

**POWER AND THE PRODUCTION OF SCIENCE:  
Assessing Cod Stocks as the Mechanistic Fishery Collapses**

*Peter R. Sinclair*

Memorial University, St. John's, Canada  
peters@mun.ca

*Jahn Petter Johnsen*

University of Tromsø, Norway and Centre for Rural Research, Trondheim,  
Norway  
jahn.johnsen@uit.no

*Paul Ripley*

Memorial University, St. John's, Canada  
pripely@mun.ca

*Abstract* This paper discusses power relations in the production of knowledge claims and the validation of management strategies. The experience of doing stock assessment science and creating management plans for Canada's east coast cod fishery illustrates this general process. We demonstrate that the cyborgization of fisheries-management is limited by its inability to produce power for stabilizing the relations between managers, fishers, technology and fish. Lack of stability forces scientists and managers either to ignore a threat or to intervene by changing their strategy. Consensus is unlikely. Scientists and managers must reconsider reasons for action or lack of it, thus producing a new rationality. Managers attempt to control that reconstruction process in the interests of resolving short-term challenges. Some scientists resist change and protect their earlier positions against new evidence or re-interpretations. The winning rationality has more to do with the power of the claimant than with the quality of reasoning.

## **Introduction**

What is the problem with rational science-based fishery management? Before the 1980s, fisheries, dominated by social and affective relations could be interpreted as organically organised activities (Burns and Stalker 1961; Johnsen *et al.* this issue); fish and fisheries were considered unmanageable. However, by the 1980s fisheries management had become a practical reality based on regulations for access to the fish and increasingly sophisticated technologies to catch them. This is a modernist project. Reason in the form of appeals to the authority of science and efficient management defends, promotes and legitimates something that we can call mechanically organised fisheries (Burns and Stalker 1994 [1961]). Different from the incrementally, bottom-up organised, organic fisheries, the mechanically organised fisheries are planned, structured, formalised and controlled by

external management on the basis of belief in rationality, scientific knowledge and efficiency. Planning, formal organising, technological modernisation and management are intended to stabilize human-nature connections in a form that makes fish and people manageable. Consequently, in the mechanistic fisheries, fish are no longer viewed as natural objects only; they are changed into quantities, numbers and economic values. Instruments like quotas stabilize and mediate the relations that make up the mechanistic organisations.

This 'mechanisation process' is also evident as a technological, organisational, and relational change at the micro level on board fishing vessels. Through the process of mechanisation, they have developed from rather loosely coupled networks of people, fish, gear, boat, and affective community relations with informal and implicit control and feedback mechanisms (Kooiman *et al.* 2005) into integrated harvesting systems or machines, with formal, explicit control and feedback mechanisms (Johnsen 2005). This change can also be observed as a change from fisheries based on local ecological knowledge to fisheries based on more globalised harvesting knowledge (Murray, Neis and Johnsen 2006). During the last decade this process has speeded up and contributed to an even more radical change – what we call cyborgization, a process described by other articles in this issue. Our story, however, is about an earlier stage in this process, where system stabilization was the objective.

Rhetorically, both these harvest machines and the regulatory mechanisms are introduced to create a governance system that will allow for sustainable fisheries. By transforming and integrating the nature-human interaction (*the-system-to-be-governed*) and the political-technical *governing system* into a cybernetically organised governance system called the *TAC machine*, both nature and humans shall be under managerial control (Nielsen and Holm 2007). From the conflict or power view of social relations, the structural change from fisheries based on human relations into mechanistic fisheries is a particular moment in the unfolding of social conflict, and one that carries serious implications for the environment. However, what if the mechanistic fisheries are unstable? Will not the whole management regime be threatened or face collapse? Through a description of the struggle to produce power to define, control, and to stabilize we will answer these questions.

