Fig.14a. Actual (ACFM 2002 estimates) fishing mortality and.fishing mortality as predicted by historical short-term forecasts (given actual landings) as explained in text. ACFM did not predict any F for 1983 (due to problems of assessing the stock).

Another way to explore the relation between predicted F and actual F is by a time series analysis as in fig.14a. Now a pattern is more apparent. The actual Fs have increased during the series and most dramatically so since 1995. On the other hand the predicted Fs have generally *decreased*. There are large fluctuations in the series of predicted F – especially in the early part of the series. Beek and Pastors (1999) did not perform such a time series analysis, but probably trends would have been apparent for their data series too (1983-1994 plus the year 1997). It is nevertheless evident, that the bias first escalates after the end their series.

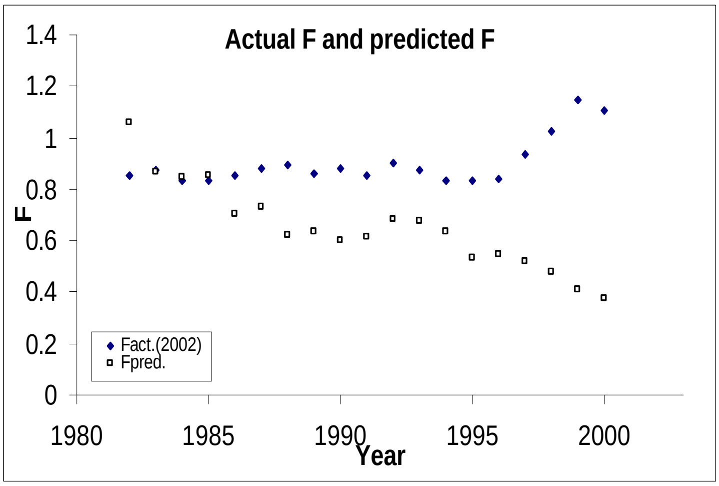

Fig.14b. 3-year running averages of actual fishing mortality vs. fishing mortality as predicted by short-term forecasts (given actual landings). For 1983 ACFM did not predict any F. In order to produce the graph a predicted F value for 1983 was taken to be the average of the predicted value of the two previous and the two succeeding years.

In order to clarify trends it may be useful to smooth the series by running averages (figure 14b). The individual points of such a plot make little sense, since the prediction in one year was a certain value and not an average of anything - as is the case with the actual Fs). Yet, the running averages make sense with respect to the possible explanation of the crisis since it is suitable to analyse effects on the stock on a basis of trends and averages.

What is evident from fig. 14a and 14b seen together, is that the predictions of the early 1980s were quite *accurate* but had a very low *precision*. After the late 1980s the relation turned around - the accuracy decreased whereas the precision increased. The catch forecasts showed more “consistency” between two consecutive years but the series of predicted F became increasingly more biased. The bias was rather constant from the late 1980s until the mid 1995s, when it increased dramatically. For the two last smoothed averages in the series, the predicted F is only about one third of its actual value.

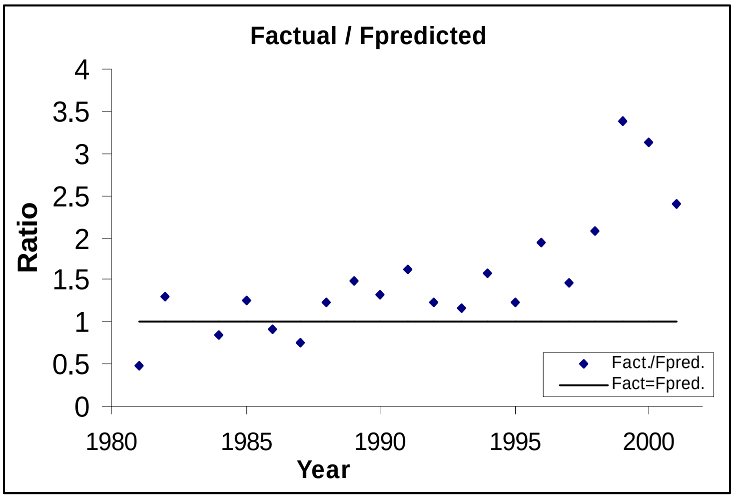

Fig.14c. Ratio of "actual" (ACFM 2002) F estimates to fishing mortality as predicted by historical short-term forecasts (given actual landings) as explained in text. For 1983 ACFM did not predict any F (due to problems of assessing the stock).

The ratio of the actual F (as estimated in 2002) to the predicted F is depicted in figure 14c. The bias in predicted F was quite constant in the decade from 1985 to 1995, with the predicted F being on average one third higher than the actual F. In the late 1990s, the bias increased dramatically. In 1996 and 1998 the "actual F" was about the double of the predicted F. In 1999 and 2000, the actual F was more than 3 times higher than the predicted from the catch forecasts. For 2001 (the latest F estimate) the assessed F was about 2.5 times the predicted value.

The TACs don't work

To sum up, the predictions are not only useless but also quite dangerous to follow with respect to TAC, since high actual Fs are associated with low predicted values. Say that a manager thinks that the F is too high. From the catch forecast he will think that the stock will recover by setting a low TAC resulting in a low predicted F. But the actual F will nevertheless be very high.

The catch prediction is a mathematical derivation of the effect of a certain catch with respect to F and SSB. It is, so to speak, accounting within a stock assessment model. It has been shown that the actual landings put into this accounting system yielded predicted Fs that were much lower than the actual Fs seen in retrospect. The TACs were generally in accordance with the recommendations for maximum TACs, but they did not have the intended effect: To stabilise and to reduce F and rebuild SSB. It therefore follows that the TACs are not effective in regulating F (and SSB) for this fishery.

As mentioned before, there are two types of reasons for the TACs not being effective. Either the TAC recommendations were too high (so they did not constrain catches) or they were not too high, but they did not regulate F for the reasons explained by ICES in 1990. However, these reasons are not mutually exclusive - they could be complementary.

Now return to the SSB assessments in figure 7a and 7b and the F assessments in figure 11a and 11b. As noted above, F was accurately assessed with increasing precision until the mid 1990s. For the SSB there was some overestimation of another kind due to the use of certain maturity assumptions in the early 1980s. However, the SSB was quite accurately estimated (there was no serious bias) between 1984 and 1996, though the precision was lower than for the Fs. Yet, the discrepancy between actual Fs and predicted Fs is significant from the late 1980s (figures 14a, 14b and 14c). The bias from the late 1980s to the mid 1990s can thus not be explained by a bias in the stock assessment, which would result in the recommendation for maximum TACs being too high. This bias is therefore likely to be explained by the reasons stated by ACFM in 1990.

The discrepancy between predicted and actual F escalated dramatically since the mid 1990s. At the same time there was a dramatic overestimation of SSB and underestimation of F. Overestimation of stock and underestimation of associated fishing mortality implies too generous catch forecasts – and therefore TACs that are neither restrictive nor effective. Figures 14a, 14b and 14c suggest that the bias in catch predictions due to the reasons stated by ACFM was quite stable from when it developed in the late 1980s and until the onset of the effects of the assessment bias. This could suggest that this effect was also stable after the mid 1990s and that the assessment bias explains the rest. Personally I find the latter to be the most likely explanation from comparing the sets of figures (7a,b; 11a,b and 14a,b,c) but it would require a more detailed quantitative analysis to examine these issues closer. I will stop my quantitative analysis here since I want to address some other important issues of the perspective of knowledge in explaining the crisis.

Intended F reductions

Another way to see that the TACs did not work is simple to compare the objective of the recommendation with respect to F reduction (the basis of the recommendation) with what was achieved in terms of F reduction. The recommendation was given in terms of catch corresponding to a desired percentage reduction in F as relative to F in some defined base year (for example F two years ago or the mean F of the last three years). In the 1985 report the recommendation was based on a 25% reduction in F compared to a base F, in 1986 and 1987 the intended reduction was 30%, in 1988 and 1989 the intended reductions were 20%. If all these reductions in F had come through, it is easy to calculate that F in 1990 would be *less than 25%* of what it was in 1985. But F did not decrease - instead it kept increasing.

Chapter 3: Qualitative analysis of the recommendations

Known and unknown problems

In the analysis part it was shown that the failure of the cod management seemingly could be related to two main issues: Over-estimations by the scientific advisors and ineffectiveness of the TAC system. Naturally, the problem of over-estimations was not known (although there were some suspicions). The over-estimations, and the scale of them, took scientists and managers by surprise.

Was it known that the TACs did not work? This is a simple question, but the answer may be quite complex. We need to explore the assessment and management history further in order to get an idea of that. However, to us *the surprise seems inevitable*. For on the one hand it would be surprising if it was *not* known – after a quite apparent and steady decline of the stock and a concurrent increase of F through a quarter of a century. But on the other hand it would also be surprising if it *was* known since that would make it somewhat incomprehensible that “business as usual” was continued.

However there is more to explore, since the TAC recommendation was not the only advice given. Or you may say that the managers trusted the maximum recommendation for TACs but they did not follow the general recommendation by the scientist: To reduce the effort and the fishing mortality. These questions will be examined more closely in the following.

History of advice

In the former chapter I have analysed the recommendation and use of recommendation in a relatively crude and quantitative way - focusing only on TACs. It was concluded that the managers generally followed the recommendations for *maximum* recommendable TACs. However, it was noted that the advice had other features than these numbers and it is now the time to address these other features. Due to the complexity of the issue I will not analyse *all* the features of the advice here. Some perspectives of the advice, which are of a more fundamental nature (objectives, form of advice, type of regulation) will be addressed later.

In the presentation of the history of advices I will continue focussing on the SSB and the fishing mortality. These are naturally important elements in the developing discourse. Further I will present and describe the development of 3 important themes, which were discussed and developed through the history: *technical measures* (mainly mesh-size changes)[10], *technical interactions* (when fishing gear catch different species) and *species interactions* (management of one species influences the stocks of others). The development of these themes contributed to the increased complexity but also uncertainty of the advices.

The advices of the 1970s: "Straight answers"

The first TAC recommendation from 1974 (CRR 49, 1975) intended to give recommendations for 1975 in order to a) maintain the F at its 1973 level or b) reduce F by 50% to 60% (to about F_{max}, the maximum sustainable yield). Consequently two numbers were presented: 230.000t for a) and 120.000t for b). These numbers were revised from the former meeting in the Liason Committee, where the figures were 250 and 130 respectively.

The recent landings were said to be very high because of strong recruitment. It was noted that the fishing mortality for cod was higher than that of maximum sustainable yield per recruit, which would be obtained at 40% to 60% of the current F and result in an increase in yield per recruit of about 33% (CRR 49, 1975).

The issue of technical measures was addressed in that the issues of changes in minimum allowable mesh-sizes were addressed. The effects of increased mesh-sizes were presented in tables showing immediate losses and long term gains in yield for cod, haddock and whiting. It was explained that the calculations were based on single species models for each species separately, since neither the data nor the knowledge required for multi-species modelling of the mesh-change were available. In a conclusion was stated that there should be a long term increase for all species, except whiting from an increase in mesh size to at least 90 mm (the mesh-size at the time was 75 mm.) (CRR 44, 1974 and CRR 49, 1975).

In the 1976 report it was (laconically) stated that the NEACF had set the TAC higher than recommended for 1976 (the TAC was 236, the recommendation 210) and that F would have to increase above the current level by 45% to catch the cod quota (CRR 56, 1976).

[10] I have omitted the discussion of the closed area measures, e.g. the "Cod Box" that was introduced in in order to protect juveniles. The history of the cod box is in many ways similar to that of the mesh-sizes (see for example CRR 168, 1989).

In the 1977 report, problems of estimating F were admitted but there was a "general agreement" that the effort was too high and should be reduced. The effort reduction was recommended to be carried out in 10% steps. It was noted that the current exploitation pattern was wasteful and the Committee reminded of the benefits of a mesh-size change as presented in the CRR 44 and CRR 49 reports (CRR 73, 1977).

In 1978 the Liaison Committee was replaced by ACFM (CRR 85, 1978). ACFM stated that it would proceed with the policy of its predecessor with respect to the policy of small, stepwise, recommendations in F. The annual yields and the spawning stock biomass were said to have declined significantly since the end of the 1960s although they were still above pre-1960 levels. This suggested that the roundfish stocks were "severely over-exploited", but none of the stocks were in "immediate danger of recruitment". For the first time the current F estimate was *presented* in the report ($F_{(8-2)} = 0.88$), where it was compared to the much lower F_{max} (F_{max}=0.33).

In the 1979 report the recommendation was to revise the (already agreed) TAC = 183 *up* to 247, since the TAC of 183 in a revised assessment would imply a reduction of F that was greater than "envisaged" at the ACFMs previous meeting (e.g. the policy of 10% annual reduction). It was stressed in the introduction that the state of the stocks and the yields from them could be "…improved much more dramatically by improving the exploitation patterns by way of appropriate mesh changes, than by reducing the fishing mortality rates."

The early advices were quite brief and contained little discussion of the methods and the numbers. The TAC numbers seemed to reflect a quite strong confidence in the assessment estimates since the recommendation of 1975 was revised by less than 10%. To the manager with little knowledge of fisheries biology, the reports would probably seem difficult and maybe somewhat puzzling at times. The biologists were talking about a fishery mortality factor, which they strongly recommended to be reduced but they did not explain what it meant or neither did they show any values of it until 1978. In the 1976 report the discrepancy in the magnitudes of cause and effect would probably be puzzling to the average manager. By exceeding the recommendation with 12% the expected increase in F was said to be 45%. The phenomenon, which is due to an exponential relation between catch an effort (as in the catch predictions of the former chapter) was explained in the working group report of 1977 (CM: F8, 1977) but the explanation was apparently not given to the managers.

The lower TAC and the increase in mesh size were suggested because they were said to lead to a more rational exploitation resulting in a higher long-term yield. There was no sign of danger to the stocks, which was confirmed by the Working group reports. For example the 1977 working group report stated that: "There seems to be no urgent need to reduce exploitation rates drastically, since there is above average recruitment and fishing mortalities are not excessively high." The aim was therefore to reduce F to about half the current level because it was *rational* to do so.

What about the policy of reducing F by 10% per year? There was little explanation of this policy. However, in the introduction of the 1977 report, it was said that the achievement of the objectives may "…need to be implemented in the long term since the *measures* required would in the short term be too drastic" (emphasis added). This, I think would indicate pragmatic political concerns to the manager.

In the 1979 working group report, which was the background of the 1979 ACFM report it was stated that "...,if the 1979 TAC is adhered to, it could be argued that the 1980 TAC should be increased considerably in order to *prevent the biomass from building up rapidly*" (CM G:7, 1979, emphasis added). Why should the scientists worry - was the increase in biomass not what they wanted in the first place?

The scientists generally wanted the F reduced towards F_{01}, (which had replaced F_{max} as the rational reference point in the meantime). However, a biologist (working in the demersal fish committee) Niels Daan had criticised the F_{01} concept, since it was based on the assumption of constant recruitment, which evidently was questionable. Daan argued that the expected gains in yield per recruit by applying F_{01} for the demersal stocks of the North Sea were small compared to the effects of the long term variations in recruitment, which could somehow be related to the general "expansion" of the North Sea fisheries (CM: F:7, 1976). Therefore Daan suggested that these demersal stocks should be reclassified from as being "fully exploited" (in stead of "overexploited").

To the manager it would seem that the stepwise reduction in F was due to pragmatic reasons but as shown there was perhaps another reason: You did not know what would happen if the F_{01} was achieved - maybe is was not desirable. I suggest the latter was an important reason for the suggested stepwise reductions, but the managers were, it seems, not informed of these concerns until later.[11]

The early 1980s: Flexibility and uncertainty

The managers had asked for more flexible recommendations and they got them from 1980 and on in the form of catch options (as described in the former chapter). The catch option figure included a graph of the resultant SSB for the next year, which ACFM said was "...perhaps more revealing than the yields curves…". ACFM warned the managers that "…the chances of getting above average recruitment are likely to be seriously diminished once the spawning stock falls below a certain level". The importance of a long-term policy in order to secure optimum yields was stressed and ACFM warned of "short-term expedients adopted to meet current economic and political problems".

ACFM noted that it had been its policy to reduce F by 10% per year - but little if anything at all had been achieved. 3 main reasons for this were mentioned: The TACs had been exceeded due to ineffective enforcement, there were large discards and the TAC recommendations had "in too many cases" been "highly optimistic". ACFM therefore recommended larger annual reductions than before. In the introduction of the report it was noted that large discards not only reduced the potential yield but also the accuracy of assessments since good data on discards were expensive to obtain.

In late 1979 the minimum legal mesh size was increased from 75mm to 80mm (CRR 93, 1979). However, ACFM stated that a 5mm increase was not expected to have "any appreciable effect". An increase to 90mm as had been advocated for several years would nevertheless be expected to reduce the discard problem to a rather low level. ACFM found it "disappointing" that little progress on this issue had been made in spite of the stressed advantages (CRR 102, 1980: 16-18).

The question of how to deal with discards was important for the assessments. In the early 1980s the working group struggled to revise an inconsistent database which suffered from problems related to the discards (CM G:8, 1980; CM G3, 1981). In its 1981 report ACFM had recalculated the assessment made by the working group.

[11] The first incident kown to me of ACFM noting that the stepwise reduction towards F_{max} also was due to biological concerns of multispecies interactions is in the 2nd dialogue meeting in October 1980 (CRR 106: 53).

The reasons were that ACFM found it better to leave the discards out of the assessment since inclusion of weak discard estimates would add considerable variance to the result. Further, ACFM criticised the working groups use of new assessment methods (CRR 114, 1981). In the 1982 report ACFM explained that it, at the May meeting, had been unable to assess the stock due to uncertainty of the recruitment level. The advice was therefore postponed to the November meeting, where ACFM, in spite of new data, stated that it had been unable to provide a sufficiently precise assessment to allow a forecast. Instead a TAC within the range of the previous 3 years was recommended (CRR 119, 1982). The 1981 and 1982 reports contained long and detailed technical explanations of the difficulties ACFM was facing in the assessments for these years. A graph of the stock history in terms of SSB and F was, for the first time, included in the 1982 report.

In the 1983 report it was said that the SSB had recovered somewhat from its lowest level in 1978, but that it was expected to decline if F was not reduced (CRR 128, 1983). The F level was said to have been at the 1982 level for 10 years. The year after, in the 1984 report, F was believed "to have generally increased in the last 20 years".

The 1980 ACFM report can in some ways be seen as a turning point. The catch options granted the managers the freedom that they wanted. But the freedom had a price: The managers were now directly in charge of the SSB. This was explicated by the curve of expected SSB related to a certain catch in the catch forecast. The SSB had only been referred to occasionally and indirectly in the previous reports - no estimates had been shown. The managers were thus provided more information as their biological responsibility became, so to speak, more direct. In this way the managers had to become familiar with the stock biology.

Furthermore, the biologists now shared the uncertainty of the assessments with the managers and detailed technical explanations of it were given. It became obvious that the assessment quality also was a management issue, since the assessment suffered from uncertainties related to the serious discard problem.

The biologists were quite frank about their views on the management: It was disappointing. The rational aims had not been achieved or even approached. Therefore they were obviously worried when they were asked to leave more freedom and responsibility to the managers.

1985-1989: Development of the first crisis

In The 1985 report the SSB was "…estimated to be at its lowest level ever and less than the annual catch with the prospect of declining further in the immediate future". Further, the SSB level was much lower than previous recorded lows in 1963 and 1977-79. ACFM stated that it was "unable to judge" whether the low SSB would "affect recruitment" but found that further declines "…must be a biologically unacceptable risk" (CRR 137, 1985). The appropriate mesh size was said to be "well in excess" of 80mm or 90mm but enforcement of a 90mm mesh would only result in a small increase in SSB.

In the 1986 report it was stated that continued fishing by the current F would lead to a further decline in SSB. Even if the fishery was closed, the SSB would not increase to the level at which "consistently good recruitment" had been observed (150.000t - the later MBAL value) (CRR 146, 1986).

In 1987 the stock prognosis had improved somewhat, since the 1985 year-class had been revised up by 40%. It was stressed that the relatively strong 1985 year class had to be protected until it would contribute to the SSB. The report recommended TACs corresponding to a 30% reduction in F. It was explained that accurate forecasting was depending on estimation of the recruiting year class since the heavy exploitation resulted in the bulk of the stock consisting of a few of the youngest year-classes. Further, there were neither adequate data on the by-catches of the reduction fishery nor on the discards. The exploitation level and the data did therefore not allow accurate forecasts.

A long section was dedicated to the problem of managing a mixed fishery by catch quotas. The cod stock was the weakest of the roundfish stocks and it required the greatest reduction in F. The problem was that when the cod quota was taken the fishery would continue to fish for haddock and whiting. The cod would then have to be discarded. The problem was described as an enforcement and management problem - to be solved by the managers (CRR 146, 1986).

In 1988 (CRR 161, 1988) ACFM warned about the high exploitation level. It was stated that the stock and survival was so low that the recruitment in most years would be insufficient to maintain the stock. The SSB was at a new historic low (95.000t). The problems of the mixed fishery was noted but, as before, left to the managers. The mesh-size was to be increased to 90mm from the first of January 1989 but ACFM recommended that the mesh-size should be increased further to 120mm.

The effects of these changes were calculated in single species models, but it was admitted that the calculations were in conflict with the results from multi-species models, which included predation effects. The single species models predicted serious short-term losses in yields, that would turn into gains in the long term - except for whiting, for which the catches would almost disappear in the short term and for which there would also be a loss in the long term. In the multi-species models the gains were less and sometimes negative. However, the mesh-size increase was believed to assist the recovery of the SSB - but quite modestly so in the short term. A rapid SSB recovery was dependent on a major reduction in F.

In the 1989 report the warnings of the high exploitation and the historic lows in low SSBs of the cod stock were repeated. Moreover, the current prospects of the *haddock* stock were said to be "very disturbing". An extensive section dealt with the problems of regulating F in a mixed fishery. ACFM concluded that:

Because of the problem indicated above it can be understood that implementation of TACs on the North Sea demersal fisheries has not resulted in the required reduction in fishing mortality. At present it is difficult to define practical methods for improving the situation. Ideally, scientist would like to adopt *direct effort regulation as the major conservation measure*, perhaps using TACs as a means of strengthening this approach. *However, it seems unlikely that such a procedure can be adopted under the current Common Fisheries Policy*, although it could be adopted at national level (CRR 168, 1989).

I have added the emphasis here. I want you to notice these two phrases since they indicate a very central theme (as it later will be apparent).

Quotas for the different species fished together should ideally be internally consistent, meaning that the quotas should be exhausted at the same time. To cope with the problem required knowledge in order to recommend proper "packages" of quotas. This knowledge was, however, not available. Further, the national allocation of quotas did not amend the problem. The strategy was therefore to continue scientific effort to recommend internally consistent quotas and it was stressed that this strategy required that the national and sectoral allocation of the quotas were adequate. However, in consequence of the mixed fishery problem, ACFM believed that the TACs only would constrain the landings and not the catch and that SSB therefore would continue its decline.

Another section was dedicated to the question whether to increase the mesh-size to 120mm. Again there were inconsistencies between results of single- and multi-species models. The gains in yields and biomass from the former were reduced or in several cases reversed in the latter. The knowledge base for such a change was not adequate:

ACFM is of that opinion that, although the idea of fishing for cod with 120 mm mesh initially appeared attractive, the problems revealed in trying to estimate the effect of such a measure are to great to overcome in the short term and that the results currently available are not sufficiently well-based to serve as the basis for regulations (CRR 168, 1989).

ACFM warned continuously of low SSBs and high exploitation levels. The SSBs reached succeedingly new "historic lows" and there were concerns for the possible effects with respect to the recruitment. The mixed fishery problem resulted in an increased scepticism of the adequacy of the TAC system and the limits in knowledge were a constraint to fine tune quotas in order to make them "internally consistent".

Mesh-size increases were initially recommended to rebuild the stocks but the complexity and uncertainty of the possible effects increased due to concerns of multi-species interactions. There were conflicts between different model approaches: what was gained in realism by more complex models was perhaps lost in clarity of model outputs. Since results of the models were in conflict, there was no longer any basis for recommending changes in mesh-sizes.

1981 was "the year of the stomach". More than 55.000 fish stomachs were analysed 1981 and provided (together with stomach samples from other sampling programmes later in the 1980s) data for multi-species modelling. Results of the multi-species models probably had a considerable impact on the general perception of the fisheries management. I will return to this in chapter 4.3. Although the results of the multi-species models had quite dramatic implications for other aspects of fishery management, they actually had quite limited effect on the short-term-forecasts (CM 1986/Assess:9).

1990-1995: The first crisis

In the 1990 ACFM report, the problem of the high level of exploitation and low SSB was repeated. The exploitation level was again noted to give assessment problems because of the truncation of the age structure of the stock. Further, it was believed that mis- and non-reporting especially of cod had been taken place for many years. A detailed explanation of the inefficiency of the TAC system was provided (as noted in chapter 2). TACs were intended to limit catches to be smaller than the catching capability. However, only official landings were constrained. The TACs did not constrain mis- and under-reporting and discards. The recommendation of ACFM consequently undertook a dramatic and qualitative change:

Given this state of affairs, ACFM feels that any TAC which it recommends would not, of itself, produce the required reduction in fishing mortality. ACFM has, therefore, refrained from making any such proposals for North Sea roundfish stocks on TACs intended to reduce fishing mortality, although option tables are presented to allow management bodies to assess the consequences and implications of different TAC levels assuming that these TACs will be effective. ACFM stresses that unless fishing effort is also controlled in an appropriate manner, it is extremely unlikely that fishing mortality will be reduced (CRR 173, 1990).