We will show that the attempt to build the mechanistic fisheries has both an ambition and a limitation. The ambition is to control the various actors in the field to produce sustainability. The limitation is the capacity to produce power for stabilizing the relations. This lack of stability forces the scientists and managers either to ignore a threat or to intervene by changing the management strategy. Consensus is unlikely. In both options, scientists and managers reconsider reasons for action or inaction, thus producing a new rationality. Managers attempt to control this reconstruction process in the interest of resolving short-term challenges. Some scientists resist change and protect their earlier positions against new evidence or re-interpretations. In these circumstances, the winning rationality has more to do with the ability to produce power (or to exercise agency) than with the quality of reasoning and evidence (Flyvbjerg 1998). In this paper, we stress the importance of institutional setting for the conduct of science, while not ignoring the possibility of individual agency. The analysis points to the fragility

of knowledge claims in a context of high uncertainty. This suggests the need to be wary of rationality claims and to manage pessimistically (on a precautionary basis) if the core guiding value is long-term environmental continuity.

Our paper has a long history, which itself speaks to the power to control the production of knowledge. In 1994-8, Sinclair was co-investigator in a project called 'Fisher's ecological knowledge, fisheries science and sustainable management'. This was itself integrated into an eco-systems research programme at Memorial University in St. John's. The federal Canadian Department of Fisheries and Oceans (DFO) was a partner in this larger project. Nevertheless, the department banned its scientists from talking to us for two years, with the result that Ripley, Sinclair's research assistant at the time, was able to complete only one interview by 1996. Although we did have permission to interview, by this time the department's employees were highly sensitive to criticism they might receive for public statements that did not clearly support DFO policy, and most refused to agree to interviews.

By 1998, despite Ripley's efforts, we had completed only seventeen of forty-two planned interviews. They covered the personal work histories of the respondents, and their experiences in the DFO during the 1980s. The interviews were semi-structured with leeway to follow the respondent in unanticipated directions. Despite believing that the interviews contained valuable information, Ripley and Sinclair felt so discouraged by the process that they abandoned this research until they joined Johnsen in this project for which these data are now central. Although the interviews are the primary sources for this paper, it also draws on public documents and Sinclair's observations as a member of CAFSAC (the Canadian Atlantic Fisheries Scientific Advisory Committee) in 1991-92 when this committee was responsible for advising the DFO Minister on appropriate catches for all commercial species in the region.<sup>1</sup> He sat on this committee as one of four external members.

## Theoretical Perspective

Power, the capacity to produce effects, is at the core of this analysis. Social relationships are networks of interaction in which actors have power to the extent that they can control the pattern of relationships. This power is a component of all relationships. It is not given, but results from the actions that take place and the relations through which it emerges. Thus 'power is implicated in all social practice, as a logically necessary feature of activity' (Isaac 1989:75). All action requires mobilization of the necessary resources and no actor is completely powerless to control what she or he does. Thus, the possibility of resisting is always present (Foucault 1978).

Consistent with this practical, action-oriented perspective on power, the concept of network highlights connections among nodes, which can be social groups with particular locations in space. Actors in the network will almost certainly be unequal in their powers and in their contributions to the network at any moment. The network as such is a map of connections. The form that the connec-

tions take is open-ended, but subject to specification through the examination of power relationships. Moreover, network relations will change as a result of both external influences and the agency of their components.

This view of power is consistent with actor-network theory (ANT). This theory depicts the attempts to construct actors, relations, and reality through stabilization of networks of relations among heterogeneous material, humans, nature, things, science, politics, and technology (Latour 2005). Actor-networks are constantly emergent linkages among people, wildlife and materials. The network concept allows for uncertainty, chaotic outcomes, positive or network transforming feedbacks, and system complexity. It does not *assume* that systems are homeostatic and resilient, that is, that they tend to reproduce their structural patterns through adaptive adjustments to external inputs.