Specifically ACFM advised all effort on roundfish fishery (except for the fishery directed against saithe, which to a reasonable extent could be seen as a separate fishery) to be reduced to 70% of the contemporary level. This could for example be achieved by constraining the number of fishing days to the same proportion.

Further ACFM responded to a request from Denmark and Norway to evaluate the impacts of different whiting stock levels on other stocks. It was stated that knowledge necessary for quantification of the effects was not available, but that a reduced whiting stock would have a positive effect on other stocks (especially haddock and herring). There was however no known way of fishing selectively on whiting (CRR 173, 1990).

In 1991 ACFM repeated and confirmed the depleted state of the cod and haddock stocks. The problems of TAC regulation were briefly described (as in the 1990 report). The managers had implemented an 8 days consecutive tie up rule for the roundfish fishery. ACFM, however, noted that this rule would have much less effect than intended since the vessels would spend some days in port anyway, and since different planning of fishing trips could reduce the effect. Moreover, the managers had given some fishers the alternative of unrestricted fishing if a minimum mesh size of 110mm was used. It was nevertheless stressed by ACFM that there was no equivalence between such technical measures and effort reduction with respect to reducing F in the short term (CRR 179, 1991).

The 1992 report repetitively reiterated the need of an effort regulation. The situation of the co stock was termed "extremely critical". It was stated that "Seen in isolation the fishing mortality on cod should be reduced to zero". Recovery of the cod would require "…at minimum a marked and sustained reduction of effort or even a closure of the fishery". As it had already been stressed for some years, there were concerns that the egg production (as a consequence of the low SSB) was so low that a high survival rate was required to produce an average recruitment. A risk analysis showed that given the current F there was 90% probability that SSB would fall below the SSB level of 1991 (56.000), which was about one third of "the lowest desirable level" of 150.000t, from which ACFM previously had observed recovery (CRR 193, 1992).

The 1993 and 1994 ACFM reports were quite similar and the messages resembled those of the 1991 and 1992 reports. ACFM confirmed the critical state of the cod stock and reiterated that an effective effort reduction by at least 30% was needed. Consequently ACFM continued the policy of abstaining from giving TAC recommendations. It was mentioned that data were deteriorating (in spite of agreement between TACs and landings), which would imply problems for the assessments (CRR 196, 1993, CRR 210, 1994).

The 1995 report was the last report where no TAC recommendation was given. The stock was still "outside safe biological limits" but the SSB was forecasted to increase in 1996 because of maturation of the relatively strong 1993 year class - even if F was not reduced. The recommendation was to reduce effort by at least 20% (CRR 214, 1995).

The SSB had continued its decline and the state of the cod (and haddock) stock was becoming critical. ACFM provided an explanation for the inefficiency of the TACs in regulating mortality that was of a more general nature than before. Further, there were increasing concerns with respect to the quality of data because of discards and illegal landings. Direct effort regulations were now seen as the only effective medicine and consequently ACFM abstained from giving recommendations for TACs.

Unfortunately, the management bodies were unable to provide significant doses of that medicine. The managers were still provided with catch options and used them as a basis for deciding TACs. The 1990 advice represented a dramatic change in the recommendation convention but "business as usual" was generally continued with respect to management.

1996-2003: Recovery and new crisis

In the 1996 ACFM report, the SSB was estimated to be 100.000t, which was said to be close to the historical low level. The stock was still "outside safe biological limits" and "well below a level where there is evidence that there has been impaired recruitment". It was noted that recent analyses had suggested that the stock may collapse if sustained F was higher than 0.75 (F had been > 0.75 since 1980 according to the same report). The recommendation was to reduce F by at least 20% (to 0.65) and a table showed a catch that corresponded to that option. In that case SSB was forecasted to increase to 142.000 (close to the MBAL value) by the end of 1997. However, ACFM noted that the required decrease in F only could be achieved by a reduction in effort in the roundfish fisheries (CRR 221, 1996). This comment had been repeated since the 1990 report and was routinely repeated in *all subsequent reports* (CRR 221, 1996).

According to the 1997 report the stock was "close to or outside safe biological limits". Further it was noted, that the SSB was expected to increase into safe biological limits if the current fishing mortality was maintained. The fishing mortality was believed to have dropped to 0.63 in 1996. ACFM recommended that the fishing mortality should not exceed the 1996 level so SSB could be rebuilt to safe levels. The landing corresponding to that F was given in the text (CRR 223, 1997).

In the 1998 report, ACFM considered the stock to be "outside safe biological limits". For the first time "precautionary reference points" were presented in the report. F (= 0.67) was slightly higher than the F_{pa} (= 0.65) and SSB (=136.000t) was lower than the B_{pa} (=150.000t). The recommendation was to reduce F to 0.60 ($<F_{pa}$) in order to bring the SSB above the B_{pa} in 1999. The expected landing corresponding to the F = 0.60 was presented in the text. (CRR 229, 1998).

In the 1999 the stock was still considered to be "outside safe biological limits". The F estimate for 1998 was below F_{pa} but ACFM recommended that F in 2000 should be less than 0.55 - corresponding to landings of less than 92.300t. ACFM noted that the 1997 and 1998 year classes respectively were the poorest and second poorest on record. It was therefore needed to reduce F further "to increase or maintain spawning stock biomass above B_{pa}".

In a section entitled "Relevant factors to be considered in management" ACFM noted that the 1997 and 1998 assessments presently were thought to have overestimated SSB and underestimated F. Further, ACFM wrote:

The same analytical formulation was used in the 1999 assessment, but the likelihood that F is underestimated cannot be evaluated at this time (CRR 236, 1999).

The continued recovery of the stock was depending on the contribution of the relatively strong 1996 year class to the SSB. The concern was that the growth rate of this year class apparently was lower than usual. Further, ACFM noted that substantial underreporting of cod had taken place in 1998 (CRR 236, 1999).

In its 2000 report (CRR 242, 2000) ACFM estimated the stock to have been below B_{pa} since 1984 and that it currently was under B_{lim} (the historical limit reference point of 70.000t). The SSB was now in a region where "the risk of collapse is high". The recommendation was to reduce the fishery to the "lowest possible level". A rebuilding plan should be implemented in order to SSB to the B_{pa} level. It was stressed that TAC reductions were not sufficient. Directed fishing, misreporting and discarding should be avoided and by-catches should be reduced to the lowest possible level. A new plot type that showed equilibrium SSB as a function of Fs indicated that the stock would collapse at current fishery mortality. The 1996 year class seemed to have been heavily exploited and to have little potential to contribute to the SSB. It was noted that the growth rate of cod for unknown reasons had declined in recent years. This would delay a recovery of SSB and make the stock more vulnerable to high exploitation levels. Further it was repeated that substantial under-reporting of cod landings occurred in 1998 but ACFM continued that "there are no reasons to suspect substantial under-reporting in 1999 or 2000." (CRR 242, 2000).

The 1997, 1998 and 1999 assessment had overestimated SSB and underestimated F. This was said to be because "inconsistencies in the commercial effort data". In the 2000 assessment the commercial CPUE data of the Scottish fleets were consequently omitted, since these were believed to be the most problematic. It was mentioned that the difference in signals between survey data and CPUE data affected the assessment of some Canadian cod stocks resulting in over optimistic management.[12]

In the report of 2001 (CRR 246, 2001) the SSB was said to be at a new historic low. F had been at historically high levels and above F_{pa} since the early 1980s. ICES recommended implementation of a recovery plan to rebuild the SSB. Fishing mortality should be reduced to the lowest possible level. The inefficiency of TACs was explicated:

[12] ACFM (most likely) referred to the now classical case of the over assessment of the cod stocks of Newfoundland, which will be discussed later.

"ICES has repeatedly stated that for various reasons, TACs alone are not effective in regulating fishing mortality"(CRR 246, 2001: 230).

Rebuilding of SSB could be carried out by reducing F or/and by changing the exploitation pattern (30mm increase in mesh-size was said to be equivalent of a 30% reduction in effort)[13].

It was said that assessments prior to 2000 consistently had underestimated F and overestimated SSB. Further, growth rates in 2000 had been lower than assumed previously. The CPUE data were now completely excluded from the assessment and the "historical consistency" had thus improved. A more specific explanation of the bias resulting from the CPUE data was presented and again the case of the Canadian over assessment of cod stocks was referred to. A variety of different assessment methods had been used and had given comparable results, which increased the confidence in the recent assessment.

A long section dealt with information from a quite extensive examination of fishermens views on the current assessment. ACFM stated that the views were quite diverse but concluded that:

There is general agreement on the poor state of the cod stock, but the stock was considered in a better condition in the Northern North Sea than further south (CRR 246: 224).

I will not summarise much of the 2002 ACFM report since some elements already have been presented in the introduction of this work. Note that this assessment serves another role than the previous assessments, since it is taken to be the reference for the currently best justified beliefs with respect to the stock - it is the assessments that is assessing its predecessors. Further, the role of this assessment is different since it constitutes the explanandum to which the previous assessments are elements of the explanans.

The stock situation is now perceived to be worse than at the 2001 assessment, which over-estimated the SSB for 2001 by around 50%. In the 2002 assessment CPUEs were also excluded (for the reasons described in the 2000 and 2001 reports). It is suspected that the landings in 2001 were seriously underreported. The assessment was reviewed by 3 independent scientist and a by a public review with participants

[13] From personal communication with Roger Larsen (2002) who is an expert in selectivity of trawls, I find that these results are probably only "theoretical". Fishermen very generally know ways to counteract the effects of mesh-size increases (e.g. changes in twine types or towing speed).

from organisations of fishermen. The review claimed that the assessment was in agreement with the proper standards and the fishermen broadly shared the perception of the stock development. However, the fishermen of the northern North Sea tended to see more fish whereas it was opposite in the south (ACFM 2002).

The stock was said to be "outside biological limits" and the recommendations were to reduce F. however, there were some signs of optimism or relief. The stock was approaching the MBAL.

In 1997 the stock was said to be "close to or *outside* safe biological limits" and the SSB was expected to *increase into* safe biological limits if the current fishing mortality was maintained. In the appendix (table 3.5.2.4) the stock parameters of the assessments are listed and the SSB estimate for 1997 report was 160.400t. Recall that the MBAL was 150.000t. I think that it is fair to say that the above statements by ACFM then are at least "grammatically incorrect". This could therefore indicate the ambition of the scientists to protect and rebuild the stock.

In 1999, things were a bit delicate but not looking really bad. You had two really poor year classes coming in, but on the other hand the strong 1996 year class was about to enter the spawning stock. F was lower than F_{pa}, but it was wise to lower it further, because of the poor incoming year classes and because the growth of the 1996 class seemed to be unusually slow. ACFM was worried about biases and the managers were warned of that. It was noted that the two last years had been too optimistically assessed. The 1999 assessment was done in the same way and ACFM was unable to judge if it was biased too. In 2000 ACFM decided that it was - and very much so. The 2000 assessment marks the onset of crisis with ACFM advising the lowest possible catch and recovery plans. ACFM explained that the over-assessments were due to the use of the CPUE data and made its adjustments accordingly. However, it later turned out that the 2000 and the 2001 assessments were also biased, which is where we are now.

It was regularly repeated that the TACs alone would not suffice to accomplish the reduction in F. However, the practice of providing TAC advice was re-established. A certain "catch" (1996) or "landing" (1997, 1998, 1999) corresponded to a certain reduction in F. There is therefore an inherent contradiction in the reports from 1996 to

1999. On the one hand is it stated that TACs cannot regulate F and on the other hand the advice is given exactly as if they can.

As the terms have generally been used by ACFM and the working group, discards and illegal landings are considered to be included in "catch" but not in "landings". If the regulatory system was ineffective, a certain catch would still correspond to a certain F. If a biologist was sceptical towards the TAC system, wouldn't he then use the term "catch" instead of "landing"? If the advisors are assumed to be consistent with the terminology, this change perhaps indicates a slightly renewed belief in the efficiency of TACs in regulating F. At least the change in words would seem to underline a step back towards the practice of the "classical" TAC recommendations.

A brief history of the advices

A general and very obvious observation is that the complexity of the advice has increased tremendously from the mid 1970s and to today. An indication of this is the increase in the number of pages in the ACFM reports (or Liason Committe reports until 1978. In the mid 1970s the number of pages was around 100 and there was a quite linear increase until the 2000 report, which contained about 900 pages (R. Poulsen, 2002: 91-92).[14]

The increase in volume indicates an increase in the complexity of the scientific knowledge in fisheries biology, which mainly resulted from development of the issues of species interactions, technical interactions and technical measures. The increase in the ACFM reports showed that there was a dramatic increase in the amount of information considered important to the managers. The development was from a brief recommendation of 2 numerical options (corresponding to the "rational" objective and corresponding to a minor, pragmatic reduction of F) to a complex description of the situation and an array of possible management options. The managers achieved more flexible advice at the price of getting more direct biological responsibility and at the cost of having to become more familiar with biological knowledge - the managers had to become familiar with the concepts of F and SSB.

Uncertainties of the assessment turned out to be related to the management - they were therefore in part the managers' responsibility. The uncertainties of the

[14] I expect that the increase in the volume of the working group reports was similar - or perhaps even more dramatic.

advice were largely shared with the managers, often in quite technical and detailed sections. ACFM explained that discards were not included in the assessment because they would to give rise to reduced precision (which was traded off with accuracy).

I have indicated that *everything* was not always told and that there may have been some tactics involved in this. The biologist generally wanted to reduce the exploitation level, and that *may* have been a reason why they did not, initially, mention that they did not know what would happen if F was reduced to F_{01}. Further they did not say that the SSB, according to the 1997 estimate, had reached the MBAL level (in 1997), although this later turned out to be wrong.

The recommendation to reduce F was not followed

From the quantitative analysis of the former chapter you may conclude that the managers trusted the maximum recommendation for TACs but they did not follow the general recommendation by the scientist: To reduce the effort and the fishing mortality. The main theme of the history is one of the advisors trying to convince managers to reduce F. Persuasion was first tried by *tempting* (rational exploitation would increase yields dramatically), then by tempting and *warning* (the SSB was low - SSBs and yields could be higher by improving the exploitation pattern) and finally by *alarming* (the risk of stock collapse). Warnings were increasingly loud but the necessary steps to reduce F were not taken (not that the managers did not *want* to, of course).

Technical regulations

For many years the scientists recommended increases in mesh-sizes, in the benefit of which they initially had strong believes. The changes were, however, not adopted until many years later, and the changes adopted were often much smaller than those recommended (Holden, 1994). Ironically, the implementation of changes largely happened after the scientists began to loose faith in mesh-size regulations. Results that included multi-species considerations were somewhat ambiguous but indicated that the possible gains were much less than was previously assumed. Moreover, mesh-changes were later said to be of less immediate importance. What was needed in the time of stock crisis was immediate response.

It was known that the TACs did not work

A key question to be examined in this historical section was the question whether it was known that the TACs were not effective in regulating Fs. The first warning that the TACs were not efficient was presented in the 1987 ACFM report. The explanation was given in terms of the mixed fisheries problem. This explanation was repeated in 1988 and 1989. In 1990 a more general explanation was given. The warnings were now really loud and often repeated - and they were followed by the statement of ACFM abstaining from giving TAC recommendations from 1990 to 1995. The explanation given in the 1990 report was often referred to and it became a routine to warn about the incapability of TACs to reduce F.

We therefore must conclude that the deficiencies of the TAC system were known and that message was strongly and recurrently delivered to the managers. The problem must have been very clear to the managers - at least since 1990.

Effort regulations *were* implemented – but insufficiently so

As mentioned, effort limits in form of allowable days at sea *were* introduced – although at an insufficient scale. Holden (1994, chapter 6) describes and explains the history of “tie ups” in the yearly 1990s. In 1990 the UK - the major nation of demersal fisheries in the North Sea – forced its fishing vessels to stay 92 days in port at the instigation of the Commission of the EC. There were no restrictions on the distribution of the tie up days and the regulation probably had no effect – it met little resistance by the industry.

In 1991 the Commission of the EC proposed a tie up limit of 200 days, which was “bitterly opposed” by the industry. As a result the Council reduced the period to 135 days and similarly reduced an alternative regulation of ten days per month to 8 consecutive days a month. This regulation was also adopted for 1992 but there were exemptions for both years. In 1991 you had no day limits if your mesh-size was at least 110mm. In 1992 you had a tie up of 67 days if the mesh-size was 110mm or none at all if it was 120mm. As mentioned, ACFM warned in 1991 that the 8 days rule would have little effect since vessels do stay some time in ports anyway. Moreover, ACFM noted that there was no equivalence between technical measures and effort reduction with respect to reduce F (in the short term). ACFM wanted a general reduction in F - not just to protect juveniles.

The Council did not renew the fishing days system for 1993. According to Holden (1994), the reason was that the UK had argued successfully that its national conservation regulation, which provided for regulation by fishing days limit, obviated the need for Community provisions.

Chapter 4: Science, Management - and Industry

I warned you that the surprise was inevitable. We ended up with one horn of the dilemma: the managers *had* knowledge of the problems of TAC regulation. The next question is then why was this knowledge was not properly used. I have chosen to work with the perspective of knowledge in order to explain the crisis and I have until now focused on the *content* of this knowledge in that it was examined what information was delivered to the managers. In trying to explain the crisis we further need to see how the knowledge was *used* in the management system.

We have already seen one indication of why the recommendation of effort regulation was not followed. The Commissions proposals were in accordance with the scientific recommendations but the proposal were not implemented because of pressure on the Council by the industry. The history of the technical regulations was similar in this respect. The industry seemingly has some power to resist the implementations of scientific recommendations.

The embarrassing limitations of this project will now become clear. My strategy will simply be to clarify these limitations and then to pick a possible route from there. I will thus capitulate to some uncomfortable problems resulting from the complexity of the subject - but it is my hope that I will surrender in the most *useful* way.

Return to the representation of the fishery system in figure 1: The fishery system as consisting of four subsystems. The resource system is subjected to the conditions of nature and the actions of man. Let us say (roughly) that management only can influence the actions of man. Further, nature is not an actor since its effects are not resulting from deliberations (e.g. they are not resulting from actions). We can then reduce the 4 sub-system model to a system of 3 *actors* and say that the actions are the only thing we can do anything about and it therefore is the role of these *actors* in the crisis that it is especially important to explain.

We have, then, three major actors: The managers, the scientists and the industry. Whit respect to the question of the use of knowledge we need to address the question of power, because it takes power to use (implement) knowledge. Obviously the management bodies contain the formal power since the decision of the Council is law. Therefore we need to examine the forces acting on the management system. To put it simple: On one side there is the force from the industry on the other side the force is from science. Or to put is less simple: In order explain the crisis properly we would need to understand all the important interactions between the three actors. Further, we would need to understand the forces *internal* to each actor. For example I have often referred to "the managers". This term is of course covering a lot of complex and interesting relations within CFP (e.g. the Council vs. the Commission) and within the management bodies of Norway.

But I simply can't examine all these relations here. To analyse the influence of the industry on the management decisions properly would require another thesis and anything less than a thesis would probably be superficial and therefore quite useless. Further, this perspective would bring us away from the perspective of scientific knowledge.[15]

What I will do is to continue with the perspective of the use of the scientific knowledge by the managers. The way I want to do it is to examine the developing relation between "science" and "management". And as it turns out this relation will indirectly tell us something about the relation between the industry and the management. The latter relation is, so to speak, reflected in the former and it will therefore not be completely left out.[16]

Three questions

Let me recapitulate some main points so far in order to proceed.

The quantitative analysis showed that recommendations for maximal TACs were followed – but that the catch options they were based on were severely and increasingly biased. The bias was due to two causes: 1) The TACs were unable to regulate the fishing mortality. 2) The stock was severely over assessed since 1995. The qualitative analysis of the recommendations showed that warnings of high Fs and low SSBs were loud and that managers, at least since 1990, were explicitly warned of the problems of the TAC system.

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[15] Note that I don't say that the industry does not use knowledge claims when they put pressure on the management bodies. Basically I think the industry uses two types of arguments. One is from temporary economic considerations (e.g.: "You can't implement that now - we already have serious economic problems"). The other is related to knowledge: "There is no *need* to implement that". The latter strategy is to challenge the scientific claims in saying, "there really *is* enough fish", or that this regulation system "*will not work anyway*". This is therefore also a knowledge perspective and I should therefore really pay attention to it. However I have chosen to focus on the *scientific knowledge*.

[16] It will be apparent that the pressure from the industry is not the only reason why a fishing days regulation system has not been implemented on a sufficient and regular basis.

I think the above naturally leads to the two following questions:

Why was the TAC system not replaced by an effort regulation system?
Why was the stock overestimated?

But, importantly, there is also a third question, which implicitly is connected to the two former questions:

Why was the *maximum* recommendable TAC nearly always chosen?

I will in the following attempt to answer these three questions, which then will conclude what the perspective of knowledge told me (so far) about the crisis.

4.1 TACs vs. effort regulation

From the former chapters it was apparent, that scientists at least since 1990 strongly warned about the TAC system and strongly recommended that it should be supplemented by an effort regulation system. However, the history of these ideas extends further back in reports of dialogue meetings between scientist and managers. These dialogues can shed light on the development of the views among these two parts on the issue.

TAC vs. effort regulation through the history

In 1976, ICES held an ad hoc meeting "on the biological basis for fisheries management" (CRR 62, 1977). At this meeting problems of managing by TACs in a mixed fishery were discussed. It was noted that direct effort control was possible but that appropriate measures of fishing power would be needed. The report continued: "In the meantime it is likely, that the Commissions will continue to ask for advice in terms of TACs" (CRR 62, 1977: 11). This indicates that biologists very early were aware of the problem and that they preferred effort control when possible.

At the 2nd dialogue between scientists and managers in October 1980, a statement from the Ministry of Fisheries of Denmark was very critical of the reliability of TAC advices[17]. The statement raised the question if the data and methods used for stock assessments were sufficiently reliable “to ensure that the estimated catch predictions and hence the TACs based upon thereon will lead to the “agreed” objectives”. It was noted that the data base bas deteriorating because of under – and misreporting since a quota system implicitly invited to cheating. It was asked directly if a TACs policy had “any future at all” and if some other management tool could replace it (CRR 106, 1981).

These points were discussed at the meeting and Scientists had answered that direct effort control would probably be more efficient and easier to enforce. Effort regulation should be aimed at, although there was a long way to go since it was problematic to quantify the effort. This statement was repeated by ACFM at the 3rd dialogue meeting in September 1981 (CRR 106, 1981) and summary of the discussion points was put into the introduction of the 1981 ACFM report (CRR 114, 1981). In

1982, at the 4th Dialogue meeting the chairman of ACFM repeated the merits of direct effort regulation and a statement by the Dutch delegation agreed to the points (CRR 122, 1982).

At the 5th dialogue, held in 1985 the question of effort regulation was a major issue. An American expert, Dr. V.C. Anthony, reviewed the experiences of ICNAF from 1964 to 1973 with combined management by TACs and direct effort regulation. The major problem had been to measure and quantify effort. Anthony maintained that effort regulation (ER) was unlikely to be possible to implement on an international basis. On a national basis it would imply allocation decisions, which were the task of managers. The ICES president and the chairman of ACFM both confirmed the point that effort regulation was not a task for scientists.

A Dutch discussion paper listed pros and cons of a TAC system and a direct effort regulation system (ERS). The effort regulation system was noted to "score better". A weakness of the ERS as compared to TACs was that species could not be managed individually. This point was however refuted, since TACs not in reality could manage species individually. The only advantages of TACs were consequently that they could be directly linked to the assessments and that the system was politically accepted. The benefits of effort regulation were its effectiveness and that discards and mis- or underreporting would be avoided. Therefore, the quality of data for assessments would improve. The technical problem of quantifying effort remained. However, the best solution to the later problem was to get experience with the system. The proper number of licences would become clear in time. Further, the licence system could be initially be supplemented by TACs for the most threatened species. The Netherlands would for these reasons welcome ICES to recommend "effort allowed".

A Norwegian and an EC manager welcomed the ERS but claimed that it could be difficult to implement at an international level. Moreover, a German manager stated that there was no basis for ERS on an international level because of "major political issues" but that it would be appropriate at a national level. As the representatives of ICES, the Norwegian and the EC manager both emphasised that this was not an issue for ICES. A Dutch manager agreed that ICES should not provide

[17] Effort regulation was apparently not (directly) discussed at the first dialogue meeting in May 1980 (CRR 106, 1981).

advice of ER but considered it appropriate for ICES to provide advice on its technical aspects (CRR 139, 1985).

In the 6th dialogue in 1987 the question of management systems was one of three main themes. A Dutch administrator presented a similar but, compared to its predecessor in the 5th dialogue, slightly more detailed analysis of pros and cons of TAC and ERS. He explicitly intended the statement to be provocative and the critique of TACs was quite frank. He said that decision-makers tended to set TACs higher than recommended by scientists in order to meet short-term needs of the industry and that enforcement was difficult with a resulting attitude change from obedience to disobedience among fishermen. Moreover, he noted that ER could be more economically efficient since the same catch could be taken with fewer vessels in a license system.

In an invited statement, an industry representative said that fishermen would have to live with TACs, that effort regulations were important but that technical regulations had not yet been used to the full extent. The statement of an ICES scientist explained the relation between TACs and ER and what data would be required for ER. The paradox was that while an ER system would increase the availability of effort data it could at the same time lead to deterioration of the same data, as it was the case of catch data in the TAC system. A German manager noted that many fishermen and managers were unfamiliar with the ERS and that it in Germany had been given up since it was too administratively complicated. Several speakers noted that TACs and ER should rather be viewed as, complimentary, than mutually exclusive, management means. The 7th, 8th, 9th and the 11th (the latest published) dialogues were not relevant to the ER question. However, a Dutch fishery scientist noted in the discussion of the 7th dialogue (CRR 171, 1989) that effort regulation in form of allowable fishing days had been "surprisingly well accepted by the industry", since it allowed planning of operations. Furthermore, the system would lead to a reduction of fleet size in the long run. The scientist proposed this method as a first step to reduce fishing mortality.

The 10th dialogue in 1995 the fisheries of the Bay of Biscay and the Atlantic waters of the Iberian Peninsula were discussed (CRR 227, 1999). However, a member of the EC commission (EC DG XIV) made a more general statement on "Recent developments in the fishery management policy". He stated:

The lack of connection between the element of annual biological management by stock and the structural element that control capture capacities has lead to the consideration that direct control of fishing effort would better than the TAC system allow for the establishment of the desired link between these two elements. Therefore the Commission sees merits in management in terms of effort and capacity regulation. But it has so far not been politically possible to implement such management schemes (CRR 227, 1999: 20).

Further he presented the following request to the scientists by the Commission:

The Commission wants a direct link between research and the management needs for information, a link that is not always functioning. The best example of this is when the Commission asked the scientist for information on the relation between the characteristics of the fishing vessels and their fishing power. There was very little response from the scientists. That means that EC finds itself unarmed for the coming discussion on effective management through the control of fishing efforts (CRR 227, 1999: 20).

Problems of TAC regulations were early recognised

I have now presented the historic discourse relating to the question of effort management, as it is apparent from the dialogue meetings and I will now comment upon it. I remind of the view of the scientist on the desirability of direct effort control as it was expressed in the ACFM reports since the late 1980s.

The problems of TAC management for a mixed fishery subjected to severe over- capacity were early recognised by both scientists and managers. The statement by the Danish Ministry in second dialogue in 1980 is noteworthy since it turned out to be almost a prediction. The problem of assessments and regulating by TACs were connected to the catch forecasts not being reliable (as in my second chapter). This indicates that at least some the managers were well informed about the strengths and weaknesses of the biological knowledge and the management system.

Technical and political problems

The scientists noted the benefits of direct effort regulation but also stressed that proper quantification of the effort would be difficult. Dutch representatives advocated strongly in favour of the ERS and later claimed to have good experiences with a licensing system. They therefore wanted ICES to study and develop the basis for ER further. Other managers and scientist asserted that ER would not be possible internationally, in part for technical reasons, and in part for "political reasons". ER would be a national issue related to allocation questions and it was consequently not an issue for scientists but for (national) managers. As a result, there was a general agreement on the problems of TACs but not on what to do about them. There were technical challenges to an ER system but science was seemingly, at least initially, not urged to face them. The issue could seem to have been postponed and the responsibility transferred to national managers.

As a national issue the ER made little progress, except in the Netherlands - although UK as mentioned introduced an ineffective fishing days limit in 1990. A reason for this could very well be a version of the notorious "prisoners dilemma". Why should one nation carry out expensive research and implement unpopular restrictive measures when the other nations did not? Only the *other* nations would benefit from the inconvenience of the nation in question. This may have been the national barrier to the ERS. The national barrier was in this way linked to the technical barrier. ICES had not been encouraged to do research on ER since it was a national question.

Unclear division of responsibility

I do not know what communication was carried out between 1989 and 1995 with respect to the ER question - except from what the scientist wrote to managers in the ACFM reports. However, something must have happened in the meantime - probably resulting from the urgency of action needed to regulate F - as stressed by the scientist in the ACFM reports. First, the Commission had not been strongly in favour of introducing ER (internationally). Later it faced "political problems" of implementing it. As mentioned in the former chapter, the Commission proposed drastic measures in form of fishing days for 1991 and 1992, which were later weakened by the Council of Ministers and which it did not renew for 1993.

This shows that ER soon returned to be an international issue - at the responsibility of the CFP (and Norway). The fishing days system was, however, not renewed in 1993 because UK successfully argued that its national conservation legislation obviated the need for Community provisions. "With the argument over "subsidiarity" fresh in its ears, the Commission was doubtless in no mood to argue against the UK" (Holden, 1994: 112). Again, little or nothing happened nationally, but the point is that the responsibility once more became national. Finally ER returned to be a CFP issue, to which the current fishing days limit introduced this year (i.e. in 2003) is a proof.

To recapitulate: The ER was first suggested as an international system. Then it was agreed that it rather was a national issue. Subsequently it was implemented in a (weak) international version, which was not continued because it was argued that it was a national issue. Finally, facing a severe crisis, ER was implemented internationally. This sequence took place in a period of about two decades and obviously identifies a very unclear division of responsibility between the international management body and the national management body. As the Danish poet Storm Petersen said: When two persons shares a responsibility, the result is about 1% for each. An unclear division of responsibility too often means no actual responsibility - and this has played a central role in the development of the crisis. My example is effort regulation and I have argued why ER *is* a very important issue. The important, somewhat complimentary example of Holden is that of the enforcement policy (Holden 1994: 261).[18]

The barrier of the CFP to effort regulation

The CEC in the mid 1990s blamed ICES (or science in general) for not having undertaking research towards ER. This blame was irrespective of that CEC some few years earlier together with other managers found that ER was not an issue for ICES but instead was a management issue related to (national) allocation questions - a point to which ICES had agreed. This is the *technical* side of the problem. Science seemingly acted too slowly when it *was* asked to investigate the question[19]. However, I personally do not doubt that the management bodies would have been able to promote research of this kind if they really stressed its importance but I will comment generally on such scientific reluctance later.

The proper question now is what the *political* problems were in relation to the question of implementing an ERS. The term "political problems" was frequently used

[18] The enforcement question is a somewhat complimentary perspective to that of ER since if the enforcement *was* strong, ER would be less needed - the TAC system would work better. However, I have chosen to work with ER, because I personally do not believe in the strategy of *excessive* enforcement. It is virtually impossible to monitor and enforce all fishing vessels on the vast ocean. It is simply too expensive - the gains cannot cover the costs. Perhaps satellite based monitoring could change this somewhat but anyway - I do not like the signal it transmits to the fishermen (the Big Brother view in Orwells sense). I suggest co-operation (e.g. co-management) as a more probable strategy (compared to supervision).

in the dialogues by different managers with respect to an international ERS but it was newer spelled out what the problems were, which suggests that there was an implicit understanding of their nature. However, whenever there is a problem, the most practical approach is to explain it and try to solve it. The fact that the "political" problem was frequently mentioned but only hinted seems to me to indicate that it was something that may have been perceived as an issue that it was somehow uncomfortable to talk about.

One possible interpretation of the political problems is of course, as mentioned, the extreme resistance that the fishing days system met by the industry. If, counterfactually, the system had been popular it would probably have been renewed in 1993 to be gradually incorporated into the CFP on a more permanent basis.

I think, however, that there is a more fundamental interpretation of the "political" reasons why ER has not been welcomed and developed earlier and more consistently within the CFP.

The view of the Danish Minister

In 2001, the Faroese politician Óli Breckmann asked the contemporary Danish Minister of Food the following question:

Would the minister positively consider a possible invitation from her Faroese college to study the, as a whole, successful Faroese arrangement of fishing days in order to compare it to EUs catastrophic arrangement of fishing quotas (National assembly, 2001 - my translation)**?**

Breckmann argued that the CFP had been very unsuccessful since "all stocks" had been "halved" since its introduction. On the other hand he explained that the introduction of a fishing days system on the Faeroe Islands had been as success since the fishery had been stabilised following the introduction of the system in 1996. Cheating, by-catches and a destructive fishery were now, he said, avoided.

The minister (Ritt Bjerregaard) congratulated the Faeroes with its success but noted that it perhaps was a bit early to draw final conclusions on the Faroese experiences. She continued that what was suitable for the Faeroes not necessarily was the right thing for EU, which had to consider different nations with different fishing

[19] . In September 1996 CEC further requested ICES to investigate the question on how to measure fishing power in order to facilitate direct effort management (CRR 223, 1997: 13).

patterns. She explained that the quotas were an important instrument with respect to the allocation of resources between third countries. Moreover, she said:

Also with respect to the internal distribution of the catch possibilities in EU, the CFP builds on the principle that the member countries are guaranteed fixed quota shares. These are given conditions, that not generally can be replaced by a fishing days system (National assembly, 2001 - my translation).

The minister finally noted that Denmark would support a more selective fishery by further technical regulations.

The minister thus implicitly admitted the problems of the TACs with respect to conservation of the stocks, but explained why an alternative fishing-days system was politically impossible. The quotas were instrumental to the *allocation* of resources - both between third countries (e.g. EU-Norway) and between member countries of EU. These were given (unchangeable) conditions.[20]

The conservation policy of CFP: Relative stability

The above perhaps got us a bit further. The minister did not *mention* problems of resistance to ER by the industry. The resulting interpretation of the "political" problem is therefore in terms of the international resource allocation. At least this was the perception of the minister - and the perception of a social system of those in power of it is pretty much the reality of that system. Further, the explanation, as derived from the minister, was supported by a telephone interview with a senior manager, Ole Poulsen, in the (now) Danish Ministry of Food, Agriculture and Fisheries.[21] But why should a fishing day system be excluded for this reason? Is it really impossible to allocate fishing days in stead of percentages of TACs? Perhaps a little more history could give us further insight in the problem.

The Marathon Negotiation

Holden (1994: chapter 3 and 4) described how the conservation policy of the CFP was build. Let me briefly summarise his analysis of its development. An important catalyst

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[20] ACFM apparently shared this view. In a ACFM meeting in 1997 almost all members considered that TAC was not adequate as the sole method of management, but many believed that TAC advice was still required by the management authorities because they represented a mean of allocation (ICES CM 1997/A:2: 7-8).

[21] This telephone interview was conducted (by me) the 7th of January 2003.

of the CFP was the introduction of EEZs since conservation measures by individual management bodies would make little sense before this arrangement. The CEC presented the Council with a proposal for conservation and management of fisheries resources in 1976. Nevertheless, it took *six years* of tough negotiations before a conservation policy was settled and agreed by the Council of Ministers in January 1983. Basically the problem was that there were not enough resources to meet the combined demand of the member states, which resulted in two main disputes: the dispute about access and the dispute about allocation. The first dispute was mainly between France, that wanted the national exclusive zone to be as small as possible ("fishing up to the beaches"), whereas UK wanted them to be as wide as possible, (UK had the largest EEZ share in EC).

The dispute of allocation was connected to the access dispute, since when the EEZ areas were agreed to be largely common, it became important to secure national fishing possibilities. Quotas and shares of TACs became the instrument of quantifying the fishing possibilities with respect to allocation. Partly because TACs were recommended by ICNAF and NEAFC as conservation measures, and partly because the stocks were scientifically measured in terms of biomass. The TAC system was therefore the natural choice: it was both a conservation measure and the instrument to deal with the problematic allocation question. Clearly stocks fluctuated and for that reason, the Commission proposed that each country should be guaranteed a fixed percentage share of the TAC. This was termed the principle of "relative stability".

The relative stability was calculated in terms of cod equivalents on a basis of historic catches. It was not a straightforward calculation since the sum of percentages in the relative TAC shares, claimed by each country, was much more than 100 (in part the claims were high because of negotiation purposes, from which a step back would be seen as a defeat by the national industry). Important themes in the negotiations were: *which* historic reference period should be used, how special provisions for areas heavily dependent on fishery should be made (the later Hague Preferences), how the former distant water fleets should be compensated for "jurisdictional losses", *tactics* with respect to technical measures and, finally, how the control policy should be designed. Let it suffice here to note that the term "Marathon Negotiation" used by Holden seems to be quite adequate (Holden, 1994: chapter 3).

The House of Cards

Holden likened the above negotiation to a war of attrition. The negotiators were *very* relieved at the agreement in January 1983. But the managers were aware that it was an "uneasy compromise", wherefore the Commission likened it to a house of cards: it would tumble if any of its elements were moved. The strategy of the Commission was consequently to establish the system as the normal, unquestioned routine (Holden, 1994 p58 notes, that precedence is important in the community - often it is the case that initial *ad hoc* solutions quickly become strong routines). The first TAC - for 1983- was finally agreed the 20th December 1983. Since the fishing in that year was almost over, the TAC served no other purpose than the extremely important role of implementing and confirming the principle of relative stability and thus establishing the routine (Holden, 1994: Chapter 4).

From a shaky house of cards to the impossible strength of paradox

According to Holden (1994: 68) the system of TACs and quotas proved to be "considerable more robust", than it was originally thought by its founders, which in addition is confirmed by the history subsequent to Holdens book on the CFP. The robustness - or the inescapability - of the principle of "relative stability" is confirmed by the Danish minister above. Holden described some of the shocks the principle withstood during history. An example was when the quotas were not big enough to cover the special provisions for the areas that were heavily dependent on fishing (the Hague Preferences). Moreover, the crisis in the demersal fishery - in particular related to the fact that scientists strongly recommended the TACs to be replaced or at least supplemented by effort limits, must be considered as important shocks, which the principle survived as well. It was no longer a shaky house of cards it was the core element of both the conservation and the allocation policy - one of the fundamental pillars of the CFP.

Nevertheless, I do not think the strength of "relative stability" was only derived from its iterated precedence, but also from the very reason why its routine was urged in the first place: the uncomfortability of its initial compromise. Its strength is thus derived from reflection on its weakness. This situation can be likened to the tale of Gleipner in the Nordic mythology. Gleipner was the chain by which the asa-gods tied the most feared hound, Fenris. The chain was made in the strongest way the gods could conceive: of paradoxes; the sound of the cats paw and the beard of women

- and until Ragnarok this chain kept the terrible beast at rest. In the same way I think the Ragnarok of renegotiating the allocation of resources in CFP is feared - in so far it needs a recalculation from fixed shares of annual cod equivalents to allowed fishing days of fleet-segments. Some fleets would be hit harder than others by the probable rule of thumb that those already must suffering from over-capacity will suffer the hardest further reductions. This question could be so politically sensitive that it could not be entrusted scientists as a "technical problem".

A confirmation

I had a telephone conversation with Ole Poulsen from the Danish ministry again the 1st of May 2003. I asked him further questions on his view on the problems of introducing ER. He noted, that managers were aware of the problems by regulating by TACs. However, he stressed that ER would *never* replace TACs. It would be an "academic exercise" (to try or take steps towards ER). He confirmed, that the problem was that of international allocation of resources. There would, "technically", be no simple way of transforming TACs into, for example, fishing days. What is the relation between engine power KW and fishing mortality for gill-netters, he asked. Facing the technical ambiguities member countries would be strongly opposed to set their share from the "relative stability" at stake (Poulsen, 2003, pers. com).

Can the barriers to ER be specified?

Poulsen explained, that the problem is basically a sensitive political problem of allocation, which interacts with technical problems. The technical problem is one of lottery: Maybe you win maybe you loose; but you just don't gamble with your livelihood.

But we can perhaps specify the problem a little more. Seen from the politicians or managers view, the problem is one of not loosing in the lottery - with respect to the "relative stability". This is a macro-perspective that I have tried to explain by reflecting on the historical development of the conservation and allocation policy of the CFP. But there is another perspective: that of the fisherman. The fisherman faces two problems: that of gambling and that of short-term losses. The gambling perspective is perhaps two-fold for the fisherman. First he faces the problem related to the relative stability: will our nation loose out? Secondly he faces the intra-national gambling perspective: will I, being in this fleet-segment and having this boat loose out? The other problem relates to the ER being an *effective regulation measure*, which must be expected to lead to loss of earnings in the short term.

The view of the fisherman strongly influences the view of the politician; such that the micro and macro-perspectives become connected and are perhaps mutually reinforcing.

4.2 About over estimations

How to approach an understanding of the nature of over assessments? I think there are two main approaches: The internal perspective of the knowledge of science (science according to science) and, on the other hand, an external perspective of the knowledge (for example a sociological approach).

Finlayson (1994) made an important contribution towards an explanation of the now classic case of over assessments of the Cod stocks of the Grand Banks of Canada. His position is that of a strong sociology of knowledge (Holm, 2001: 93-124), from which he opposes a "traditional" approach of explaining over assessments:

"Tradition" holds that the "success" and /or "failure" of stock assessment science is attributable solely to the ability or inability of scientists to objectively and accurately understand, describe and predict the dynamics of external natural reality (Finlayson, 1994: 10).

I do not agree with the strong sociology of knowledge (SSK). For if science, counterfactually, *was* able to predict, understand and describe objectively, I *would* consider that a successful assessment. Further, the position of SSK, as expressed by Finlayson[22], does not allow an explanation of *the stock crisis* but only an explanation of the changed *perceptions* of the stock. I find the latter somewhat bizarre since it implies that there, according to Finlayson, not necessarily *was* a crisis in the stock, which on the other hand must be thought to be the main reason to bother with science and its role in explaining the fisheries crisis in the first place.

I think this point is more than a quarrel of words and abstract theoretical positions because the position of SSK, consequently, excludes the *explanatory relevance* of assessment science as such with respect to the over estimations.[23] Yet,

[22] The ontological / epistemological position of Finalyson is apparent from the following quote ("The Harris report" can be said to be the first report that recognised the stock crisis - a result that has been confirmed by all subsequent assessments):

"Despite what may appear to some readers as a congruence between my claim and those of the Harris Report, there is a powerful difference. Harris (and most other critics of DFO science) assume that knowledge claims about the state of the Northern Cod stocks can be, in principle at least, independent of the social context of the production of those claims. In other words, there is a "truth" that transcends time and place that can be revealed through the proper application of science. (The latter half, I think, does not follow).

The social constructivist perspective, however, holds that knowledge about the natural world must inevitably be constructed within and reflect a specific historical /cultural context. There is nothing in the natural world that uniquely determines the scientist's attribution of meaning to data (Finlayson , 1994: 32).

This is the position I intend to challenge in appendix 9.

[23] It is tempting to add that Finlaysons only points out the "arrogance" of *natural science* - is there a "tribal war" he does not mention?

since the present work is of more practical nature and intention than discussions of abstract positions I will leave the issue here, although I have included a, somewhat informal, challenge to the *position* of Finlayson in an appendix (appendix 9).

On the other hand I do not at all deny the importance of sociological explanations. On the contrary I find that the internal perspective of science and the external perspective of sociology are *complementing* each other towards a fuller understanding. This is a view that recognises interaction between nature, science and sociological forces resulting in knowledge (in a broad sense) - and changes in knowledge. I therefore do recognise the importance of the empirical work of Finlayson - given a slightly different interpretation, and complemented with the perspective of the science itself. I will consequently use an outline of his explanation structure to see if we by it can approach an explanation of the present North Sea cod case.

The explanation by Finlayson

Let me briefly present my reading of the general argument in "Fishing for truth" (Finlayson, 1994). For clarity I will reconstruct the argument as a *modus ponens* structure with two premises.

1 Fisheries science (by DFO, Canada) was subjected to a wide *interpretative flexibility*

2 A range of *social forces* exploited the interpretative flexibility

\ The over assessments are explained as a *social construction*[24]

This is *my* construction of his argument and I have already hinted why Finlayson probably not will agree to this interpretation. I nevertheless find it to be the strongest interpretation and I will use it for now. Further, if I do not make the presented reconstruction, I (strictly) cannot use the framework of Finlayson for my purpose because his position (as explained above) does not allow for an explanation of the

[24] This interpretation makes the term "social construction" somewhat redundant, in so far the term then refers exactly to the presented argument structure (e.g. the over assessments were explained by interpretative flexibility subjected to exploitation of certain social forces). What is meant by "social construction" by Finlayson is a bit unclear to me.

stock crisis. Finlayson, is by his SSK[25] position forced to adopt another explanandum: the changed *perception* of the stock, which to his position not, at least not necessarily, is related to changes in the stock itself.

Interpretative flexibility

The concept of "interpretative flexibility" plays a central role. Finlayson offers the following definition:

> **From the social constructivist perspective, the possibility of reading different but, *a priori*, equally plausible conclusions into a single data set is called "interpretative flexibility"** (Finlayson, 1994: 33).

Let it suffice for now to say that the retrospective pattern in F and SSBs (as shown in chapter 2 for the North Sea cod stock) is a manifestation of interpretative flexibility. The data for, say, 1999 have not changed very much (if at all) but the interpretation with respect to the stock condition in that year certainly has. Later in this chapter I will address origins of interpretative flexibility in addressing assessment methodology and data limitations.

If the account of an explanation as presented in my introduction is accepted something is explained to the degree that you see that it was likely (which can be seen as the main *virtue* of explanations). The interpretative flexibility alone does not make an over assessment more likely than an under assessment. It is therefore not sufficient to establish interpretative flexibility in order to explain the over assessment. The flexibility in itself could just as well have resulted in under assessment.

The main themes and *social forces*:

Institutional m*arriage* of science and politics is the claimed "backbone" Finlaysons work. Science became embedded in the state, resulting in tension of two different types of rationality - a "bureaucratic, political" rationality vs. a "scientific" rationality resulting in the social dynamics he describes (Finlayson, 1994: 2). The *driving* social force was the c*ommitment* of science to a management success after introduction EEZs (which were admitted for scientific/management reasons). The form the commitment was given was scientific *optimism*:

[25] "Strong" Sociology of Science. A description of SSK positions is given in Holm (2001: 93-124).

In this case, the commitment to the idea of a strongly rebuilding Northern Cod stock was so powerful that it can be shown to have been read back into ambiguous data through analytical models built upon necessary but hypothetical assumptions about population and ecosystem dynamics (Finlayson, 1994: 13).

This is the main social force that, in exploiting the interpretative flexibility, made the *over assessments* likely.

The interpretative flexibility (IF) is established through a critical view on the assessment and its methodology (Finlayson chapter 4). Certain social forces affect the IF: The incentive structure within the science department led to a knowledge production, which was inefficient with respect to its mandate (e.g. to assess the stock). Careers and recognition depend on *publications* in major scientific journals. This in turn tended to lead (ambitious) scientists to disregard routine assessment work in favour of "interesting" issues that are suitable for "fine" publications. For the same reason *data*, being material for publications, were not shared freely. Application of new methodology by "Young Turks" to the data was thus hindered (Finlayson chapter 5).

Moreover, Finlayson (chapter 6) argues the case of an epistemological incompatibility between inshore-fishermen and science. The overconfident science neglected repeated claims of inshore-fishermen, maintaining that the stock was not increasing since they experienced dwindling catches. Science was on the other hand "epistemologically compatible" with the rationally organised offshore fleet that experienced increasing catch rates - and for which the CPUEs were used to calibrate or "tune" the basic assessment model.[26] [27]

Over assessment of North Sea cod

With Finlaysons explanation in mind I will now return to the present case of the North Sea. I will begin by describing the basics of the assessment methods used for the North Sea cod to get an impression of the "interpretative flexibility" involved. The following is not at all meant to be any comprehensive or sufficient technical

[26] Note that these points perhaps imply a problem for the SSK position of Finlayson. If he a) admits the importance of the knowledge of inshore fishermen and b) recognises the undesirability of an incentive structure, which is a barrier to production of good scientific knowledge, then does he not, implicitly, admit a "truth seeking" property of (scientific) knowledge (e.g. a sort of realism or correspondance theory of truth)? Or, on the other hand: why does he, seemingly, feel himself obligated to *establish* IF?

[27] Another, perhaps stronger, reason that the data of inshore-men were not used were the logistic problems or high costs of proper sampling of this fleet.

description of stock assessment methods (I am no expert). The point is just to give brief, non-mathematical, outline of the methods used, which hopefully will be a sufficient base for some comments. The comments will partly be in terms of how the science appears to a (somewhat) layman and partly in terms of what problems there seems to be according to the science.

The essence of VPA

The basic population model used by ICES is the Virtual Population Analysis model. The VPA model is a "catch at age method" (or a cohort analysis) that is based on data on catches and age data of samples of the catch. The method analyses the historical development of a stock as composite of its age groups or cohorts. The fundamental principle is that the number alive (of a cohort) at the beginning of next year is equal to the number alive at the beginning of this year less the number that is *caught* and less the number that has died from *natural mortality* (Hilborn and Walters, 1992: 364).