There is some overlap in our case study with Finlayson's (1994) work on DFO stock assessment and the collapse of Northern cod, but Finlayson attempts to maintain a strong commitment to a radical social constructivism, focussing on DFO scientists as the constructors. In doing so, he actually under-communicates the importance of the reality-in-making and the framing that takes place in institutions, methods, measuring techniques and nature itself, which also contributes to the construction of the DFO scientists. ANT is more concerned about the process of constructing reality than about reality as a stable and constructed object. As in this particular case, our concern is how difficult it was to stabilize the network required to construct mechanistic fisheries. This constructive realism actually accepts the scientists' constructions as performative and as mediators of reality, and this is central to our analysis. This means that the scientists' constructions are not merely representations of the real world, but that they actually both constitute what is perceived as reality and also mediate and communicate the same reality.<sup>2</sup>

Recent sociological writing on fisheries science is somewhat tangential to our interests. For example, Wilson *et al.* (2002) surveyed 349 scientists working in diverse institutional settings in the USA. They recognize that scientists face institutional pressures around defining and answering various questions, but this report reviewed general working practices, experience of job pressure, and their attitudes towards research and careers rather than the politics of conducting their work. However, in their study of scientists working on the Common Fisheries Policy of the European Union, Wilson and Heglund (2005) analyze the experience of doing science within the political context of policy formation and implementation. This context included industry participants and environmental organizations as well as the EU and its member states. Scientists experienced increasing pressure to work in a way that they did not consider science, especially when asked for advice for which existing models and data were inadequate. Those employed in stock assessment were less satisfied than others due to travel demands and reduced opportunities to publish. However, they were not employees of those who received their advice. Our research on a group of scientists employed by the administrative state will demonstrate an even more demanding and disheartening work environment.

Sociological studies of other branches of science include work relevant to our investigation, although the amount of empirical demonstration of power rela-

tionships involved in the production of science is modest.<sup>3</sup> There is evidence that the research process is political as well as technical. Thus, among workers on leading edge projects, such factors as secrecy, isolation and competition within and between scientific research groups influence the pace and outcomes of research (Atkinson *et al.* 1998). More directly relevant to our study is the interconnection between research funders, researchers, and political actors. For example, Fishman (2004) portrays academic scientists as occupying a mediating position in a network that links drug producers and consumers. They contribute actively to market development by promoting awareness of problems and solutions, as well as conducting clinical trials for pharmaceutical companies. The perceived objectivity of the scientists contributes significantly to acceptance of their claims and advice by both consumers and the us Federal Drug Administration, which must approve new products. Yet, these scientists are rewarded financially and professionally, which means that they cannot be considered disinterested in drugs they support.

Political interests can affect the reception and validation of scientific ideas. In a study of the politics of BSE (mad cow disease) in Portugal, Gonçalves (2000) argues that the government rejected and discredited scientific claims that BSE had been found in that country until the disease was recognized in much of Western Europe. In other words, what counts as good science in the eyes of state actors depends on its likely political impact rather than any technical evaluation. In this case, scientists could not establish their definition as the dominant one in public settings, but there is no suggestion that the scientists altered their judgement to fit the political context. However, that clearly is a possibility, one that is evident in Murphy's (2001) examination of scientific evidence concerning nicotine addiction in which she demonstrated that researchers' interpretations corresponded to the interests of their employers, whether pro- or anti-tobacco. Our study will provide a similar link between fisheries science and management.

We now turn to the production of science within the DFO. We will summarize the structure and culture of the social network from which advice and managerial decisions emerged. We leave the various tools and other material components of the actor-networks in the background. However, a complete analysis should recognize that the technological aspects of data collection with both commercial and government vessels, the characteristics of the material ocean environment, and the equipment and physical working environments of the participants all contributed to what took place.

### Science in the DFO

Doing science is an active process, but always conditioned by the institutional context. This context is the prior history of those who assembled resources and inscribed regulations, which then provide access to opportunity and condition what is possible in the present. The period we write about is informative because it is one in which institutions appeared to unravel as the fishery failed to function as managers and scientists had planned. How did scientists contribute to and respond to this situation?

In earlier work, our team discussed the emergence of the cyborg fishery (Johnsen *et al.* 2005). Essentially, we reviewed the attempt of managers, scientists, and fishers to translate the unmanageable into the manageable through the creation and application of rules and technologies. We drew our examples mainly from Norway. For the fisheries of Atlantic Canada, a similar process took place after the extension of fisheries jurisdiction to 200 miles in 1977. By 1987, regulation of the cod fisheries had moved from the Northwest Atlantic Fisheries Organization to the national DFO where stock assessors produced advice and ultimately the minister set quotas that could be caught by those who had valid licences for their boats and gear. In the early 1980s, there was considerable optimism on the east coast about the future of cod and other groundfish, given that the fishery could now be managed and that less controllable foreigners were excluded from all but fisheries outside the extended jurisdiction.