Data and parameters

The data on *landings* are central in the VPAs, and the VPA will therefore be sensitive to noise and biases in these. According to Hilborn and Walters (1992: 368) discarding and further fishing induced mortality can be thought of as underreported catch. Here is one source of error: you need the *catches* (or actually: the inflicted mortality on the stock) but what you have got is the landings. Further, you need estimations of the *age distributions* of the catches from age determinations of samples of the catches.

The VPA will also require an estimate of the *natural mortality* (M). This estimate is notoriously difficult to obtain. Moreover, M has been shown to vary between years and between ages. Until 1986 a "traditional" value of M = 0.20 was used for all age classes. (CM 1985/Assess 9: p63 - and earlier WG reports for the North Sea demersal stocks). After 1986, Ms resulting from "Key runs" of the more complex multi-species models were used (CM 1986/Assess 9: 3 - and later WG reports). The mortalities from the multi-species models were considerably higher for ages lower than 4 (but quite similar for older ages). Multi-species interactions had thus proved to impact the mortalities. The effect of the higher Ms was to increase the stock sizes and reduce the Fs of younger fish since if they die faster, there must have been more of them before. Later key runs did not change M very much (for example CM 1987/Assess:15).

In order to transform numbers at age into biomass, some further parameters are needed - e.g. length at age and weight at length keys. These are dependent on the current growth conditions of the cod and can therefore sometimes introduce problems for forward projections of the stock (as mentioned in the ACFM reports of the late 1990s). Further, a maturity ogive is required in order to calculate a spawning stock biomass. It was shown in chapter two that the SSB is quite sensitive to the assumptions of maturity.

VPA procedures

At some point all fish of a cohort are, or can reasonably be assumed to be, dead and the number of fish in the cohort is zero. With this information you have sufficient information to calculate what the stock was in the years before in an iterative backward fashion. However, the problem is that the information on past stock history is less interesting than information regarding the *present* stock size. The present cohorts are not "complete" and there are accordingly too few parameters to calculate the cohort sizes. One way to proceed is by use of statistical methods (generally preferred by North American scientists). Another way (favoured by Europeans), used by the demersal working groups for the North Sea, is to estimate further parameters. For the latter method you would either need some independent estimate of the stock size, or you would need to estimate or assume fishing mortalities on the fished cohorts (e.g. a terminal F assumption) (Hilborn and Walters, 1992: 356-360). In this way the VPAs can be "tuned" by other sources of data e.g. CPUEs or data from surveys in order to narrow down the interpretative flexibility.

With respect to forecasting the stock, assumptions, models and estimations relating to the question of recruitment are needed. This aspect is notoriously difficult and can affect the assessment considerable (see for example CM 1996/Assess:6: 95). Since the recruitment is particularly difficult to estimate, the stock size, as mentioned earlier, is more difficult to assess when heavy exploitation has truncated the age structure towards so its bulk consisting of fewer and young age classes. I will nevertheless return to the tuning aspect of the assessments.

The dilemma of VPA

As indicated above VPA from catch data (and an M estimate) can in itself only provide information on the cohorts no longer present in the fishery. Similarly, the longer a present age class has been in the fishery (the older it is), the more information you have on that age class - and vice versa. This reinforces the difficulties of assessing the heavily exploited stock.

What *kind* of flexibility does VPA in itself exhibit? The fundamental data is the catch data - it is this data you basically try to interpret. There is, *ceteris paribus*, two very different interpretations of a certain catch. On the one hand the catch could be taken from an abundant stock by a modest F, on the other hand, the catch could be taken by a small stock by a high F. This is the dilemma: The VPA, by catch data alone, will not tell you what *the manager* needs to know. For with respect to management the important question is exactly if the stock is in a good condition with a low exploitation rate - or if it is the opposite (or somewhere in between). The role of the tuning is to narrow down the range of possible interpretations (the interpretative flexibility). Consequently, the tuning is crucial. As explained, even a "perfect" set of catch data will be subjected to interpretative flexibility.

XSA tuning

There are many methods used for tuning the VPAs. During the history of stock assessment for the demersal stocks in the North Sea a great number of methods have been developed, used and later refined or replaced. Yet, the change in methodology has seemingly been less frequent in the later years. Since 1994 (CM 1994/Assess: 6) the working group has used the XSA (eXtended Survivor Analysis) method to tune the VPAs.[28] The XSA method is now the standard method used by ICES (Lassen and Medley, 2001: 58).

XSA is a tuning method based on dis-aggregated abundance indexes (CPUEs from commercial fleet and scientific surveys). The population and the abundance index are linked by a catchability parameter, witch is allowed to vary between years and ages. You provide the model with an estimate for the natural mortality and an initial guess on the number of survivors of the oldest cohort present in the stock. By standard VPA stocks sizes are then calculated. From these an initial catchability (and a related modifying) parameter are calculated. Subsequently, the stock estimate is corrected by use of the abundance indexes and averages of stocks estimates are calculated. The latter are used as a new starting point for a VPA and the process is continued iteratively until convergence (sometimes divergence - results) (Lassen and Medley, 2001: 58-59).

Further two supplementary models are used in combination with the XSA. One model is used for "down weighting" of older data - for example with respect to CPUE data. It is known that CPUEs develop through time (technology and learning tend to increase catchability). When using CPUE data the earlier WG-reports usually choose a reference period of for example 10 years in stead of using the whole known time series in order to avoid that the catchability had increased too much in the meantime. Now a model is used to calculate a "tapered" down weighting. The second auxiliary model is that of "regularisation":

The basic idea of regularisation is to assume that the exploitation pattern and the fishing mortality do not change abruptly from one year to the next…In many fisheries it is reasonable to assume that certain variables vary slowly (e.g. the fishing mortality)…The regularisation parameter (1) controls how much variation between years is expected between years in the fishing mortality (Lassen and Medley, 2001: 60-61).

The XSA version of regularisation with respect to F, used by the Working Group, is called "shrinkage to the mean". It means that the last F in the series is not allowed to change very much in that it will be weighted averaged by use of some of the F estimates prior to it.

An impression

I expect that you (at least if you are not a fisheries biologist) would agree that the methodology is quite complex. There are many parameters that need to be estimated and the assessment is sensitive to these and to the quality of the data on landings. My impression is that there has been a process of standardisation of methods during the last two decades. The methods are now integrated in large software packages. This could be interpreted as a way of standardising the subjectivity of scientist. In the early meetings of the working group he would perhaps say: "I think we should limit the CPUE series to the last 10 years". To day it would perhaps be:"I think we should set λ at this value" and so on. There is, perhaps, no essential, practical difference.[29]

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[28] From reading of the Working Group reports between the mid 1970s and until 2001 with respect to the North Sea cod assessment.

[29] There may be sociological aspect of the standardisation of models into large software packages. A biologist working with stock assessment told me the experience of one of his colleges at such assessment meetings. He said that there were roughly two types of scientists present: those who knew

Consider first the choice of the following quote from the description of "methods and software" under the subheading "XSA" from the 1998 report of the working group on the assessment of Demersal Stocks in the North Sea and Skagerrak:

The implementation of various analysis tools is chosen on basis of explorations. The decision on such choices as ages for which catchabilities are assumed dependent on stock size, time taper and fleets to be included is based on inspection of diagnostic output including residuals plots and retrospective analysis for a range of options (CM 1999/ACFM:8: 7).

This explains that assessing fish stocks is a sort of interactive process. You set the model, look at the results and then you change some parameters a little or discard the data on these and these fleets etc. The methodology is therefore dependent on an important element of subjectivity or implicit knowledge of the scientist. But there is also an "objective" element: the use of residual plots, which is a statistical procedure to optimise "goodness of fit" of the models. The standardisation of methodology has probably resulted in more consistent results - but they are not necessary less biased. The flexibility of interpretation has probably not decreased since the exactness of the assessment is probably much more given by data precision than the specific tuning model (Pope, 2003: personal com.).

Interpretative flexibility

The two quotes above illustrate two things: interpretative flexibility and the use of somewhat implicit biological knowledge or experience. Once you are looking for these aspects you can find them in about every stock assessment report. I should not think that this it is controversial at all - it is an integrated part of assessing stocks. Let me however point out again that the range of interpretative flexibility basically is dependent on the data quality. In the extreme: If your landing data are perfect and you catch, say *50%* of the fish in scientific surveys there would be little flexibility (even there theoretically still would be a possibility of a bias). Consequently, there would be little scope for social forces exploiting the flexibility in the sense of Finlayson.

"the program" (trained in mathematics, statistics or computer science) and the more classical biologists who did not know "the program". He indicated that there was a clear difference in epistemological power of these "types" of scientists. The "programmers" would "run" the program, the "biologists" would be silent and perhaps a bit nervous of revealing ignorance of how the programme really worked. Yet, the "biologists" would sometimes sit back with a gut feeling of what was happening in the stock that would not always be expressed by the run of the programme. I do not include this perspective, since I have no "real" information on the issue.

The range of the uncertainty is assessed by for example sensitivity analysis. You change a parameter a little and you monitor how sensitive the assessments are to that parameter. However, this will not necessarily tell you of possible biases. The nature of discards and illegal landings is such that you never really will *know* their impact. It is something you know that you don't know. And again these become issues of subjective judgement (e.g.: should the weak estimates of discards and illegal landings be included or not - a trade off between bias and precision). Let me finally point out that the over assessments themselves confirm these points.

All this goes towards saying that there, unavoidably, *is* a scope for social forces exploiting this flexibility through the subjectivity. If scientist implicitly or unconsciously, agree that things are going great then they certainly could read it into the data and *vice versa*. But reflecting on the methodology of VPA: you cannot grossly overestimate the past eternally - at some point the model *forces* you to revise your perceptions.

Possible interpretations

Consider now the definition of interpretative flexibility. Finlayson defined IF as the possibility of *a priory* equally plausible interpretations of a single data set. As we have seen, the VPA works such that it succeedingly - *a posteriory* - narrows down the interpretative flexibility of a certain historic year, which is another way saying that it was *a priori* flexible. But what does "equally plausible" mean? I think the latter term ignores the action of the implicit knowledge of biologists. Granted, that the IF is large (depending on the data), but the larger it is, the more important is the subjective gut feeling of the scientists. I think, that IF then should perhaps be redefined simply as "possible interpretations". To this range of possible interpretations the biologist could then *qualify*: "But we think the stock is about here and these are the probability distributions, the possible biases are this and this etc." In one word: Transparency in communicating uncertainty. By the way: If *only* dealt with uncertainty in terms of statistics, the treatment of uncertainty may be more transparent to the *scientists* - but, importantly, not necessarily so to *manager* and almost certainly not to the *fisherman*.

The case of North Sea cod assessment: Possible social forces

With the explanation Finlayson in mind, let us turn to the present case. I think there is a main difference. The social force that was *driving* the argument (e.g. exploiting the

IF) was the commitment of science to a management success, which with respect to the over assessment was moulded in optimism.

Independence and political neutrality

ICES is broadly recognised to be an independent scientific institution. It claims that its advices are "unbiased and non-political" (ICES, 2003: www.ices.dk/aboutus/aboutus.asp). This is in strong contrast to the relations between the institutions of science and politics in the example of Finlayson.

Originally ACFM consisted almost solely of *national* members. At the dialogue meeting in May 1980 CEC stated that this composition of ACFM would not enhance its objectivity. On the contrary CEC stated that this could result in ACFMs advices beeing "the lowest common denominator of national agreement" (CRR 106, 1981: 43). The episode is curious since the rule that ACFM members should be chosen from a pool of national nominees was not requested by *ICES* but by *"managers"*.[30] ICES representatives, however, firmly rejected these accusations.

The point of politically non-neutrality of ICES has nevertheless not been mentioned at later dialogue meetings, or so it seems from the Dialogue Reports. Management bodies have repetitively stressed the importance of objectivity of the science. But this was argued from stressing that social and economical concerns should be clearly separated from science. The point is that these warnings were not concerns of the objectivity of ACFM being distorted by *national* politics. Further the warnings were formulated in terms of that ICES should *continue* to separate its science from social and economical considerations (see for example the discussion in 1985 dialogue CRR 139, 1985).

Moreover, in later years, there has seemingly been little difference between the assessments presented by working groups and by ACFM - contrary to especially the early 1980s. When considering sources of biases in the assessment we should then not only take ACFM into account but also consider the working groups who make the initial calculations. The fisheries scientist John Pope says that it is his impression, that the working groups are independent of national interests: "You think as an *ICES* scientist" (Pope, 2003: Pers.com).

[30] According to Poulsen (2002: 37), the rule was a result of Norwegian pressure, which may indicate some strategic role of the rule - at least in the early history of ACFM.

Further ICES scientists are not committed to, for example, CEC. Their contribution is on a voluntary basis (Corten , 1996: 12). It is my impression that there is little reason to suspect that national - or international politics plays any significant role within ICES. This is not to say that the advises are unbiased or objective, which I think they probably never can be. What it says is that the institutional relation is such that you would not expect these kinds of political concerns to be important. Therefore the current case is different from that of Finlayson: The situation of science being embedded in the political structure, creating a commitment of the former to the latter, is not the case.

In Finlaysons argument the commitment of science to politics was expressed as an over-optimistic interpretation of data: the biologist were confident that their F_{01} strategy was paying off. The expectations were high and, so to speak, confirmed by themselves. With respect to the ICES case the fisheries biologist, Corten, on the contrary argues that a main problem within CFP is that ICES scientists have *lost the commitment* to rational objectives (like F_{01}) because they became disappointed by the poor management results of the past two decades (Corten, 1996).

Struggling to be "objective" - and worried

Even not caused by the political structure it could still be the case that that the working group and / or ACFM were optimistic, and that this was causing them to over assess the stock. In fact it could be argued that *any* over assessment exactly *is* characterised by the expression of optimism in interpreting data (i.e. no optimism - no over assessment). I think latter is too simple. There is nothing preventing an optimist from under assessing a stock - the reason being simply that *the scientist generally struggles to interpret data in an objective way*. This is naturally what the scientist sees as his first virtue (and I hope I can include myself). Above, I have just indicated that it is not always *possible*. And sometimes data *are* delusive.

Further, I do not think the scientist *were* optimistic. If you doubt this then return to the recommendations of the years that were over assessed (from 1996 and on). The advisors consistently claimed the stock state was very poor and that reduction in effort was needed to reduce F etc. Yes, the stock *was* seen to recover considerably, the 1996 class was seen to be strong. But that makes my point rather than to refute it: The "optimism of stock recovery" should exactly *not be expected* because of a more general pessimism: Biologists were *worried* and *disappointed* over the lack of implementation of effective management means. And still they believed in a quite rapid recovery - and later in modest recovery or modest decline - where the situation later was "discovered" to be much worse. This rather points towards saying that data let them astray than the contrary.

I therefore see no reason to invoke an explanation similar to that of Finlayson. The premises of the North Sea case are simply different from the over-assessments of the Grand Banks cod stocks in the late 1970s and 1980s.

Steps towards a technical explanation

Can we not explain the over-assessments then? I think we can. When you believe, as I do, that there is a finite number of cod in the sea at any time, then it follows that there *always is a technical explanation* if we fail to assess how many cods there were. Irrespective of whether we will ever approach that explanation or not. In this case I did just not succeed in finding a strong *complementing* social explanation. That is, I do not see that or how the interpretative flexibility was consistently "exploited" by use of the "sociological" approach.

Let us therefore turn towards the scientist own explanations. The two cases of over-assessment (i.e. North Sea cod and Grand Banks cod) are strikingly similar in this respect. The CPUEs were creating biases in the VPA tuning and the growth rate of the cod was at the same time slowing down and recruitment was week. In the case of the Grand Banks, three processes were making the CPUEs “artificially” high. There were technological improvements (also among the inshore fishermen), fast learning of good fishing spots in new areas and increases in cod concentrations (McGuire, 1997). In the North Sea the situation is strikingly similar.

In a joint fisherman-scientist project 336 skippers from across the North Sea were in 2002 asked to note differences between the current and previous years. The study claims to confirm the perception of the scientists[31], although with a higher resolution: The cod was becoming depleted in the southern North Sea (individuals noted to be "small"), whereas the fishermen in the north were experiencing *increased* catch rates of larger cod. (Duncan, 2002).

[31] Perhaps this refers to a similar (somewhat less internationally co-ordinated) study, which is refereed in the 2001 report of ACFM (CRR 246: 223-224). From the collected information ACFM concludes

The situation very much resembles a "classic" case of VPA error:

The place that VPA has most often been found to be wrong is when catchability has increased while the stock was declining. In these cases, the assumption that the terminal F has not been changing in the most recent few years leads to a systematic overestimation of the stock size (Hilborn and Walters, 1992: 364).

Bias, precision and uncertainty

You always expect the catchability to increase (through learning and improved technology). But the problem is naturally particularly important when the catchability increase is higher than you expect. And the "shrinkage" of the XSA does not help. On the contrary the "shrinkage" could actually reinforce the bias of the estimations. If F is generally increasing, the increase will, if shrinkage to the mean of the Fs is applied, only be noticed by a certain time lag.

A third case of over estimations of an important cod stock is that of the Icelandic cod in the late 1990s. From a request by the Icelandic Minister of Fisheries, Rosenberg *et. al* (2002) investigated this case of over-assessment by use of alternative assessment methods and simulation models. They concluded:

Whether this is a price worth paying is a question for later consideration. But we do seem to have confirmed that it was indeed XSA shrinkage parameter settings that led to the apparently ubiquitous retrospective pattern in XSA fits to the Icelandic cod data (Rosenberg *et. al* 2002: 9).

The price mentioned is referring to the "price of precision". The trade off is with respect to XSA shrinkage is, therefore, between bias and precision. "If taken at face value", the results of Rosenberg *et. al* suggested that it would have been better if the shrinkage was left out in that case. A work by Patterson *et. al* was referred to have indicated that uncertainty of the XSA method was "greatly underestimated" for all parameters and that the method appeared to generate "overly high forecasts of stock sizes and allowable catches" (Rosenberg *et. al* 2002: 9).

Was science "overconfident" as Finlayson noted it to be in the Newfoundland case? Perhaps the "signals" were somewhat mixed. In every ACFM report since 1996 ACFM by a "standard phrase" has claimed that: "The biological data available from scientific sources are relatively good" with respect to the North Sea demersal stocks. This is the first thing mentioned in the introduction section named "Data". Later,

that there is a general perception of the poor state of the cod stock but that it is better off in the north than in the south.

however, it was sometimes qualified that under or mis-reporting were suspected to take place. From 1999, the data section also mentioned that the commercial effort data were perhaps not reliable. As mentioned before, the report this year noted the two previous assessments to be over-assessments.

But there were no *representations* of the *possible* uncertainty. There were no confidence intervals (which are perhaps not possible to calculate formally in the used methodology - but anyway) or probability distributions. When I read the report (as a "layman") I have little idea of the actual uncertainty involved. Is it 10% or 50%? It turned out to be up to 100% - sometimes even more. An administrator said that he was really surprised when he read the 2000 report - and I do understand that.

On the ACFM meeting in 1997 it was discussed if science was sufficiently precise to support the TAC system. It was suggested that a realistic level of precision was around 25% whereas many managers perceived the precision of advices to be 10% (ICES CM 1997/A:2: 7-8).

So what?

My intention is not to gloat over the over-assessments in the easy view of hindsight. Furthermore the above is nothing but outlines of explanation sketches, which *may* be fruitful. The "real" technical explanation should of course be undertaking by experts like those of the ICES scientists themselves. My intention is different; there is also a social issue here: The choice of models is not only a scientific question. This, I think should be made clearer to other stakeholders (i.e. managers and fishermen). It must be asked *what kind of assessment* we want with respect to the trade off between bias and precision. Further, the uncertainty of the assessment must be clearer to the other stakeholders.

Moreover: *If* there has been some "strategic" purpose in not revealing the "real" uncertainty, then I think that this strategy is not very likely to achieve its objective - i.e. to induce confidence and a perception of "relevance" of the advices. In that case I, on the contrary, think that *transparency* will be a much better strategy. Is it possible to look more into these questions?

The 1999 ACFM meeting

I have argued that the largest change in perception of the state of the cod stock between two consecutive years was between the 1999 report and the 2000 report. The

2000 report was the report that established the crisis. Therefore, it would be interesting to know what happened at the 1999 ACFM meeting. Was there a suspicion of things being not right? Fortunately, there is some information on this since a summary of the findings and discussions on the meetings are recorded (i.e. in the Minutes of ACFM documents).

The meeting took place in ICES (Copenhagen) from the 26th of October to 4th of November 1999.

On the 28th of October a discussion took place with respect to reference points. It was noted that there was a perceived bias in some of the North Sea round fish stocks and that this perhaps should be reflected in the precautionary reference points:

It was noted that the bias of 20% that is suggested by the Sub-group may be a serious under estimation of the uncertainty. The actually (sic) estimates of CV (coefficient of variation) are often not 20% but rather around 100%. It was concluded that the perception of a bias in the assessment was not so well founded that this would warrant a change in the reference points (CM 2000/A:2/ACFM:00A:4).

The 29th of October the discussion was picked up again:

The possibility of bias in the North Sea roundfish assessment was again discussed. In accordance with the decision made by ACFM on the previous day the reference points are left unchanged. It was now discussed if the advice should takes (sic) any bias into account, the value of 20% (overestimating biomass and underestimating F) was suggested. The conclusions of the plenum discussions should be effectively communicated to the WGNSSK (CM 2000/A:2/ACFM:00A:4).

The 30th of October the discussion on the perceived biases was continued. The chair of ACFM reviewed problems of deciding when to correct for biases and when not to. It was not only a problem for VPAs but also for a model of recruitment abundance. It was mentioned that a common reason for bias was technology improvement in the commercial CPUEs. The summary continued (and I am sorry for the inconvenience of a long quote but I think this is important):

ICES responsibility was discussed from the starting point that it is the scientists (AWG) responsibility to evaluate if the perception of a bias in the assessment is well founded. It is the responsibility of ACFM to judge if this perception should be included in the advice. ICES should in this evaluation be objective (as possible) and it is up to the managers to be precautionary. It would be irresponsible of ICES to ignore the bias if this conclusion is considered well founded and the scientists are probably in a better position to make that judgement than the managers. It was discussed to what extent the PA procedures take bias sufficiently into account. It was recognised that the procedures used at present do not take bias into account and that perhaps the PA reference point should not do so either.

We should also be aware that if we now say that our assessments are biased and if we reject them based on this, then what we have advised in the past has been useless as well.

ACFM was reminded that problems like Tapering, technological creeping in commercial fleets, influence of environment variability on stock productivity, etc. are scientific issues that should be pursued in cooperation with science committees in particular RMC.

The conclusion of the discussion was a reconfirmation of the discussion that was held on the first day: We do not fiddle around with the assessment to correct for the bias. In cases where we have long time series trends and maybe a likely reason for the bias we could consider correcting for it. In summary:

1) Decide if the assessment can be accepted or not. Decide if the assessment reflects trends only or whether also the absolute estimates are useful.
2) If the assessment is accepted then this is the basis for "state of the stock" and "catch option table".
3) Bias should be judged on a case by case basis. Basis should be commented upon in the Elaboration and Special Comment. In those few cases (if any) when the evidence of bias is very strong this bias is included in the Advice (2000/A:2/ACFM:00A).

Finally there is one more reference to the bias given under the heading: "MINUTES PLENUM 1-4 NOVEMBER".

***North Sea cod*: Long discussions on whether the assessment is subject to a bias or not. The assumed bias would be to underestimate F and overestimate SSB. ACFM found that it is quite uncertain if there is a bias but certainly there is tendency. ACFM concluded that the advice shall be based on the assumption that the assessment is not subject to a bias on the form described above but that there is a strong suspicion that the assessment is over-optimistic** (2000/A:2/ACFM:00A).

What was said in the special comment of the 1999 ACFM report? The report mentioned that the 1997 and 1998 presently were thought to be have overestimated SSB and underestimated F and continued:

The same general analytical formulation was used in the 1999 assessment, but the likelihood that F is underestimated and SSB is overestimated cannot be evaluated at this time (CRR 236, 1999: 234).

Let me now comment on the above. It really goes without saying that the suspicion of a serious bias was strong. After all this point was discussed at four different occasions on a very stressed meeting.

One sentence, I think, is striking:

We should also be aware that if we now say that our assessments are biased and if we reject them based on this, then what we have advised in the past has been useless as well.

Taken on face value, this is a very serious claim because it states that the assessments were "useless" if they were biased, which they finally were admitted to be the following year. But it is perhaps not only the content of the sentence that is striking, but also its grammatical style: It uses 1st person plural, whereas most of the summary is in terms of "ACFM" (ACFM decided, asked, made etc.). Further, the sentence is striking because it is "alone" - there is no "follow up" and therefore it seems that it could be hinting something rather than saying it. Or, on the other hand, perhaps the sentence is only striking to *me* because I implicitly was *looking* for these things.

Therefore, the sentence could either indicate an implicit agreement of that the preservation of a good record of advice would count towards not implementing the bias in the advice (interpretation A). Or it is simply saying that the advises are useless if the bias is real (interpretation B) (or both: A and B).

Another sentence *I* noticed is the one saying: "We do not fiddle around with the assessment to correct for the bias". This sentence clearly represents" the struggle for objectivity". The "easiest solution" would in fact be to fiddle around with the models and make the bias go away. But that is unscientific and not "objective". The model gave us this result and it looks like a bias. The question is then if is a bias or a random pattern, which could (or rather *should*) be neglected.

In this regard, ACFM discussed the question of responsibility. The working group should establish *if* it is a bias. And if it is, ACFM should decide *if* it should be included in the advice. The decision rule is thus: WG decides if the bias is "well founded". If so, ACFM will comment on it - and if there is "strong evidence" for it - include it in the advice. I am a bit bewildered here because "well founded" and "strong evidence" seems quite equivalent to me. Then *both* WG and ACFM have to evaluate *if* there is a bias and the decision rule contradicts itself.

Interpretation A: The decision rule was "constructed" in order to make it possible to *agree* in rejecting the bias and still feel "objective". Interpretation B: The decision rule was an attempt to deal with the difficult question if a bias should be considered or not in a formal, "objective" way.

In the 1999 ACFM report it was said that the likelihood of an over assessment could not be evaluated. The question of bias was not mentioned, although it was said that the previous two assessments were over assessments. Thus ACFM had "dissolved" a suggested bias into two over assessments and a current possibility of one more. But it was *not* mentioned what *scale* of over assessment was involved.

What would you do as a manager? Shrug, perhaps? You had got your recommendation, so why bother about a special comment saying that ACFM does not know if the current assessment is overestimated? I have constructed the latter as an "A-interpretation". The B interpretation goes: "The last *two* assessments were over assessments. Further, the likelihood of a current over assessment is *unknown*."

Transparency

My point is that whatever interpretation is right (A or B) *transparency* of the advice seems to be an obvious solution. Transparency does not solve the question of who is responsible of what within ICES, but it does make the responsibility easier to carry - and less important. ACFM could say: "This is what the model says and we don't know where we are this time", which will leave it to managers what kind of advices it considers useful (or useless). The above can, therefore, be seen as a lack of proper *communication* between managers and scientists.

Assessments *could* be better

On the technical side, the main obstacles to reliable assessments are perhaps high exploitation levels and limitations in data. Clearly the first point is a management question. The second is, obviously, related to costs, although not only so.[32] There is a limit to how much it pays off to invest in research since the marginal return of investing in science is expected to decrease. Further, there is a limit to the possible precision of assessments (Degnbol, 2003). However, similar to the case of Finlayson, it is possible that certain social forces can affect the precision of the assessment negatively for a given input of resources.

Reflecting on "The widening gap between fisheries biology and fisheries management in the European Union" a fisheries scientists, Corten, summarised some of his main points as the follows:

> **The lack of results in fisheries management had a distinct effect on biological research related to fisheries. Three of the most important consequences were a decrease in national funding, a decreasing interest in routine stock assessment, and a decreasing commitment of biologists to the former objective of rational exploitation** (Corten, 1996: 5).

This was noted to lead to a possible downward spiral, with "reduced advice leading to poor management, and poor management leading to poor advice" (Corten 1996: 15).

According to Corten (1996), an unfavourable incentive structure with respect to the production of knowledge useful for assessments applies to the present case of fisheries science (e.g. ICES, national research etc.) - similar to the case Finlayson described for DFO science in Canada. But the current case is different because the incentive structure became related to the research funding policy. Corten explains that while the national funding for assessments decreased it at the same time became possible (and important) to scientists to apply for EU subsidy programmes. These programmes were, however, intended for "innovative" research and not for routine assessments. The scientists thus had to reduce their time for used assessment research, but also the budget since many of the EU projects were *jointly* financed. The "status" of the scientist became increasingly dependent on his score on "external contracts" (Corten, 1996: 6-7).

What could be specifically be improved? The assessments suffer from biases and / or lack of precision due to insufficient information on discards, unreliability of commercial CPUEs and illegal landings. A cost efficient way to get information on the two first and somewhat amend the latter would be to put observers onboard - at least on larger vessels. Generally the problem is more related to lack of proper planning of data collection. There is a need of better co-operation between countries, between institutes in one country and even between departments of one institute. There is often an unfavourable competition between resources, resulting in a non-efficient use of resources with respect to better assessments.[33] These points call for planning of research on a proper level.

Further, scarcity of resources limits *ICES* in its work. Many ACFM and WG-documents report scarcity of available time. The workload increased dramatically with the complexity and numbers of requests. This question was specifically addressed in the October meeting of ACFM in 1998. In an analysis it was stated that: "Consistent and reliable advice is unlikely to be produced on a sustained basis under the current overstressed advisory system." The analysis mentioned three possibilities: 1) Resources should be increased. 2) Workload should be decreased. 3) Efficiency should be increased. It was noted that the two first possibilities were unlikely to

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[32] Corten (1996) cites a figure of the costs of assessment related biological research in the North Sea, Skagerrak and Kattegat: 15 million ECU (excluding fixed costs and depreciation of research vessels).

[33] These points (general and specific) "resulted" from a conversation with the fisheries scientist John Pope (May 2003).

happen. Therefore advisory process should be reviewed in order to increase efficiency (CM 1999/A:2: 33). Obviously, however, there is a limit to increases in efficiency without ending up in the problem you started out with: the problem of consistency and reliability of the advices.

The question of peer review also relates to constraints in time and human resources.[34] In discussing the possibility of independent review, it was noted that reviewers outside the working group would face technical problems of understanding the WG reports. Specifically, the 1997 report was not reviewed by anyone independent to the WG, because the WG report was only available 3 days before the ACFM meeting (CM 1997/A:2: 7).

My point is that the WG assessment and the ACFM advise represent the final steps in a long and very expensive research process. To limit the resources available at this high but relatively inexpensive step would (to a layman) seem to be a very inefficient way of proving "value for money" advices, when the knowledge production is viewed on a whole.

[34] There is perhaps an interpretation "A" here too.

4.3: The maximum recommendable

Why was the TAC generally set at the *maximum* (sometimes a little more, sometimes a little less) of what was recommendable? Obviously this question calls for an analysis of the objectives of the fishery. As it turns out, the objectives in the present case were little specified. The lack of a specified objective may be thought to leave the fishery system to be very "open". To put it strongly: Scientists will initially not know how to advice and managers not how to make decisions. Therefore, the question of "the maximum recommendable" can also be explored through an analysis of the form of the advice and changes in this. When objectives or management decision procedures are not specified there will at the outset be no formal or obvious relation between scientists and managers. The boundaries are flexible and the roles and rules evolve as you go. Moreover, the flexibility or softness of the former relation could make the system sensitive to the third actor: The industry. But perhaps the latter is really a "chicken and hen problem". For what came first: the flexible boundaries and lack of a specified objective or the industry *as an actor?*

The advisors' objectives and their initial self-perceived role

In 1976, ICES held an *ad hoc* meeting "on the biological basis for fisheries management" (CRR 62, 1976). The Council of ICES requested a review of the objectives of fishery management and encouraged the scientists to "question the adequacy and relevance of their function in the process of fishery management". The findings of the meeting were published in a report (i.e. CRR 62) which is an important document on how the scientists initially viewed their own role in the management process. These findings were summarised in the 1976 report of the Liaison Committee (CRR, 56: 21-26) and later, on the first Dialogue Meeting in 1980, managers were provided the report and were encouraged to read it (CRR 106). The report presented the biologists view on proper management.

The main part of the 1976 meeting was focused on the objectives of fisheries management. It was noted that:

Because biologically based objectives such as the highest physical yield from a resource has been thought to represent a more generally acceptable aim of fishery management than for instance economic objectives, fishery scientists have played a primary role in formulating and promoting objectives for resource management (CRR 62, 1976).

The objectives were discussed with respect to properties of certain reference points. Maximum Sustainable Yield was stated to be regarded "a common aim" for administrators and scientists. Yet, the MSY concept (the yield from fishing at F_{max}) was critically reviewed on a technical biological basis. The scientists wanted to replace MSY concept by an Optimal Sustainable Yield concept in order to avoid risks related to MSY harvest. The OSY should consider the optimum exploitation pattern, optimal spawning stock size and minimum fishable stock level. The OSY would still be calculated by (single species) yield per recruit models. Specifically it was recommended first to calculate a TAC corresponding to F_{01}[35] and then to modify the TAC against the objectives of spawning stock and fishable stock. It was stated that this policy would reduce the fluctuations in TAC, the risks of depletion and at the same time increase the catch rates and the reliability of the scientific advice.

The biologists thus perceived that they had a central role in the management process. They were not merely "technicians" who measured quantities of fish; they were actually performing the key task of designing the objective of the fishery. This role is explained and confirmed by the claim that biologically objectives were recognised to be the most acceptable.

The benefits of the discussed objectives are obviously not purely biological. Catch rates or CPUEs concerns the fisher and not the stock. Also the issue of risk may be considered an socio-economic issue. The advisors had, so to speak, challenged the boundary or rather: They had taken an expansionistic stand in an uncharted territory since a boundary *as such* has to be faced before its *position* can be challenged. In the following I will point out elements from the dialogue meetings that can give an indication of the nature and evolution of the advices and the boundary between science and management.

The first dialogue (May 1980)

The practice of dialogue meetings was, on the initiative of ICES, established in order to facilitate the communication between managers and scientific advisors (and later also representatives of the fishing industry).

[35] F_{01} is the F for which the increase yield for an small (infinitesimal) increase in F is 10% of the increase in yield that any small (infinitesimal) increase in F in pristine stock would result in.

In the first dialogue meeting CEC stated that ACFM gave much firmer advices than the former Liaison Committee. The overly important reason for that was, according to CEC, that ACFM had seen the consequences of providing options:

Invariably, the option which was chosen was that which gave the largest catch in the short term without considering the long-term consequences. In these circumstances and taking account of the state of many of the most important fish stocks, ICES to its credit, took over a role of management. In deed, it had to, because its recommendations had to be based upon assumptions about the way in which the stocks would be fished (CRR 106: 42-43).

CEC warned that a consequence of the "almost mandatory" advices: ICES would be seen as the body taking the final decisions and it would be "conceivable" that political pressure would be exerted on the Committee (CEC hinted that the 1979 report suggested the latter). Further, the managers should decide the policy and then inform ICES about it.

CEC wanted a more formal relation with ICES since it was not an ICES member. This was unsatisfactory for both parts since ICES was not paid for its services, whereas CEC wanted a formal commitment for ICES to respond to requests of EC. Moreover, CEC felt that it would be mutually beneficially if an EC observer was allowed at the WG-meetings.

CEC stressed the importance of that the advices should only be based on biological criteria. Implicit or explicit economical and social considerations should be omitted. The CEC hoped that its suggestions would enable ICES to play its true scientific role and leave the responsibility of the management to the management bodies.

There was "a long discussion about the desirability for ACFM to present its advice in form of management options". Administrators sometimes felt that the advice was a "fait accompli". Managers wanted to be able to choose between alternative "biologically acceptable" alternatives. The ACFM chairman said that the options would be presented in the next report, but that it was not always possible to give options. Further it was said that the effectiveness of advising in form of options would be strongly dependent on managers asking specific questions related to specified objectives. "If this is not done, managers should not criticize biologists for not giving sufficient options" (CRR 106, 1981: 4).

It was nevertheless realised that it in many cases would be difficult to agree on objectives of stocks shared between countries with different social, economic and political conditions. In addition, the problem for fisheries of demersal species was that its mixed nature would make the choice of objective dependent on economic and political considerations as well as biological.

A statement from Norway criticised that EC had asked ICES for an extraordinary advise on "some shared stocks in the North Sea" to consider possible "amendments" of the previously given advice. The advices should, according to the Norwegian statement, "only be submitted in consultation by all the coastal parties concerned"(CRR 106: 40). Perhaps this was what the ICES president had in mind when he said: "There is little sympathy for requests which are produced by political or economic pressure groups rather than because of apparent changes in the stock. (CRR 106, 1981: 29).

The 2nd dialogue (October 1980)

At the 2nd dialogue the CEC expressed that it was pleased with that it now was provided catch possibilities, although it understood the reservations ACFM had in granting these (as in chapter 3). Then CEC stated:

Because all who are concerned with fisheries management are fully aware that a range of catch possibilities exists and because the data from which to calculate them are freely available, it would seem, to the CEC, that it is best for ACFM to make the calculations and to present the results so that it can then authoritatively comment upon them, as it has done in the Introduction to its reports (CRR 106: 63).

CEC was interested in knowing what ACFM meant by saying that its TACs to achieve a certain F had been over optimistic. CEC would like to have firm evidence of this since ACFM in some fora was accused of being over-cautious.

The CEC asked ACFM to present F_{max} (current and previous values) in a consistent tabular form.

In the discussion ACFM noted that there presently was a transition period between two management regimes and that managers yet had to agree on their management objectives. In this situation it was necessary for ACFM to assume that the new management would broadly have the same aim as the former, which was fishing at MSY. ACFM would not advice to reach MSY fishing in one step since this would lead to a major disruption of the fishery. In addition, a stepwise reduction of the fishery would make it possible to see if the theoretical expectations followed or if the latter were unrealistic because of species interactions.

In the discussion it became clear that the setting of objectives clearly was a task of managers. ACFM noted that it only would be possible to advise on biological possibilities in achieving objectives, when these had been defined and agreed upon. Moreover, ACFM would welcome comments on the objectives on which it based its advice.

Boundaries and institutional dilemmas

In the first dialogue a boundary was recognised. CEC stressed that there should be a clear separation of science and politics. The pressure EC was subjected to by the industry is apparent from its warning of the potential for ACFM receiving pressure if its advice was inflexible and that it - perhaps in neglect of some implicit understanding with Norway - had asked for possible amendment of quotas. The CEC did not want the work of ACFM to be biased due to the industry pressure. At the same time CEC somehow wanted a closer "control" of science. It wanted to be a formal and paying customer and it expressed a desire for having observers in the working group. The latter was, however, not granted - exactly because it was important that "impartial objective scientific solutions" of the working group was not "influenced in one way or another" by representatives of "customers" (CRR 106: 4). Furthermore, CEC had clarified the power relations. CEC was a customer and when it wanted catch options it should have them. Otherwise it would be perfectly able to acquire the options by other means.

As indicated above, the relation between mandated science and politics is dilemmatic. Finlayson (1994) describes the complex and almost paradoxical relationship between the institutions of state and science. Legitimacy of the state's fishery policy is derived from its closeness to the fishery science and the objectivity of the latter. However the objectivity of the science is derived from its independence from the state. Science can only serve the purpose of the state when it is legitimate, but the political power will only support science when it serves the political objective (Finlayson, 1994: 151).

The proper link between government and science is therefore delicate. If it is too tight, political pressure may bias the scientific knowledge products. If is too weak, the scientists may be inefficient with respect to their institutional mandate. The complex and paradoxical relation will perhaps facilitate that a little politics at times sneaks into science and perhaps also that a little science at times sneaks into politics.

The missing objectives

ACFM had argued that it would be easier to advice on catch options if a specified objective was agreed upon. In fact it may be added that an advice makes little *sense* if there is no specification of what the advice is for. ACFM regarded the lack of objectives to relate to the transition in management regimes following the introduction of EEZs. In order to *advice* it had assumed a continuation of the previous biological objectives and invited CEC to revise or comment on these. It was thus clearly recognised that the setting of objectives was a task of managers. The policy of stepwise reduction was mentioned to have two reasons: An socio-economic reason (disruption of fisheries) and a biological reason (multi-species concerns). It is perhaps noteworthy that the issue of separating science and politics was not invoked in this regard.

CEC had explained difficulties in agreeing on objectives. The difficulties were social and economical - concerning both international and inter-species questions. Apparently CEC implicitly accepted the biological objectives since they asked for F_{max} in tables. However, CEC did not yet reveal if or how it was *committed* to these objectives. Remember that the conservation policy of CFP (e.g. the "relative stability") was negotiated between 1977 and 1983.

The 3rd dialogue (September 1981)

In the introduction of the meeting, the ICES president said that it was desirable if management representatives would provide feedback on the objectives implicitly or explicitly stated in the ACFM reports.

The chairman of ACFM presented its new form of advice, which was used for the first time in the July 1981 report. ACFM would provide catch options for the stocks where it was possible. Yet, ACFM would not present options for depleted stocks, or for stocks suffering from recruitment failure. Further, for stocks fished largely in excess of the biological reference points ACFM would *provide* options "within safe biological limits" and *recommend* one of these options. For stocks largely fished at the biological reference points ACFM would provide options "within safe biological limits" and only indicate a *preference* (CRR 106, 1981).

Statement of CEC: The industry problem

In its statement the CEC welcomed the dedication of the meeting for a discussion on objectives. However, the intention of CEC was not to develop an overall policy. In stead CEC wanted to set the biological advise (and the objectives it was based on) in the political arena, in which CEC tried to achieve agreement, and to explain why the form of advice rather was a hindrance than a help to effective management.

CEC reminded that it previously had stated that the setting of objectives was a management task. However, the contemporary serious problems of the fishery were a hindrance for decisions. The fishery was an open access system where increased fuel prices and decline in fish prices lately had turned a break-even situation into that of unprofitability. This lead to an *increased* pressure for larger catches even that these only would result in a short-term release. The CEC continued noting that: " 'Solutions' (sic) were made and still are sought in operational and market subsidies, even though economic analysis shows that such subsidies do not provide a solution but lead to increased exploitation of the fish stocks".

The CEC agreed with ACFM that a long-term objective would be to reduce F and bring the fishery closer to MSY and that the reductions should be stepwise. The benefits would be higher catch-rates (profitability), greater stability, better forecasts and planning possibilities and elimination of recruitment failures.

Where CEC disagreed was with respect to the tactics of reaching the objective. The CEC considered that there was no necessary reason to reduce F by any predetermined amount (if at all) unless the stock was suffering from recruitment failure, although the CEC agreed that F should not be allowed to increase. There was no point in ACFM recommending reductions that were unacceptable to managers. Reaching agreements would be increasingly difficult, and ACFM would become increasingly "disenchanted" that its advices were not accepted - which was the present situation.

CEC also stated:

If the dialogue between ICES and managers is going to be meaningful, it cannot be conducted on the basis of ACFM deciding what information and advice it will entrust to the managers.

Fisheries science does not exist in a closed world and the managers will turn elsewhere for advice if ACFM adopts this attitude (CRR 113:29).

CEC continued that if ACFM (to the above quote) responded that its advices were not mandatory, then that was only theoretically true. For CEC found that Norway considered the recommendations of ACFM "nonnegotiable".

CEC therefore found that the policy of ACFM to recommend annual reductions in F of 10% or more was a hindrance to an overall management policy. First F should be stabilised and then it should be slowly reduced.

Adopting roles and rules

The new form of advice can be seen as a sort of compromise. The catch options were granted but were not provided for stocks suffering from recruitment failure. Further, the recommendation by ACFM would be increasingly restrictive with a decline in the stock condition. Catch options would only be provided "within safe biological limits".

A puzzle is why CEC wanted less restrictive *recommendations* once it had got its catch options. After all it had argued for the catch options in terms of that it needed flexibility and because it wanted the responsibility itself. The comment on the "strategic" issue that Norway would stick to the recommendations is, at least partly, an answer. On a more general level I think we can explain in terms of the "institutional dilemma". One hand CFP had stressed the importance of separation of science from politics (and it did that on several later occasions). Managers needed an "objective" science (otherwise it doesn't really back you up). On the other hand managers of CFP needed actual backup for their policies: "Sorry, but this is what *ICES recommends*" - without the recommendation being completely "unpalatable". Therefore, the CEC had to influence first, the *way* the advices were given and secondly, the *recommendations* themselves.

I included another statement of the power relation: If the catch options were not granted, it would perhaps be a problem for ICES - but not for the managers of the CFP. Yet, ACFM apparently resisted the pressure to change their recommendation policy: ACFM continued recommending 10% F reductions. Actually ACFM at times, especially later, increased the annual F reduction to 20-30% - on the paper that is.

CEC had explained thoroughly why objectives were not easily agreed upon in the CFP. Or rather they did in fact agree on the objectives. Yet, they were facing an immense pressure from the industry, which was already suffering from serious economic problems. In the 4th Dialogue in 1982, the state of these problems was confirmed when "some administrators" noted that there was "no real prospect of sound management" before the enormous over capacity of "some member countries" were brought under control (CRR 122: 4). Thus the CEC found that is was unable to commit itself to a policy in order to achieve the objective (e.g. decision rules), although that it did say that it would not allow increases in F.

The roles that were developing were clearly those of science allowing a certain maximum limit and management, because of industry pressure, to take all science would allow. However, the pressure would perhaps also destabilise the biological limits that science felt it was necessary to constrain the freedom of the managers.

From normative to explorative advice

After the 3rd Dialogue a former ACFM chairman reflected on the changes in the form of advice (CM 1982/Assess:21). He (succinctly) described the changes as a transition from "normative" to "explorative" advices. The explorative advice was favoured in a situation where objectives could not be agreed on. The former chairman continued: "The only norms to be indicated being the norms introduced by the biological system itself (…) covered by the term "safe biological limits". The former chairman added that the position of ACFM was inconsistent to the explorative advice with respect to "additional conservation measure". For example ACFM was still, normatively, recommending increased mesh sizes.

This paper, which reflected on the transition from "normative" to explorative advice, was later presented at the 4th Dialogue in October 1982. In the discussion, perhaps raised by this paper, it was noted that one should distinguish between *predictions* and *advices*. Advices could only be given when an objective somehow was assumed. The 4th Dialogue report notes that some speakers felt that "ICES should restrict itself to making predictions" (CRR 122: 4).

The latter is perhaps another expression of the "institutional dilemma". The managers wanted "predictions" but they should at the same time be (labelled as) "advices".

The CFP Conservation policy of 1983

In 1983 the fisheries CFP was completed by the adoption of the conservation policy, which simultaneously was the resource allocation policy. The conservation policy was adopted by the Council of Ministers as the Council Regulation no. 170/83. The objective was seemingly stated in article 1 of the 170/83[36]:

In order to ensure the protection of fishing grounds, the conservation of the biological resources of the sea and their balanced exploitation on a lasting basis and in appropriate economic and social conditions, a Community system for the conservation and management of fishery is hereby established (Council Regulation No. 170/83).

Article 2 goes:

The conservation, measures necessary to achieve the aims set out in Article 1 shall be formulated in the light of the available scientific advice, and in particular, of the report prepared by the Scientific and Technical Committee for fisheries provided for in article 12[37]**(**Council Regulation No. 170/83).

So according to Article 2 the *aims* are presented in article 1. In referring to the 170/83, Corten (1996: 3) notes that: "The Community committed itself firmly to the objective of rational exploitation of the resources, in order to achieve the maximum socio-economic benefits of the stocks". This must accordingly be Cortens interpretation of Article 1. However, the problem, as it turns out, is exactly the flexibility of interpretation of the sentence. Nothing was mentioned of "rational" exploitation and the "balanced exploitation" is not specified. What about the phrase "appropriate economic and social conditions"? Did that phrase commit the Community? Let us see how the dialogues continued.

The 5th Dialogue (October 1985)

A statement from Norway noted that there, for the demersal fish Stocks of the North Sea, often was a "significant gap between the catch levels recommended and the rate of exploitation that the scientists in practice are prepared to accept". This made it "difficult to identify the real biological issues" and the "weight to attach to the principal scientific recommendation." Decision-makers would find it difficult to know

[36] The Council Regulation No. 170/83 is (for example) presented in Wise (1984: 263-269).
[37] The role of STFC or later STECF would be interesting to examine. However, I cannot do that in the present work.

"when to be tough and when to be complacent". If the stock needed "stiff protection" that, as well as the opposite, should be made clear (CRR 139, 1985: 21).

The critique by Norway indicates that politics perhaps not yet had been purged from the now exploratory advices. According to the form of advice, ACFM *provided* catch options "within safe biological limits", whereas they would *recommend* a decrease in F towards F_{01}. Since Norway undoubtedly knew the form of advice, it must either have doubted that the "safe biological limits" provided "stiff protection" or that being outside safe biological limits was a way of saying that the stock needed "stiff protection". In either case it seems that Norway hinted that the term "safe biological limits" was not rigid enough to avoid "political exploitation".

In the discussion Holden, a commissioner of the CEC, made two points relating to the question of objectives. First he noted that F_{01} was not a biological but an economic reference point and that it thus was irrelevant with respect to the scientific advice. He mentioned that to reach the reference points (in the short term) would imply social and economic problems and would require tough political decisions. Secondly, he questioned the scientific justification of the expected benefits from F_{max}. In a discussion on the relation between ICES and fishery management,

..,. Mr Holden questioned whether fisheries administrators can be considered as managers, but felt instead that the politicians, who respond to the various pressures to which they are subjected, especially from their fishing industries, are the actual managers. This is the case in the EEC where the Commission interprets the scientific advice and drafts legislation but the Council of Ministers makes the final decisions. He indicated that within the EEC, the management objectives have to be set in the light of these pressures and circumstances. Because of this, there is no single long-term management objective within the Community; but a current aim is to endeavour to maintain the fishing mortality at the same level from year to year (CRR 139, 1985).

The administrators were thus no longer interested in the objectives as assumed by biologist in order to *advice*. Holden explained that there still were no actual objectives; the 170/83 regulation had not improved this. In explaining the decision-making process, Holden pointed out how the industrial pressures impacted on the council of ministers to prevent agreement on management objectives. Holden (1994) provides a more detailed analysis of these matters.[38]

[38] Decision making process: 1-16 and the effect of industry pressure on the Council of Ministers: "Political Expediency vs. Scientific Advice": 88-116.

In the 6th dialogue (October 1987), the discussion of long-term objectives was one of three main issues. Yet, there was no statement from CEC on the issue, even that it had arranged the meeting. Long-term objectives were perhaps still not a possibility in the CFP (CRR 158).

The 7th Dialogue (November 1989)

The theme for the dialogue was "biological, economic and social considerations in determining objectives of fisheries management".

Holden, explained, relating to the issue of "stability", that the critical problem was over capacity. The fisherman had to pay his loan "today" in order to be able to fish "tomorrow". Investment in the stock to get higher catches and stability was not an option. Holden maintained a certain "ratchet effect". When advices permitted high TACs, the industry pressure would result in the highest possible TAC ("stability" would not be "invoked"), whereas "stability" would be invoked to make the reductions as small as possible when advices recommended smaller TACs.

Holden also explained the conflicting pressures the EC and governments are subjected to. Many stocks were fished far beyond what is optimal, which generated a pressure on management to reduce the exploitation level. On the other hand the managers needed to respect the "democratic process" by responding to the pressures from the fishing industry.

The lack of commitment to 170/83

If the summaries 5th and 7th dialogues seem somewhat repetitive, I apologise for that but it is actually in itself a point: Little or no improvements had resulted from the establishment of the conservation policy. The stocks were still heavily over fished and the pressure on the national governments in order to set the TACs as high as possible continued to be very strong - and effective.

Holden (1994) explained that the 170/83 rather than being a policy to facilitate implementation of conservation measures, paradoxically, became a hindrance to implement them. In the 170/83, the terms "social and economic considerations" were, seemingly constituting parts of the description of the objectives in the conservation policy. Yet, it was, according to Holden, exactly with reference to "social and economic reasons" that measures, which the industry felt to be restrictive, were blocked by the national ministers in the Council. The phrase "social and economic reasons" provided a legal basis for *not* adopting proposals (or delaying proposals). It was in Holdens words "a magic talisman", a reason that provided a ready justification for voting against proposals, since the social and economic reason did not have to be specified. And the talisman was kept for exactly this reason (Holden, 1994: 113-115).

Safe biological limits

In the 1985 Dialogue, a statement from Norway had hinted that the term "safe biological limits", which was fundamental in the form of advice from 1981, perhaps did not provide "stiff biological protection". Let us therefore examine the use of this term.

In the 1981 form of advice, which basically was not revised until 1991**,** *Catch options* would only be provided within safe biological limits and they would not be provided for stocks "which were depleted or suffered from recruitment failure" (CRR 114: 2). Therefore, the condition for a stock being within "safe biological limits" must logically have been that it was not "depleted or suffering from recruitment failure".

In the 1986 report ACFM noted 150.000t to be the lowest SSB level at which "a consistently good recruitment had been observed". The SSB was in the same report estimated to be 78.000t. To obtain a SSB of 150.000 at the beginning of 1988 would require a *closure* of the fishery. Still ACFM *recommended* a catch less than 125.000, which corresponded to a predicted SSB of *73.000t* in 1988. Further, ACFM *provided* catch a option corresponding to an F that was such that the resultant SSB in 1988 was expected to be *55.000t*. (CRR 146, 1986: 80). Hence, according to the stated form of advice, a SSB of 55.000t was within safe biological limits, which in turn was contradicted by stating that 150.000t was required to give consistently high recruitment. For the latter was after all the content of the definition of the "safe biological limits".

Similarly, the ACFM report of 1984 had provided a catch option resulting in an expected SSB of 84.000t in 1985 (CRR 131, 1984: 62). The highest option in 1985 would result in SSB being 95.000t in 1986, in 1987 the highest catch option would imply SSB of 106.000t in 1988, and in 1989 it was 91.000t for 1990. In the 1989 report, ACFM reminded that an SSB of 150.000 was the suggested minimum. ACFM warned about the low egg production that may result from the concurrent low SSB, which was estimated to 96.000t. It was said that the 1984-1988 year-classes gave "reason for concern in this respect". Yet, ACFM recommended a catch corresponding to an SSB of 102.000 in 1990.

Thus ACFM had either given up its "form of advice" of only providing catch options inside safe biological limits, or the suspicion of Norway, that the "safe biological limits" were too flexible to protect the stock, was justified. In any case the advices provided cannot exactly be said to have been within safe biological limits. After all the stock was at a historically low level.

Management by avoidance

Although flexible, the term "safe biological limit", introduced in the form of advice in 1981 was an important precursor of the later MBALs and the Precautionary Approach. It was important in representing a new way of thinking: Management as not being in terms of *where you want to be* but in terms of *where you don't want to be*. This new way of thinking was suitable for two purposes: The biological purpose of not "collapsing" the stock, and the *implicit management purpose* of being able to meet short- term industry needs while still being backed up by science. It was flexible and scientifically objective at the same time.

Introduction of MBALs

In 1991 ACFM changed its form of advice. Now ACFM would basically recognise two types of stocks: Those with an SSB higher than a Minimum Biological Acceptable Level (MBAL), and those for which that was not the case.
Stocks below the MBAL, would be "outside safe biological limits" and ACFM would advise in order to rectify this. To set objectives was a task of managers. ACFM itself would only have one declared objective: To maintain fisheries within sustainable ecosystems. ACFM was worried of the state of some stocks (CRR 1979, 1991: 4-8) (note that the culmination of the first crisis of the cod stocks was in 1992) - and the cod stock was one of the most important stocks within CFP.

The important difference between the form of the advices prior and subsequent to 1991 was that "safe biological limits" were now *specified*. For the cod stock the MBAL was set at 150.000t. The explanation was as follows: Detailed data existed from 1963 an onwards. The spawning stock had recently declined to a historically low level of 70.000t. The lowest SSB level from which a recovery of the stock had been observed (since 1963) was 150.000t, which ACFM therefore considered to be the "lowest desirable level" or MBAL (CRR 1991: 78).

The MBAL was thus equal to the SSB level, which in the 1986 report was mentioned to be the minimum value for which consistent recruitment had been observed. The 1991 definition of "safe biological limits" thus clearly contradicted the implied meaning of the term in the previous advices for the cod stock (as described above).

The MBAL was set from a scientific point of view - it was purely biologically *defined*. Further, it was a *number* set by assessment science. And you cannot argue about a number - unless you want question the *objectivity* of that number. Science had taken a firm stand even that the ground perhaps was still a bit shaky. After all it was perhaps a bit arbitrary that the stock had recovered from exactly 150.000 tons since 1963. But now there was a biological fix-point. Yet, rules for navigation around that fix-point were still not specified. Remember that the cod was instantly on the wrong side of the MBAL - and it stayed on that side until today, even that you thought you had reached the MBAL in 1997 (although ACFM perhaps did not admit that it thought that the stock had exceeded it).

The 1991 report also introduced three other biological reference points (BRPs): F_{high}, F_{med} and F_{low}. F_{high} was the F for which survival was so low that recruitment historically was not able to compensate the losses in 9 of 10 years, whereas F_{med}, was the F for which compensation was observed half of the years and F_{low}: compensation in 9 of 10 years. It was explicitly (and in capital letters) noted that the BRPs were not objectives. The BRPs should only serve as a "guide to aid managers in choosing from the range of options open to them" (CRR 179, 1991: 6). Personally I am not sure how important these BRPs were to the managers, but they perhaps underscored a slight change in the *discourse* towards "pure biology". As it turned out there was an implicit problem of the BRPs: They were, strictly, only useful if the general level of Fs did not change, which it did for (for example) the cod stock. If the F increases, the BRPs for F will increase too. In other words, the BRPs will only guide you to go no further than where you are - and if F increases anyway, the "guide" will consequently accept successively higher F levels.[39]

[39] F_{max} and F_{msy} are subjected to related problems. When the assessment parameters change these reference points will change too. The same goes for the MBALs and the PA reference points.

Biological critique of MBAL advices

In one way, the MBAL could be seen as a boundary shift. Science regained some territory and *defined* a border. For science and stock conservation this could be seen as a step forward. Nevertheless, some scientists saw the change as a step backwards. In "The use of the MBAL concept in management advice" Corten strongly criticised the change in the form of advice (Corten, 1993). The MBALs had become "the cornerstone" of the advice, which Corten felt was wrong for several reasons.

Firstly, the *position* of the MBAL was not known and would change with history since it was dependent on recruitment relations, which were heavily dependent on unknown environmental factors. This was a problem when a stock would approach the MBAL since managers would tend to delay a hard decision when there was no hard justification of that decision.

Secondly, the MBAL would tend to be interpreted as an *objective* by managers. And as Corten explained, the phrases used to describe the MBALs could support that interpretation in that the phrase "lowest *desirable* level" for example was used for the cod stock (CRR 179, 1991: 78).

Thirdly, the MBAL was ignoring the *ecosystem effects* of fishing. If the MBAL of a commercial species allows a high F, this could result in extinction of other, non-commercial, species. Which, I add, would contradict the explicit objective ACFM stated when it introduced the MBALs.

Corten argued for a return to the "rational" objectives, i.e. F_{max} or rather F_{01}, which would provide a combined solution to the problems of the MBAL.

Firstly, the rational reference points were also arbitrary, but they were arbitrary o*ptimums* instead of arbitrary *minimums*. Secondly, they would provide higher and more stable catches at lower costs. Thirdly, they were much more *precautionary*. Let me explain the latter. For the cod stock the F_{max} was generally calculated to be around 0.3 or less and the F_{01} was generally around 0.2 or less. The 2002 values are 0.248 and 0.148 respectively (ACFM 2002: 40). On the other hand the later F_{pa}, which was calculated to correspond to an expected SSB being equal to the MBAL or B_{pa} was 0.65. In other words *the rational exploitation level "were around three or more times as precautionary" as the MBAL or as the later Precautionary Approach.*

ACFM had changed from "normative" to "explorative" advices. This was essentially because managers had asked for flexibility or catch options. And Corten felt that it was wrong. More freedom was left to the managers than was "justifiable on biological grounds". The MBAL, Corten said, was the "last defence line" to put a "limit to the freedom of managers". To him it was a question if "to advice or not to advice" (Corten, 1993: 7).

The multispecies perspective: Changing the paradigm?

Yet, there was perhaps *another* reason why ACFM - in being exploratory advisors - did not *stress* the rational reference points as much anymore: The results of the multi-species models.

In 1985 the Multi-species Assessment Working Group met "to continue trials with Multi-species-VPA models". The results were quite striking: The yield curves when inter species predation was included were much flatter "than conventionally assumed" - and the cod stock currently appeared to be *under-exploited* (CM 1986/Assess:9: 21). If F was halved, the yield would be unchanged or even *decline* moderately, whereas the SSB would increase (by more than 100%) but much less than predicted if predation was not included (CM 1986/Assess:9: figures 4.3.7 and 4.3.8). Later multi-species works generally confirmed these results. The multi-species perspective did not affect *the short-term forecast*, the retrospective stock history or the assessment very much. What was changed considerable were the medium, and in particular long-term predictions. Whereas the general conclusion from single species predictions was that most stocks were fished at levels of fishing mortality far above F_{max} , the outcome of multi-species predictions was that the level of fishing was close to F_{01} (CM 1996/Assess:6 : p578). Therefore:

Multispecies predictions would thus tend to give relatively more importance to recruitment overfishing in managament considerations at the expense of considerations pertaining to of growth overfishing (CM 1996/Assess:6 : p578).

On the background of the multi-species results it is thus tempting to conclude that whereas "rational" exploitation was more precautionary than the later Precautionary Approach, the Precautionary Approach on the other hand was more rational than the "rational" exploitation. When Corten argued for F_{01}, and when ACFM continued to present the rational reference points in the ACFM reports there, accordingly, are two interpretations. "A": the reference points is a "rhetoric device" to "tempt" managers to reduce exploitation towards more precautionary levels - or in fact more *economic* levels: it is still *expensive* to over-fish - which is a *normative* interpretation. The interpretation "B" is that the multi-species results still were/are too controversial to be recognised. However, let me be fair: For the later years the gains in yield as expected from the rational reference points seem to have decreased (i.e. the yield curve has become more flat) in the ACFM reports. Moreover, the above probably only concerned one of Cortens three points.

At the 6th Dialogue in 1989, a ICES scientist had presented the gains that would had been possible for the period from 1971 to 1986 from rational management. If the F had been reduced gradually towards F_{max}, the average catch of cod in the North Sea would have been 300.000t and not the 200.000t that became the actual situation. If, by technical measures, no fishing of juveniles was allowed, the average catch would have been 350.000t (CRR 158, 1989: 7-17). The SSBs in 1987 from managing by the "biological strategies" would, according to the single species models, have been 1.300.000t by fishing at F_{max} - about 5 times the highest recorded SSB levels.

At the same time multi-species calculations were saying that the cod was under-exploited. There clearly was a conflict and perhaps a paradigm shift was developing.

The reviewed conservation policy

In 1992 the conservation was policy was reviewed. With respect to objectives it said:

As concerns the exploitation activities the general objectives of the common fisheries policy shall be to protect and conserve available and accessible living marine aquatic resources, and to provide for rational and responsible exploitation on a sustainable basis, in appropriate economic and social conditions for the sector, taking account of its implications for the marine ecosystem, and in particular taking account of the needs of both producers and consumers (Article 2 is cited in Holden, 1994: 223).

In comparison with the 170/83 this was certainly a step towards the setting of objectives. However, the stated objectives obviously involve trade-offs and there was no specification of how to prioritise the objectives or how to implement them. An Article 8 provided for that objectives "*may*" be specified, which perhaps admits the former. (Holden, 1994: 223). The Council of Ministers was thus still not very constrained by the legislation of the conservation policy. In the view of Holden the 1992 legislation implied that fixing of objectives was only seen as an "occasional necessity" (Holden, 1994: 225). The Council was still not "committed". There was a flexibility, which the advices, as will follow, still allowed for.

Safe biological limits and MBALs

The MBAL based form of advice carried on until 1997, where the Precautionary Approach was incorporated into the form of advice. There are no Dialogue Meetings from between 1991 to 1997 relevant to the present question and I do not have much further information on the perceptions of this form of advice. But let me briefly compare the MBAL-format to the previous forms of advices.

Before 1991 the "rigid" part of the advice was the *range* of catch options provided, which was constrained to be "inside safe biological limits". The "safe biological limits" were, however, not defined and thus subjected to flexibility and political or socio-economic pressure. The latter was expressed by the discrepancy between the "unofficial MBAL" mentioned in the 1986 report and, on the other hand what was must have been *implied* as "safe biological limits" in order to provide the range of catch options that were apparent form the ACFM reports. This was in spite of that the description of the "unofficial MBAL" very much resembled the official description of "safe biological limits". On the other hand, the later official MBALs or "safe biological limits" were "rigid", but there was no "rigid" or predetermined way of deciding on the *range of catch options* to be presented or on what to *recommend* from this range. For stocks below the MBAL:

…, ACFM will in so far as possible give advice on what measures are needed to rectify the situation. The severity of this advice and the extent to which management options are possible, will normally depend on the degree of depletion of the stock and on what information is available on the historic series of stock and recruitment (CRR 179, 1991: 7).

So the MBAL was rigid, but what to do when you were below it was open to interpretation. The MBALs was one step forward and one step back with respect to provide "stiff" biological protection: The policy for giving advices was still flexible, which we can see from the actual given advices.

Until and including the 1994 report the range of catch options provided was from corresponding to an F equal zero and up to status quo F. Remember that the years from 1991 to 1994 were the years of the culmination of the first crisis. The reports of 1995 and 1996 increased this catch option range to $1.2*F_{sq}$. In the 1991, 1992 and 1993 reports *none* of the presented options would be expected to increase the SSB back to the MBAL in the following year. In the 1994 report F < 0.20*Fsq would, in the 1995 report F< 0.3 would and so forth. In these years, however ACFM did not recommend any TAC. Instead ACFM recommended a reduction of F to *0.70*Fsq*.

The point is that only by combining the rigid elements of the two forms of advice you would make the advice strict in order to ensure the SSB to be within safe biological limits in the sense of the unofficial or official MBAL.

Surrogate objectives

Importantly, the MBAL, as biologists had warned against, had probably become a sort of "surrogate objective". And as Corten had noted the phrases "desirable level" and "target level" did perhaps not do much to prevent this. The thought of a fix-point was gaining foothold, which was an improvement with respect to conservation, although it with respect to management was in terms of "where you don't want to be". Nevertheless, there were still no effective constrains on the advices in order to "force" a recovery.

The Precautionary Approach

The Precautionary Approach (PA) was incorporated into the form of advice in the 1997 ACFM report, and the resulting form of advice has continued until today. The intention of ICES was to provide advice in accordance with the international agreements of the PA. In order to carry out the PA advice ICES would continue with its stated objective of 1991 (i.e. in terms of a sustainable ecosystem). A distinction was made between limit and target reference points. The former were to constrain harvesting "within safe biological limits", whereas the latter were objectives within the providence of managers.

In order to avoid the limit reference points (F_{lim} and B_{lim}) ICES/ACFM would propose two more reference points, F_{pa} and B_{pa}, at which, when uncertainty was taken into account, the probability of reaching the limit reference point was very low. Moreover, ACFM would encourage and assist managers in defining target reference points and, if necessary, in designing recovery plans (CRR 223, 1997: 8). The burden of proof had shifted such that the less you knew about the stock the more precautionary you would have to be. On the May 1997 ACFM meeting the term "safe biological limits" was mentioned to be "subjective" (ICES CM 2997/A:3). The Precautionary Approach would imply rigid definitions in order to eliminate the subjectivity.

PA in ACFM advices

In the 1997 report the PA reference points were not yet defined - the MBAL of 150.000t thus continued to define "SBL". In the 1998 report the PA reference points were defined:

ICES considers that:
B_{lim} is 70.000t, the lowest observed biomass.

ICES proposes that:
B_{pa} be set at 150.000t. This is the previously agreed MBAL and affords a high probability of maintaining SSB above B_{lim}, taking into account the uncertainty of assessments. Below this value the probability of below average recruitment increases (CRR 229: 207).

What does it mean? Does the "this value", below which recruitment is impaired, refer to B_{pa} or B_{lim}?

Furthermore, ICES/ACFM established F_{lim} = 0.86, which was the F "estimated to lead to potential stock collapse". In addition ICES/ACFM proposed a F_{pa}=0.65 to be the F for which there was a 95% probability of avoiding F_{lim}, when uncertainty was taken into account.

In the introduction it was explained that making the MBAL equal to the SBL was a "needlessly restricted interpretation" of a multidimensional concept (Fs, SSB and more). For stocks being inside SBL there were two conditions: 1) The stock level should be such that it with a high probability was above the level where recruitment is impaired (B_{lim}). 2) The F should be such that it with a high probability would not drive the stock towards the B_{lim}. In other words: to be inside SBL, the stock should be inside the PA reference points (CRR 229, 1998: 5).

In the introduction the B_{lim} was defined as the limit SSB "below which recruitment is impaired or the dynamics of the stocks are unknown". Consequently the above question is answered: The recruitment was impaired below 70.000t. The PA reference points were thus (qua B_{lim}) *less* precautionary than the MBAL.

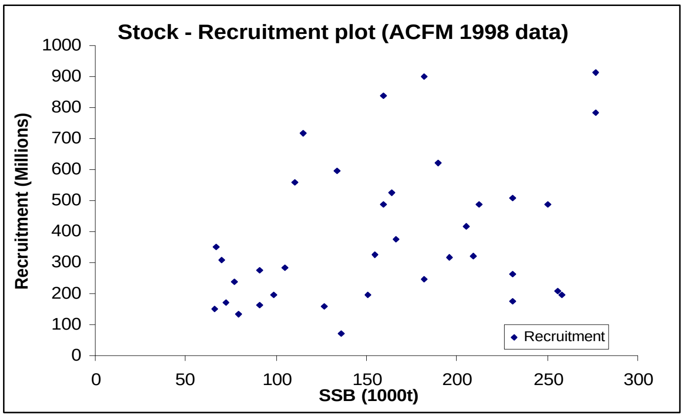

Figure 15. Stock recruitment plot for Cod for the combined ICES areas of the North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). The data series is from 1963 to1998. Source: ACFM CRR 229, 1998: Table 3.5.2.2, p213.

In figure 15, the recruitment of each year is plotted against the SSB estimate of that year. The data are from a table in the 1998 report, which lists the assessment outputs from the concurrent assessment. It therefore represents the ACFM 1998 report perception of the stock history. Certainly the B_{lim} is an SSB, below which the probability of high recruitment is impaired. But it is perhaps more important that this *also* seems to be true for the old MBAL value of 150.000t - which was introduced, unofficially, in 1986 for that reason.

The WG on North Sea stocks was for its meeting in October 1997 requested to "propose limit reference points to be avoided with a high probability" (CM 1998/Assess:7: 620-623). The WG referred to a work of The Study Group On the Precautionary Approach to Fisheries Management, which had recommended PA reference points to be associated with no more than 5% probability of the limit reference points being exceeded. The WG group used this as a guideline and presented its results in a table with the heading: "Biological reference points based on PA criteria". The results for the North Sea cod were B_{lim}=150.000t, $F_{pa.5}$=0.73 and $B_{pa.5}$=221.000t. The WG had used the old MBAL as a limit reference point, which the WG noted to be subjected to a the following criterion:

…stocks for which there is adequate information on historic stock and recruitment, the MBAL is defined by the level of spawning stock below which the data indicate that the probability of poor recruitment increases as stock sizes decreases;… (CM 1998/Assess:7: 620).

Which resembles the description of the unofficial MBAL ACFM in 1986 (and in some subsequent reports). Using the MBAL as the limit reference point, which was in accordance with the PA due to the above MBAL criterion, the WG had calculated the B_{pa} reference point, which was estimated to be associated with a 5% risk of reaching B_{lim} - or the MBAL. The B_{pa} was accordingly 221.000t. The WG had calculated the reference points on a basis of medium term simulations but mentioned the following technical problems: The reference points were very sensitive to the chosen recruitment model, some sources of variability were not included and the procedures to estimate variances may had resulted in underestimates. Therefore the WG warned that:

These points will all tend to result in underestimates of the variability in SSB in the medium term and thus in overestimates of Fpa. For several stocks the precautionary approach reference points indicated should therefore be considered overestimates of Fpa and underestimates of Bpa (CM 1998/Assess 7: 622).

The WG therefore considered its B_{pa} was perhaps not *sufficiently* precautionary. What happened between the calculations of the WG in October 1997 and the 1998 ACFM report I do not know. But I do know that the B_{lim} of 70.000t and the B_{pa} of 150.000 continued in the ACFM advice for the cod stock until and including the latest ACFM report in 2002.

The Study Group on Precautionary Reference Points For Advice on Fishery Management (SGPRPAFM) met in February 2003 to propose revisions of the reference points. This WG proposed to raise the B_{lim} to *160.000t*, which was "slightly above the present B_{pa}" (SGPRPAFM, 2003: 34). The SGPRPAFM concluded:

North Sea cod has received a substantial amount of scrutiny over the most recent years because of the rapid decline of the stock to well below the current B_{lim}. The existing reference points for North Sea cod were established in 1998. B_{lim} (70.000t) was determined using a rounded B_{loss} (from the method of Cook) while B_{pa} (150.000t) was taken as the MBAL current at that time. (…).

In the period since setting these reference points, recruitments for cod have stayed low and the SSB has continued to decline; the most recent estimate suggests that SSB is around 38.000t. It is clear that for some time recruitment has been impaired and that this becomes increasingly evident in the structure of the stock and recruitment plot which shows a more or less steady decline to the origin.

An examination of the stock-recruitment plot shows that recruitment impairment has been occurring some way above the existing B_{lim}. Whereas within the ICES precautionary approach framework, B_{lim} is by definition taken to indicate a point below which impairment occurs. There is clearly an issue to address here and the current B_{lim} is inappropriate. On basis of new evidence in data which have been added since the 1998 the WG should consider the change point of 160.000 tonnes as a potential candidate for B_{lim} and investigate the consequences in terms of PA points for this stock (SGPRPAFM, 2003: 37).

So the B_{lim} of 1998 was set by using another method than the WG did in 1997 and the addition of later data points pointed towards revising the B_{lim} up to 160.000t. Is the above discrepancy then explained?

When I look at figure 15 I do not think so. Further, as the WG noted, the MBAL was defined to indicate the SSB level below which recruitment was impaired. The WG had in 1997 accordingly calculated PA points to avoid that the limit of 150.000t with a high probability. Moreover, it is somewhat strange that ACFM first requests a team of experts to calculate the reference point and then later rejects the result. It is true that the last 5 data points reinforce the interpretation that B_{lim} should be at least 150.000t. The rounded B_{loss} method implies to set B_{lim} as a rounded figure of the lowest observed stock size (CM 1997/Assess:7: 9). This was how B_{lim} was set and is still is defined for the cod stock. But so set B_{lim} at 70.000t, given the 1998 data series, can hardly be termed precautionary. And for every subsequent data point since 1998 (with very low recruitment and SSB levels) this should have become increasingly apparent.

At least I expect you to agree that the presentation of PA reference points were not exactly straightforward. I cannot "prove", but I do suggest, that the above confusion is somehow due to the highly political sensitivity of the issue. Implicitly or explicitly biologist were concerned about the immediate catches of fishermen. The PA values were never a strict biological calculation, and as before, the interpretative flexibility can be exploited by social forces in one way or another. If, on the other hand, I have been led astray be successive misinterpretations I would like to be informed about it since I admit that I feel "uneasy" about the suggestions above. The question of PA reference points is of so conflicting nature that I almost do not know what to think about it, which, however, to me seems to support the above suggestions rather than to refute them.[40]

How does this relate to the question of the "maximum recommendable"? The PA reference points formed the basis of the recommendations subsequent to the 1997 report.

[40] In all cases I think the question of PA reference points would be worth to investigate further. The role of the recruitment assumption in calculating the reference points is interesting. What happens to the PA reference points if you assume that "the gadoid outburst" is over?

The Maximum Recommendable

The question of why "the maximum recommendable" was chosen by managers turned out to require a two-sided analysis. One the one hand: why the managers decided on the "maximum". On the other hand: what during the history was implied by "recommendable" by the advisors.

Why the managers decided on the "maximum" is perhaps not so surprising. It was because of the intensive pressure on and by the industry. The industry was subjected to a general over capacity since the introduction of the EEZs and faced increasingly severe economical problems as the stocks and TACs declined. Note that the decline was not only due to over-fishing but, importantly, also due to the decline of the "gadoid outburst". The fleet North Sea had been built up on high expectations because of the high recruitment. So there were probably at least two reasons for the existence of over-capacity and at least two reasons for the decline in stocks. This gave the CFP a very tough start. Fishermen and therefore managers faced severe problems - agreements were difficult or almost impossible to obtain.

As noted by Holden, the CFP became discredited from the very beginning since catches of the important demersal stocks were declining after its introduction (Holden, 1994: 151-154). No one would blame nature, so CFP had to take the beating. The latter relates to a complimenting perspective of knowledge: the question of trust and legitimacy of the management system, which relates to the complementing side of the inefficiency of TACs in regulating F: The issue of compliance to regulations and MCS.

Importantly, the management system for these reasons above became, with respect to legislation, "designed" to allow the industry pressure to be effective. There was never an agreement on a useful specification on management objectives. And there was newer any specification on how to achieve the "general objectives". Consequently, there was no commitment to any specified policy. The legal side was flexible to interpretation - and thus open to external pressure.

Granted the above, "the managers problem" became, in terms of the "institutional dilemma", to obtain recommendations by an "objective" science, which were compatible with the "open system" and which was suitable for a system where the focal point was to obtain difficult political agreements. The criterions by which the advices were given changed and I have tried to understand how and why they changed.

Explanation in metaphors: power and frames

Since the word of the Council of Ministers is law the nominal power was theirs. But the Council did not want this power due to a "democratic" recognition of the will of the industry. Each national minister faced the pressure from his or hers national industry. The Commission had the power of proposing, but when proposals were not accepted it was necessary to make new proposals until agreement in the Council was reached. I have indicated that the power was in favour of the CEC in its relation with science. But whereas CEC had the upper hand in its relation with science, it was not so in the relation between CEC and the Council. The Council was in turn subjected to the power of the industry. In this way there was, somewhat metaphorically, a sort of power transmission system working through the two management bodies from the industry to science. The terminal power of the industry would not allow itself to be constrained. When there were some rigid elements in the advice, the power would find a way through the flexibility that surrounded them.

This is why have I used so much space on the initial history, which I think perhaps is the most important part of the history of the CFP. In chapter 4.1 I described how the political climate was such that TACs became "inescapable" and effort regulation "impossible", which I argue can also be related to the initial history. The relative stability became "hard" partly on a reflection on the weakness of the compromise it represented. In this chapter we see a similar story. The initial *framing* of the fishery system was very important because the frame, however initially soft, becomes hardened. The fundamental institutional settings become more and more difficult to change.

Am I contradicting myself? One hand I say that the "frame" is hard, on the other that "the boundaries" were soft. What I mean by "the frame" is the "relative stability" in terms of TACs and the *lack* of specified objectives. The frame is the core of the system, and it is an essential *property* of that core, that it was (and still is) *hollow*: objectives and decision rules were not specified. The form of this frame was, at least partly, a result of pressure form the industry. The industry pressure *required* soft boundaries between science and management and influenced how boundaries within it moved - and the frame *allowed* the boundaries to be flexible.

On the other hand the formal power was, as mentioned, always in hand of the "managers" (i.e. the Council). And it was the managers who *decided* on the "frame". Counterfactually, the frame could thus have been different; the core could have been "hard" (say specified objectives and "strict" decision rules). In that case the boundaries between science and management would perhaps had been more clear and stable and the industry would consequently not have been *allowed* to be a (strong) actor.

The above is the "chicken and hen" problem and I cannot "solve" it - except by pointing out that the only possible explanations of such questions are *evolutionary* explanations: The industry as an actor must have *co-evolved* with a fishery system that allowed it to *act*. And evolutionary explanations are in fact exactly historic explanations, such as the one I have tried to provide.

I have in some places indicated how politics, seemingly, sneaked into science. How science perhaps "sometimes sneaked into politics" and sometimes *did* "sneak into politics" I have also indicated in several places in this and in the former chapters, and I think there is no need to elaborate this point. For I am trying to explain the cod crisis and I think that when "science sneaked into politics" it was generally in order to *protect* the stock (or to build it up towards rational exploitation) - in one way or the other. When politics sneaked into science, it was on the other hand led by the understandable, but unfortunate, requirement of the industry: to provide a short term socio-economic relief at inescapable cost of the long-term prospects.

How the three questions meet

In the last 3 chapters I have tried to answer the three questions that seemed to be focal with respect to explanation of the cod crisis as a management problem: Why was the TAC system not replaced by an effort regulation system? Why was the stock overestimated? Why was the *maximum* recommendable TAC nearly always chosen? I indicated that the questions were perhaps connected.

When I above said that the management was not committed to any specific policy that is not entirely right. For the CEC did apparently commit itself to the policy of not letting F increase. Here is then one way the three questions meet. The *TACs* that were agreed did not allow increases in F (on the contrary the catch options predicted considerable reductions) but the regulatory inefficiency of the *TAC-system* did. Further, the *assessments* were such that the actual increases in F were often not recognised until later. But "later" implicitly meant that it was another (and higher) F that the managers were committed to not allowing to increase. This is a sort of ratchet mechanism somewhat similar to the one mentioned by Holden. I will provide an indication of this. On its meeting in the fall of 1992 ACFM discussed how to present its advice:

For the North Sea overview section, it was suggested that the base year for the recommendation on roundfish should be changed to a more recent year, as relating to 1989 was becoming progressively more difficult. However, this suggestion was rejected for fear that the recommendation might lose its impact if it was seen to be have been changed, even if the percentage was modified as to equate it to the original recommendation (CM A:2, 1993: 18).

At the same time the example indicates, as suggested above, that when science sneaks into politics, it is in order to protect the stock.

Rituals and institutional cramps

In the last part of chapter 2 it was mentioned that there was a large discrepancy between the intended reductions in F, which was the basis of the recommendation, and the Fs that, retrospectively, resulted from TACs corresponding to those recommendations. The example, that F in 1990 "should have been" less than 25% of the F in 1985 was given. If you continue the argument further back the discrepancy obviously is larger, although ACFM (and before it the Liaison Committee) prior to 1985 generally recommended in terms of smaller reductions before 1985 (i.e. 10% reductions).

This kind of recommending something you know will not come through year after year could be called a "ritual"; an act that initially was thought to be meaningful but in time lost its original meaning. The lack of significance of the act is recognised, but the habit continues because the system "expects" it. The system even *demands* the habit to continue.

This was why ACFM did not want to give TAC recommendations between 1990 and 1995. ACFM was tired of the ritual. Therefore ACFM indicated that the TAC recommendation *was* really a ritual and that it did not want to continue it. However, ACFM still provided the catch options so the managers at least could continue *their* part of the ritual. And the managers did that. From the catch options and the chosen TACs it is possible to see that they intended to reduce F by 20% in 1991, 10% in 1992, 20% in 1993, 30% in 1994, and 25% in 1996. By this they would have reduced F to 30% of its 1990 value in 1996.

In 1996 ACFM perhaps was kindly asked to continue the old ritual[41], at least it did so. Did ACFM suddenly *believe* in the TAC system? I do not think so - unless its continued warnings of the inefficiency of TACs were also rituals. The reductions recommended by ACFM were 20% for 1997 and 1998, 10% for 1999 and 20% for 2000. This implies that the F in 2000 "should" have been 46% of its 1996 value. Or *from 1985 to 2000 the recommendations and TACs should have achieved to reduce F to about **3%** of its 1984 value*. But the F continued to increase. That is why you could say that the catch options and (most of) the recommendations and adoption of TAC pertain to "a ritual".

Sorry about that, I do not intend to offend anyone. The point is actually quite contrary: People sit in different positions of the system and *do* their best although they *know* that it is wrong. But actually it is slightly worse: Individuals mostly only know that *their* part of the system is wrong. But the system needs that you do your part and therefore you do it. You know that it is impossible to change anything anyway. And then it is indeed impossible.

This is what could be called an "institutional cramp". A cramp is, seen from the perspective of the muscle, a painful *local* tension. And how should the nerve in this muscle be able to do anything in order to ease the tension? The nerve *has* to transmit the signal; the tension in the muscle *has* to continue. The nerve just hopes that the cramp in its location will disappear, and it fortunately does not know of the *myosis* that has spread to the other muscles too.

So seen from every local part of the system, the ritual must continue. But it happens to be an expensive ritual, for all parts of the system costs money; the science, the managing, the subsidies and the economic losses that the industry experiences from using far too much effort to catch far too little fish.

[41] This is guesswork. A requests from The EC Directorate General for Fisheries was put in the 1996 ACFM report it goes:

The Commission is fully satisfied with the way the new form of advice is being discussed at present. However, I would like to stress the importance of receiving scientific advice in usefull terms, taking into account the existing management tools and the constraints imposed by the current state of the art of fisheries in the Union. Our officers are willing to collaborate in that regard (CRR 221, 1996: 8).

Chapter 5: Conclusions, recommendations and comments

5.1 Conclusions

The cod crisis can be seen as a management problem subjected to an unfortunate coincidence of over-capacity and recruitment decline. The perspective of scientific knowledge - with respect to the quality and use of this knowledge in the management system - provides a step forward towards an explanation of the crisis.

Recommendations for maximum recommendable TACs were generally followed by the management institutions. However, the TACs were/are recommended and decided on basis of catch forecasts, which were/are *completely inadequate* with respect to the specified intention of F reduction.

It is indicated that the discrepancy between "predicted F" and F as seen by the latest assessment relates to two main issues: The incapability of TACs to regulate F and, since 1995, consistent over-estimations of the stock and associated under-estimations of F.

The above findings led to an investigation of why the inappropriate management measure of TACs were continued in spite of scientific warnings and recommendations of an alternative effort regulation system. Moreover, the above led to an investigation of possible reasons of the over-assessments. The third but related question that was investigated was why management bodies generally decided on the TAC, which was "maximally recommendable".

The over-assessments seemed to be best explained in technical terms relating to biased data for tuning of the VPA assessment model and to the tuning procedures. Yet, the assessment problems were indicated to also relate to issues of a "social nature". For example assessment quality relates to the question of allocation resources for assessment science. Further, the reliability of assessments relates to management since heavily exploited stocks are difficult to assess accurately, and since the regulatory inefficiency of TACs is associated with a deterioration of data. Moreover, assessment science involves "social" questions with respect to a possible trade of between bias and precision of assessment models, and with respect to an adequate presentation of the uncertainty of the assessments.

Why TACs were not replaced by effort regulation was related to the problem of resource allocation within CFP. Long and difficult negotiations resulted in the principle of "relative stability", by which every member state (and Norway) was guaranteed a fixed percentage of the TAC of each stock. The principle of relative stability constitutes a fundamental part of the CFP. The TAC system could not be thought to be replaced by an effort regulation system since there, technically, would, be no straightforward way of translating the resource allocation given by the relative stability into an effort regulation system. On the political level the member state would be reluctant to gamble about its resource share, and the same is perhaps the case on the level of the fisherman, who belongs to a certain fleet-segment. Further, the effort regulation system would be expected to result in short-term losses since it would be a more effective way of regulating, which consequently could reinforce a possible industry resistance to effort regulation.

The question of why the "maximum recommendable" generally was implemented as the TAC was explored as a two-sided problem. The "maximum" was chosen in order to meet short-term needs of the industry, and it is indicated that the industry achieved this by lobbying on those national representatives who together constitute the Council of Ministers.

The question of what was "recommendable" is complicated. The management system was "open" in that there were no specified objectives or specifications of, for example, decision rules with respect to possible scientific advices. This made the management system vulnerable to pressure industry pressure - the industry was allowed to be an *actor*. But the pressure also had to affect the scientific recommendation since the management institution generally was constrained by the "maximum recommendable". Hence, normative scientific advices were a constraint to the pressure for higher TACs - the normative "maximum recommendable" was not enough.

The pressure by the industry was, therefore, expressed in the request by management bodies for flexible advices. The concept of catch options was suitable for granting this flexibility. Yet, facing what could be called the managers "institutional dilemma", management bodies still needed the advice to be a scientific or objective recommendation in order to provide scientific support for a policy. The advices became explorative, which was compatible with the lack of a specified policy. However, predictions were not sufficient to provide political support. The role of the advisors hence developed from one of the hypothetical imperative: "If you want this (higher and more stable catches etc.) then do that (mesh size increases, lower F)", to one of categorical imperative: "Do not let SSB be lower than this". This was a change from "management by where you want to be" to "management by where you don't want to be". The change in advice was suitable for the management arena in that it provided scientific support for the policy combined with the maximal flexibility, which was required for management by an unspecified policy, subjected to intensive pressure.

The categorical limits set by biologists in form of "safe biological limits" now became the next constraint and these accordingly had to be flexible. I have indicated the flexibility of these limits by exposing their changing and mutually contradictory nature. However, when the biological limits were quantified, it would be politically impossible reject these limits as "objective" without eliminating the scientific support for a policy. When the stock was below a quantified biological limit, the form of advice, however, became flexible. Further, it is indicated how the nominal form of advice was not always followed.

The policy of recommendations was thus flexible enough to be sensitive to "political forces", whether it was through conscious processes or not. Hence, it is in general indicated that wherever there is interpretative flexibility, there is also sensitivity to social forces. And interpretative flexibility can originate both in technical or scientific domain and in the way advices are provided. This flexibility, together with intensive political pressure, is not biologically stable; the probable result was the current cod crisis.

To put it pointy (but intended humourous) the cod crisis could therefore be explained simply by saying:

Oh yes, they *did* fish too much. But to *allow* it with extremely expensive science and management proved to be very complicated!

5.2 Recommendations

The above conclusion is in one way written too strong, but in another it is not strong enough! Let me explain. This work has been subjected to a severe time constraint (which probably is quite evident in several places). I started 6 months ago from scratch: I knew absolutely nothing of the North Sea fisheries, which I consider to have been an advantage. As an external you do not at the outset accept the order of things as "normal". You look at the system and try to discover its logic: How does it work? And you compare this to how it is "nominally" said to work. If there are some conflicts you try to understand them and so on.

On the other hand my initial ignorance of the system later became a hindrance. For to understand the precise *mechanics* of such a fishery system is very demanding. When the mechanic says: "the problem is probably in the electrical system", that information is only useful when is followed up by a further analysis; i.e. "the disconnect is due to this plug". And I admit that I perhaps did not succeed in that respect. But I think I have at least managed to open the hood of the car, and also to point at some parts of the engine that seem to be dysfunctional.

What is needed is a precise diagnosis of the fishery system and there is consequently a need for further and more comprehensive studies.

Recognition of problems

I do not intend underestimate the difficulties involved, but it is almost a logical necessity, that the first step towards solving problems is to recognise them.

With respect to science it must be recognised that there is interpretative flexibility, and it must be recognised also that social forces *can* exploit this interpretative flexibility.

Management bodies must recognise that the TACs system does not work under the present conditions; i.e. the "ritual" must be recognised as such. Further, it must be recognised *exactly* why effort regulation is "impossible". What can be done in order to make it possible? After all it is hard to conceive a system more costly society and stakeholders (especially the industry) than the present system.

Fishermen must recognise that stocks can be, and often *are* in trouble even when they sometimes have high *catch rates*. Further, fishermen must interact with management and scientist in a more constructive way than in order to argue for higher TACs (and I know that the latter is not entirely fair).

It must consequently be recognised that a constructive dialogue is needed between these three parts. It must be recognised that planning of and implementing steps towards achieving objectives is needed.

Institutional tradeoffs

One aspect of the "institutional dilemma" was the "proximity" of science to management. The institutional setting was in the present case such that you would expect the basic scientific work to be independent of management or politics. The latter was its advantage compared to the case of the cod stocks of the Grand Banks. However, *both* cases suffered from a science, which was not adequately committed to its institutional mandate due to some unfavourable incentive structures. The latter could indicate that the commitment of science to its mandate is at least somewhat independent of the proximity of the science to the management institution.

The latter is fortunate in that the present work generally points towards saying that separation of political or socio-economic concerns from the process of preparing scientific advice is desirable in so far it is possible. Moreover, in so far it is not possible to maintain this separation this must be recognised. If the above is correct there is no necessary trade-off and it should be possible to commit science to its institutional mandate without affecting the ideal of its political independence.

In the present case the unfortunate coincidence of over-capacity and recruitment decline resulted in an socio-economic pressure, which both the institutions of management and science were unable to resist. The consequences did least of all benefit the industry. As a topic for further research it would perhaps be fruitful to examine the nature of the relation between management and science, and the tradeoffs involved in this relation. This could be done through a comparative study of actual cases. I will briefly suggest a possible way to examine properties of this trade-off.

Comparison of fishery systems		Scientific advice process	
		"Hard"	"Soft"
Decision making Process	"Hard"	Atlantic Bluefish	Grand Bank Cod
	"Soft"	"Ideal system"	CFP Cod

Figure 16. Comparison of fishery systems by two parameters: Advisory process and decision making process.

Wilson and Degnbol (2002) described the case of Atlantic Bluefish in the US. They argued that science in this case was hindered by its legal mandate to express what it felt was the present situation of the stock. The legal mandate of science required it to express the assessment in terms of standard assessment models, which would pass peer review. The latter, however, barred scientists from expressing their judgement that the stock was not heavily exploited, but had instead become largely inaccessible to the fishery due to a change in the migration pattern of the fish. Consequently, the "hard" advisory process and the "hard" decision making process resulted in, perhaps, unnecessary restrictive management measures.[42]

I have already introduced the example of the Grand Bank cod, as interpreted by Finlayson. The decision making process was relatively clear in this case: It was the F_{01} strategy. However, socio-economic pressure, facilitated by the institutional location of the science and enhanced by its commitment to politics, resulted (according to Finlayson) in a consistent exploitation of the interpretative flexibility. In other words science was primarily soft due to its institutional location.

The present case is different. It was an "open system". There were no specified objectives or commitment to them in form of decision rules - and the advisory process was, so to speak, forced open by socio-economic pressure. Or you could perhaps say that there was a mutually reinforcing interaction between the softness of the decision process and the advisory process. The open system was sensitive to socio-economic pressure - and did perhaps exactly evolve in order to maintain that property because of the pressure. Therefore, the flexibility associated with a soft system was not used

[42] There *may* be an alternative interpretation: Perhaps science *was* sensitive or soft towards socio-economic pressure, but the "hard" rules for the advisory process barred this softness from affecting the advice (conversation with Petter Holm, May 2003). In that case the hard rules "saved" the fishery from ending in a situation similar to the Grand Bank cod example. The question is thus if the legal mandates "distorted" science or if they kept science from being distorted.

constructively as it may be possible in an "ideal system". In stead strong socio-economic pressures exploited the flexibility leading to depletion of the stock.

The likely trade of between a "hard" system and a "soft" system, is that the soft system is "adaptive" and *can* thus be more efficient than, on the other hand a "hard system", which is likely to be more robust towards socio-economic pressures, but which may be inefficient. How to balance with respect to this trade-off is one of the major challenges in designing and planning a fishery system. I suggest that one external parameter, which should be taking into consideration, is the extent of expected socio-economic pressure. A tough situation requires tough decisions. And tough decisions are, from experience, unlikely to be taken unless there is a "hard" framework for taking that decision. Without a rigid framework no manager or scientist would be able to stand firm on their ground facing intensive socio-economic pressure.[43]

The "mature system" is the ideal system, where the scientific advisory process is hard but, the management process is allowed to be flexible, which allows for more efficient management towards agreed objectives. The necessary condition for the mature system is that the possible "distorting" socio-economic pressures is not strong enough to affect the management significantly. This ideal system of course requires preconditions, which were *not* met at the onset of the CFP (i.e. an "adequate" relation between available resources and catching capacity). The latter perhaps reveals what you could call the tragedy of management: When thing are good you cannot implement management measures "because there is no need to", when things are bad you cannot implement the measures because the industry already has problems. A mature system could be based on joint participation of stakeholders in the management process (i.e. co-management), but the necessary condition is that stakeholders are willing to agree on a common objective and to accept means to achieve it.

The "mature system" is, however, likely to be a utopia for fisheries management. It *may* even be an unstable system since the catchability always increases - and so does the catching-capability of the fleet. Basically this will generate

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[43] Holden argued, that managers in respecting the *democratic* process *should* respond to pressure. I do not agree. First, the democratic process is (should be) mainly working through election of political representatives. Politicians are not exclusively elected by fishermen. Secondly, result of this

an increasing pressure for higher catches. Further, a well-regulated fishery generates high catch-rates and therefore strong incentives to fish more. One possible solution is that of the economists: To attempt to internalise the externalities induced by fishing - for example by ITQs. Well-known problems related to this suggestion are the consequences of ITQs with respect to considerations of equity and social stability. However, it is, with respect to the question of managing the stock "rationally", notoriously difficult to achieve a state of perfect property rights in fisheries, so the above problems may still apply to some degree.

The above suggest that it may be preferable to design a combined portfolio of hard and soft elements. There is may be a need to safeguard any system, including the "mature" system. One way is by the Precautionary Approach. For example, it is conceivable to design a "hard", pre-programmed decision system, which applies whenever the stock is on the wrong side of the PA reference points. Once the stock is on the right side of the B_{pa} and the F_{pa}, more flexibility or softness is allowed. Actually, this was the idea of the advice-policy of ACFM as early as when the catch options were introduced in 1981. When the stock was outside safe biological limits, there would be no options. On the other hand managers would have flexibility when they were inside SBL. The idea carried on and was developed with the MBAL concept and the PA.

The problem was in the present case that when you were on the good side on the limit, there was no objective to "attract" the stock level to stay there. And when you, on the other hand, were on the wrong side, the limits and rules for advising and managing were not rigid enough to provide protection. Consequently, what is needed are the objectives (target reference points), rigid biological limits and rigid procedures for advising and rigid decision rules to apply for the "below limit" situation. The rigidity of the lasts two elements could, for example, be obtained by extended independent peer review processes.

Quite independent of the above trade-off between hard and soft management is the question of the proper *scale* of management. Ideally, the scale of management should mach the scale of the resource - for example in this case by a North Sea council. This is likely to facilitate planning of objectives by co-operation of the different

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"democratic process" similar to the present serves no one (i.e. a somewhat technocratic position). But this is another discussion.

stakeholders. Further, decentralisation to a "proper" scale is likely to facilitate commitment to an agreed policy, and the division of responsibility would be expected to be clearer. Finally, the proper planning of data collection for assessment of a certain fishery could be expected to be enhanced by decentralisation, provided that the regional council is granted the necessary means and responsibility.

Where the advantage of the relation between science and management was the relative institutional independence of the latter to the former, the drawback was, as was often pointed out by the managers, a slow and not always efficient communication. Here is therefore another possible trade-off: Facilitated communication at the cost at scientific independence. I suggest that the severity of this trade-off may be related to the scale of management. But an analysis of these questions would require comparative studies of fishery system cases.

Final credo

Believe it or not: I have "struggled to be objective". But I will agree that my information is not complete and so is my analysis of the information I have. Therefore my "answer" is far from the complete too. Further, my answer, to some degree, must be "conditioned" by my personal idiosyncrasies. But that does not necessarily make it "wrong". And perhaps my strength could be that my biases might be somewhat averaged since I personally have tasted a bit of biology, social science and of fishery. I have not "tasted" the management perspective, so you could argue for a bias here. But I do recognise the difficult position of the manager: being subjected to pressure from all sides and having responsibility, and being forces to take impossible decisions. My intention has exactly been to explain why the managers position is that difficult, and what we perhaps could do to improve this. Remember that I do not intend to criticise any *persons* but a *system* - it is the system we can and need to revise.

The near future of management of the demersal species in the North Sea is extremely important. The CEC has proposed a recovery plan for the cod stock from which the following quote is taken:

Control of fishing effort is a central pillar in this recovery plan. Experience has shown - and scientific advice has confirmed - that TACs and quotas on the one hand and technical measures on the other are not sufficient to regulate fishing mortality, particularly when, as at present,

fishing capacity is too large for the available fish resources. Furthermore, in mixed fisheries, as in cod fisheries, several species are caught together as fishing continues until all the TACs for all the species concerned have been caught. In the process the low TACs for some species such as cod are overshot. This is one of the main reasons why scientists have long been advising limits on fishing effort (CEC, 2003).

It is needless to add that I hope the Council will adopt the recovery plan and that there will be political will to support the industry with eventual short-term losses, which hopefully will be less necessary with time.

Now it is up to the Council of Ministers[44].

[44] With respect to my thesis I consider this a win-win situation. If the Council adopts the recovery plan, which implies that effort regulation really is urgently needed and the Council (backed against the wall) has to agree on continued effort regulations. If the Council will not adopt the proposal (or they dilute it sufficiently to be without significant effect), I am right because I have correctly described the barriers to effort regulation. And if you consequently claim me to be unscientific because of a lack of falsifiability, I will be delighted to falsify the principle of falsification.

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CRR 173, 1990: "Report of ACFM 1990"
CRR 179, 1991: "Report of ACFM 1991"
CRR 193, 1992: "Report of ACFM 1992"
CRR 196, 1993: "Report of ACFM 1993"
CRR 210, 1994: "Report of ACFM 1994"
CRR 214, 1995: "Report of ACFM 1995"
CRR 221, 1996: "Report of ACFM 1996"
CRR 223, 1997: "Report of ACFM 1997"
CRR 227, 1999: "Report of the 10th Dialogue Meeting, 1995"
CRR 228, 1999: "Report of the 11th Dialogue Meeting, January 26-27, 1999"
CRR 229, 1998: "Report of ACFM 1998"
CRR 236, 1999: "Report of ACFM 1999"
CRR 242, 2000: "Report of ACFM 2000"
CRR 246, 2001: "Report of ACFM 2001"

ACFM 2002:
(not yet printed: http://www.ices.dk/committe/acfm/comwork/report/2002/oct/cod-347d.pdf)

ICES Council Meeting documents

CM 1976/F:7: "Comment of the Concepts of $F_{0.1}$ and Stock/Recruitment relationship in relation to fisheries management" (Daan, N.).

CM 1977/F:8: "Report of the North Sea Roundfish Working Group."

CM 1979/G:7: "Report of the North Sea Roundfish Working Group."

CM A:5, 1987 : "ACFM minutes May 5-14, 1987."

C.M. 1984/Assess:10: "Report of the North Sea Roundfish Working Group."

CM 1980/G:8: "Report of the North Sea Roundfish Working Group."

CM 1981/G:3: "Report of the North Sea Roundfish Working Group."

CM 1982/Assess:21: "From Normative to Explorative Advice on Fishery Management" (Hoydal, K.).

CM 1986/Assess:9: "Report of the ad hoc Multi Species Assessment Working Group."

CM 1987/Assess:15: "Report of the North Sea Roundfish Working Group."

C.M., 1991/E:7: "Eutrophication of the North Sea continental zone, A Blessing in disguise" (Boddecke, R. and Hagel, P.).

CM A:2, 1993: "Minutes of ACFM Meeting, October 27 - November 4, 1992"

C.M., 1993/H:19: "The use of the MBAL concept in management advice", Pelagic Fish Committee Sess. P. (Corten, A).

CM 1994/Assess:6: "Report of the Working Group on the Assessment of Demersal Stocks in the North Sea and Skagerrak."

CM 1997/A:2: "Minutes of ACFM Meeting, October 24 - November 1, 1992"

CM 1996/Assess:6: "Report of the Working Group on the Assessment of Demersal Stocks in the North Sea and Skagerrak."

CM 1997/Assess:7: "Study Group on the Precautionary Approach to Fisheries Management."

CM 1999/ACFM:8: Report of the Working Group on the Assessment of Demersal Stocks in the North Sea and Skagerrak.

C.M. 1999/R:04: "Theme session: Fishing Capacity, Effort and mortality": "Evaluating ICES catch forecasts: the realationships between implied and realised fishing mortality" (Beek,V. And Pastoors, M.A.)

CM 1999 A:2: "Minutes of ACFM meeting, 26-29/10 - 4/11 - 1999".

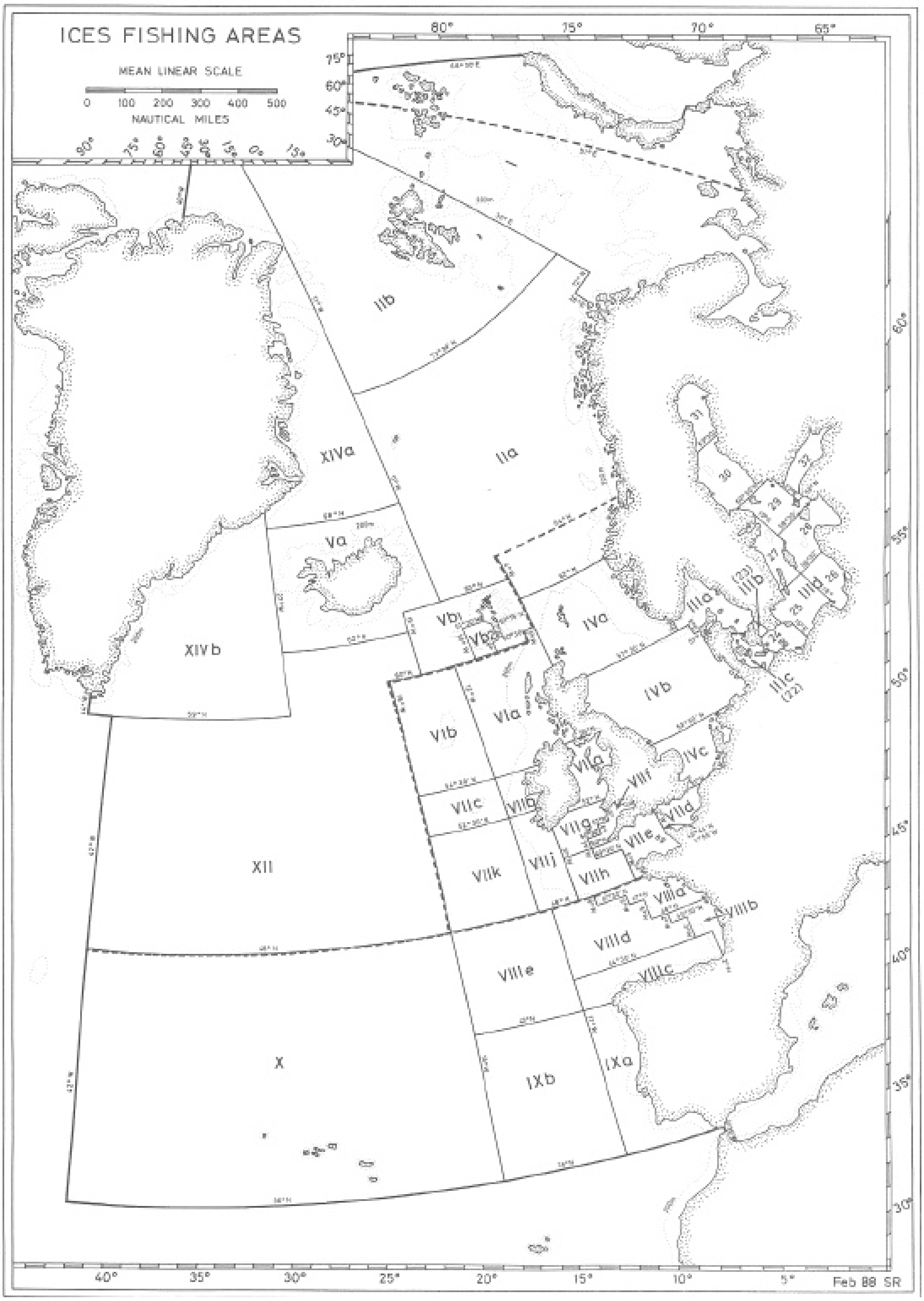
CM 2000/A:2/ACFM:00A: "Minutes of ACFM Meeting, 26/10-4/11-1999".

CM 2000/ACFM:7: "Report of the Working Group on the Assessment of Demersal Stocks in the North Sea and Skagerrak", (11-20/10-1991).

CM 2000/D:02: "Report of the Working Group on Fishery Systems".

Appendices

Appendix 1: ICES areas



Appendix 2

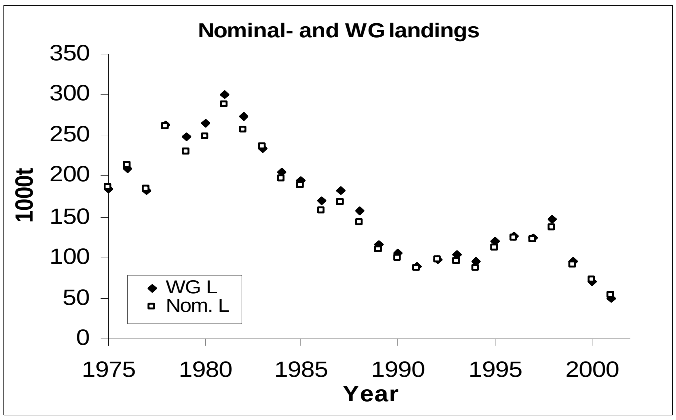


Figure Ia. Nominal landings and Working Group landings. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). Data and sources: Table I, appendix 3.

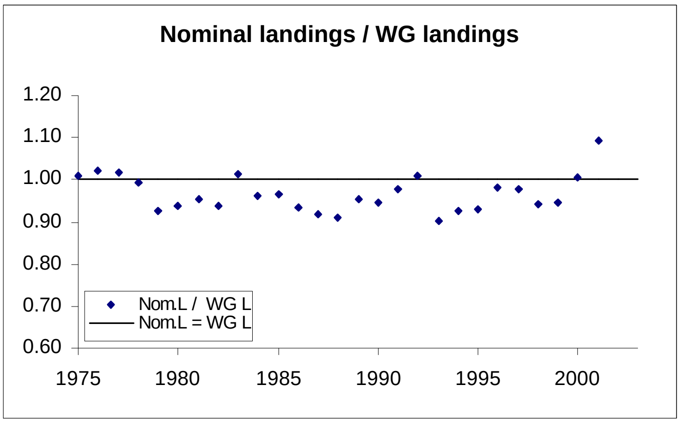


Figure Ib. Ratio of Nominal landings to the Working Group landings. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). The avarage ratio for the period is 0.966. Data and sources: Table I, appendix 3.

Appendix 3

Table I

Year	TAC_{rec}	TAC	Nom. L	WG L	TAC_{rec} / TAC	WG L /TAC	TAC_{rec} / WG L	Nom. L / WG L
1975	230	236	186	185	0.97	0.78	0.80	1.01
1976	210	236	213	209	0.89	0.89	1.00	1.02
1977	220	220	185	182	1.00	0.83	0.83	1.02
1978	220	236	261	263	0.93	1.11	1.20	0.99
1979	247	247	231	249	1.00	1.01	1.01	0.93
1980	200	200	249	265	1.00	1.33	1.33	0.94
1981	220	220	287	301	1.00	1.37	1.37	0.95
1982	235	235	256	273	1.00	1.16	1.16	0.94
1983	220	240	237	234	0.92	0.98	1.06	1.01
1984	215	215	197	205	1.00	0.95	0.95	0.96
1985	259	250	188	195	1.04	0.78	0.75	0.97
1986	130	170	158	169	0.76	0.99	1.30	0.93
1987	200	175	167	182	1.14	1.04	0.91	0.92
1988	148	160	142	157	0.93	0.98	1.06	0.91
1989	124	124	110	116	1.00	0.93	0.93	0.95
1990	113	105	99	105	1.08	1.00	0.93	0.95
1991		100	87	89		0.89		0.98
1992		100	98	97		0.97		1.01
1993		101	94	105		1.04		0.90
1994		102	87	95		0.93		0.93
1995		120	112	120		1.00		0.93
1996	141	130	124	126	1.08	0.97	0.90	0.98
1997	135	115	121	124	1.17	1.08	0.92	0.98
1998	153	140	137	146	1.09	1.04	0.95	0.94
1999	125	132	91	96	0.94	0.73	0.77	0.95
2000	79	81	72	71	0.98	0.87	0.89	1.02
2001	0	45	54	50	0	1.10		1.09
2002	0	49.3			0			
2003	0	27.3			0			
Averages								
Excluding 1991-1995 and 2001-2003	182	184	177	183	1.00	0.98	1.04	0.97[1]
1991-1995		105	96	101		0.96		

Table I) Maximum recommendable TACs, Agreed TACs and landings as used by the Working Group and some ratios between these. Since no TACs was recommended for the years 1991- 1995, these years are treated separately with regard to averages. Further the last three years are excluded in the averages, since the recommendation of TAC = 0 can be thought to be qualitatively different and because of reasons explained in the text.

Sources:
Data on landings: 1975-1983: CRR 214:1995, 1984-1995: CM ACFM:7, 2000 and 1996-2001: ACFM, 2002 (the latest available).
Data on recommended TAC: As explained in text.
Data on agreed TAC: Any source can be used since these do not change. For example the latest CRR report containing the year in question.

1) The average ratio of nominal landings to working group landings is for the whole series.

Appendix 4:

Table II

Year	$SSB_{hist.}$(t)	Source	SSB_{2002} (t)	$SSB_{hist.}$ / SSB_{2002}
1978	164.000	1979: CM: G7	141.640	1.16
1979	154.500	1979: CM: G7	146.010	1.06
1980	271.000	1981: CRR 114	161.660	1.68
1981	280.000	1981: CRR 114	173.980	1.61
1982	239.000	1982: CRR 119	169.080	1.41
1983	255.000	1983: CRR 128	137.760	1.85
1984	120.000	1984: CRR 131	118.590	1.01
1985	92.000	1985: CRR 137	112.180	0.82
1986	78.000	1986: CRR 146	101.520	0.77
1987	110.000	1987: CRR 153	93.080	1.18
1988	96.000	1988: CRR 161	87.680	1.09
1989	91.000	1989: CRR 168	80.530	1.13
1990	87.000	1990: CRR 171	69.370	1.25
1991	64.000	1991: CRR 179	63.210	1.01
1992	51.000	1992: CRR 193	61.240	0.83
1993	58.000	1993: CRR 196	57.850	1.00
1994	63.000	1994: CRR 210	57.600	1.09
1995	78.000	1995: CRR 214	63.070	1.24
1996	103.000	1996: CRR 221	76.250	1.35
1997	160.400	1997: CRR 223	79.740	2.01
1998	136.220	1998: CRR 229	70.150	1.94
1999	128.080	1999: CRR 236	56.900	2.25
2000	66.710	2000: CRR 242	41.110	1.62
2001	54.700	2001: CRR 246	30.280	1.81
2002	37.600	ACFM: 2002	37.600	1.00

Table II. Historical SSB estimates (as assessed in historic year) and recent (2002) SSB estimates. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). The 2002 assessment was rescaled to cover only the North Sea for the years before 1996.

Rescaling:
The latest (2002) assessment is for the combined area (IV, IIIa and VIId), whereas the historical SSB estimates prior to 1996 were for the North Sea only. Since there was not a separate SSB estimate for IIIa and VIId these could not be added to the North Sea estimate to give an estimate for the combined area for all years. Instead the 2002 was rescaled as to cover only the North Sea area for the years prior to 1996.

The rescaling was done by use of the latest series of SSB estimates available for the separate areas. This series was from the 1995 ACFM report (CRR 214, 1995), which contains a series of SSB estimates from 1978 to 1995 for the separate areas. When the SSBs from the separate areas are summed, the resulting series is comparable to the 2002 series for the years 1978-1995. Since the average of the combined 1995 series is actually almost identical (by a factor of 1.002), to the average of the 2002 series, there was no need of rescaling the averages. A scaling factor was calculated as the ratio of the average SSB for the North Sea only to the average SSB for the combined area. The scaling factor was 0.8889, which shows that the Skagerrak (IIIa) and Eastern English Channel (VIId) only are of minor importance in that they in average contain about 11% of the spawning stock of the combined area. .

The scaling factor was multiplied to the 2002 SSB estimates from 1978-1995 to give the 2002 assessment for the SSB of North Sea only for these years.

Appendix 5

Table III

Year of assessment	Source
1981	CM 1981, G:8
1982	CM 1982, Assess 8
1983	CM 1983, Assess 18
1984	CM 1984, Assess 10
1985	CM 1985, Assess 9
1986	CM 1986, Assess 16

Table III: Sources of data for figure 8.

Table VI

Age	Proportion Mature
1	0.01
2	0.05
3	0.23
4	0.62
5	0.86
6	1.00
7+	1.00

Table VI: Maturity ogive as used by the Working Group in the 1984 North Sea Cod assessment (CM 1984: assesses 10, Table 17.1).

Table V

Year of assessment	Source
1995	CRR 214,1995
1996	CRR 221,1996
1997	CRR 223,1997
1998	CRR 229,1998
1999	CRR 236,1999
2000	CRR 242,2000
2001	CRR 246,2001
2002	ACFM, 2002

Table V: Sources of data for figure 10.

Appendix 6

Table VI

Year	$F_{hist.}$	F-type	Source	F_{2002}	F_{2002} / $F_{hist.}$
1977	0.88	(2-8)	CRR:85, 1978	0.71	0.80
1978	0.74	(2-8)	CRR:93, 1979	0.81	1.09
1979	0.95	(2-8)	CRR:102, 1980	0.69	0.72
1980	0.72	(2-8)	CRR:114, 1981	0.79	1.09
1981	0.57	(3-8)	CRR:114, 1981	0.77	1.35
1982	1.01	(3-8)	CRR:119, 1982	0.90	0.89
1983	0.82	(3-8)	CRR:128, 1983	0.91	1.10
1984	0.91	(3-8)	CRR: 131, 1984	0.86	0.94
1985	0.85	(3-8)	CRR:137, 1985	0.83	0.98
1986	1.00	(3-8)	CRR:146, 1986	0.86	0.86
1987	0.86	(3-8)	CRR:161, 1988	0.91	1.06
1988	0.80	(2-8)	CRR:168, 1989	0.89	1.11
1989	0.89	(2-8)	1990: CRR 171	0.99	1.11
1990	0.79	(2-8)	1991: CRR 179	0.72	0.91
1991	0.93	(2-8)	1992: CRR 193	0.93	1.00
1992	0.86	(2-8)	1993: CRR 196	0.86	1.00
1993	0.94	(2-8)	1994: CRR 210	0.91	0.97
1994	0.85	(2-8)	1995: CRR 214	0.85	1.00
1995	0.81	(2-8)	1996: CRR 221	0.72	0.89
1996	0.63	(2-8)	1997: CRR 223	0.92	1.45
1997	0.63	(2-8)	1998: CRR 229	0.87	1.38
1998	0.59	(2-8)	1999: CRR 236	1.02	1.74
1999	0.90	(2-8)	2000: CRR 242	1.18	1.31
2000	0.83	(2-8)	2001: CRR 246	1.23	1.48
2001	0.91	(2-8)	2002: ACFM	0.91	1.10

Table VI: Historical F estimates (as assessed in historic year) and recent (2002) F estimates. 1975-1995: North Sea only. 1996-2003: North Sea (IV), Skagerrak (IIIa) and Eastern English Channel (VIId). 2002 estimates are from ACFM 2002**.**

Appendix 7

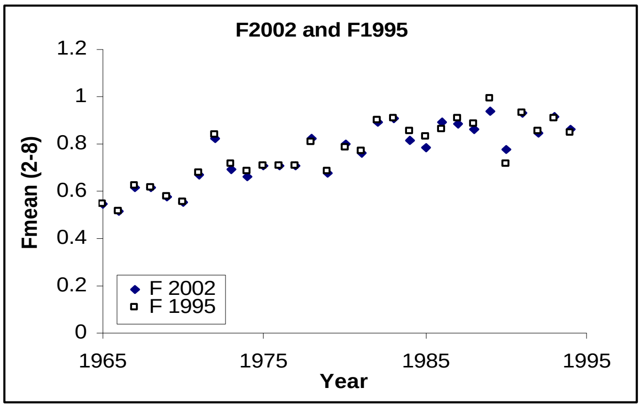


Fig. IIa. Series (1963-1994) of F as assessed in 2002 and 1995 respectively. Sources: ACFM 2002 and CRR 214, 1995.

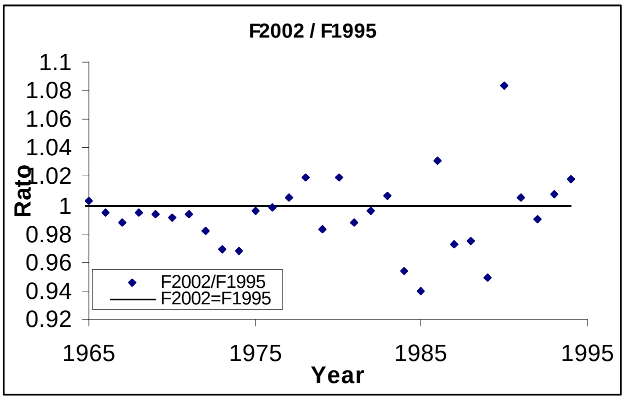


Fig. IIb. Ratio of F as assessed in 2002 to F as assessed in1995 for the years 1963 to 1994. Sources: ACFM 2002 and CRR 214, 1995.

Appendix 8

		F estimates							
		Year of assessment	1996	1997	1998	1999	2000	2001	2002
Year assessed	1995		0.809	0.615	0.653	0.679	0.722	0.724	0.725
	1996			0.633	0.734	0.787	0.914	0.918	0.921
	1997				0.627	0.687	0.841	0.857	0.866
	1998					0.588	0.949	0.992	1.025
	1999						0.900	1.064	1.177
	2000							0.832	1.232
	2001							0.830	0.912

Table VIIa: Data for figure 12.

Year of assessment	Source
1995	CRR 214, 1995
1996	CRR 221, 1996
1997	CRR 223, 1997
1998	CRR 229, 1998
1999	CRR 236, 1999
2000	CRR 242, 2000
2001	CRR 246, 2001
2002	ACFM, 2002

Table VIIb: Sources for figure 12.

Appendix 9

An informal challenge to a theoretical position

Allow me to, informally, challenge the abstract *position* of Finlayson as it is apparent in "Fishing for truth" (Finlayson, 1994). For a more detailed description of the position I will refer to the book itself, however I have indicated the position in footnote number 22. It is this position I want to "challenge" by a small thought example.

In a later paper (McCay and Finlayson, 1995) Finlayson apparently adheres to a much more "realistic" position in that explanatory relevance of *stock changes* are allowed. However, my critique was never meant to be *ad hominem*.

In think there are some elements in "Fishing for truth" that indicate that Finlayson is somewhat ambiguous with respect to his "position". Some of his arguments imply some sort of realism (see footnote 5) whereas his stated theoretical position rejects it. Perhaps this "tension" is apparent in the following quote alone:

Fisheries scientist do not now, and probably newer will know enough about fish and their ecosystem to construct enough facts to support agreement and cooperation. (Finlayson, 1996: 154).

Cods in a bathtub

Imagine that I one day invite Finlayson home for a cup of coffee. In my bathtub I have put 4 cods. I ask him: How many cods are there in the bathtub? If I understand it right Finlayson now is in dilemma. Of course he will probably answer "4". If so, I ask him what "truth is illusive" means to him. He will then have to say that it means that we will never know how many cods there are in the *sea*. I will then ask him to tell what relevant difference with respect to truth being illusive there is between the cods in my bathtub and the cods in the North Sea. I would admit that one such difference is that some cods swim in and out of the North Sea - unlike my bathtub. But then I will ask him if he would agree that there *was* a definite number of cods in the ICES area IV on the 1st of January 2003 at 00.00.00 GMT. If he answers "yes", we will then agree on what "truth is illusive" means: It refers to an epistemological problem - or, consequently, a problem of *insufficient* scientific knowledge. If he answers "no" he would have to qualify some relevant difference that I have not thought of - I think the burden of evidence is then on his side.

On the other hand he could answer to my first question: "I do not know". When I ask him to explain his answer he would perhaps do it in term of epistemology (e.g. "are these *really cods*?" - "or "I see 4, but *senses* are illusive, maybe there are just 3"). Then we also agree as above. Otherwise he would explain in terms of ontology: "I see 4 cods but I do not know if they are *really there*". If so I will admit that it is a completely consistent viewpoint but that it happens to be different from my view. Further I will ask him if he still would not be interested in knowing *how many cods there seems to be to us*. There is then a philosophically interesting theoretical difference but there is no *practical* difference.

This should not be taken as a naive way to defend "common sense ontology" or a correspondence theory of truth or to say that science is really objective. I know of theoretical problems of these positions (e.g. observer dependence, the lack meaning of observer independence, inductive problems, under-determination of language etc.).

My point is simple but different: I do not (at least only) want to challenge Finlayson in terms of abstract theoretical positions but (at least also) I want to challenge him in terms of *cod*.[45]

[45] This is a bit crude - but since the present work is not a philosophical dissertation it will have to make the point. Perhaps this place is not the best place for such a discussion theoretical positions- but I could not resist the temptation.