However, the new regime was soon out of control and the unmanageable became evident. By 1992, DFO closed the northern cod fishery and by 1993 all major groundfish stocks were subject to moratoria, which remain in force with some exceptions.<sup>4</sup> We turn to the period from 1985 to 1995 to examine how scientists and fisheries managers constructed their arguments before and after the collapse of the fishery. We will demonstrate that the relative powers of various parties to speak in the name of the DFO outweighed any technical merits in their arguments. Thus, the winning rationality was the product of power.

By the 1980s, many DFO scientists were heavily involved in research related to stock assessment, and when this was not the case, a request for information or advice from management would often require that other research be dropped. Still, even towards the end of the decade, the work expectations might be as vague as to conduct research on a particular group of species, and scientists who could obtain external funding were free to work on their own projects. That said, scientists had to advise on the status of the stocks and how much might reasonably be caught.

CAFSAC was composed of the national and regional science directors and the chairs of the various subcommittees for fish species as well as for marine mammals, oceanography, methods and statistics. In 1991, four external members were appointed. Meetings lasted two to three days during which time subcommittee chairs presented their recommendations and the committee decided on the advice that would be conveyed, ultimately to the minister, about the total allowable catch and research priorities. Prior to CAFSAC's assessment, fisheries scientists across the east coast divisions of the department would meet in subcommittees for the various groups of species, including groundfish. Their objective was to produce a report, following discussion that would reflect a consensus of peers. The groundfish committee would meet for up to two weeks after the most recent survey data from research vessels had been processed in order to generate advice. The chair of the subcommittee wrote this advice.

For cod and other groundfish, scientific advice was based on virtual population analysis. As stated by Sinclair *et al.* (1991:59), '[t]he method basically consists of adding up the catches of a cohort of fish while adjusting for non-fishing or natural mortality ( $m$ ) during the life of the cohort ... An estimate of the surviving

fish in the last year of the time series is required to begin the process'. As fisheries scientists sometimes told us, this model was much better for evaluating past stock status than for forecasting the future. The danger lies in estimating biomass and associated allowable catches that are unwarranted based on actual circumstances, which do not become evident until several years later.<sup>5</sup>

Management may be based on various criteria, such as a judgement of how much fish can be caught while maintaining the current population, or, more often in the 1980s, how much can be caught and still allow the population to grow at some estimated rate. To do this, natural mortality was always estimated at twenty per cent. To that was added a proportion of the stock that might be caught (usually eighteen per cent or the  $F_{0.1}$  level), which would leave an increasing biomass for the following year. In that year, new evidence would be assembled to judge the state of the stock. When there were discrepancies between sources of evidence (commercial versus research estimates) or between expected and 'actual' cod biomass, this might be attributed to various factors such as random survey error, overfishing, natural causes, technological changes and fishers' learning.

Although DFO managers wanted to claim that advice and the data on which it depended reflected a consensus after debate, this consensus would have been difficult to achieve under any circumstances because the department's scientists had quite different backgrounds and spheres of competence. Their cultural repertoires for action (fields in the sense of Bourdieu [1990]), encouraged them to act differently. In a sense, they arrived as parts of different networks that made problematic their insertion into the fisheries management network at DFO. Some had master's degrees, while others had doctorates. Some had final degrees in ecology or biology, but with a non-fisheries specialization, or in oceanography. In addition, technicians might become biologists, a category different from research scientist, but it was possible for these biologists to make careers as science managers. Finally, in the 1980s, the department hired some members who were primarily statisticians or mathematical modellers interested in population dynamics. Some scientists were committed to a purely science-based career, while others were active as managers or aspired to managerial positions.

These background factors, at times exaggerated by personality characteristics, were the source of different work orientations and tension. Biologists and some research scientists tended to emphasize the value of experience at sea. People who stay in the office, according to one respondent, 'don't know the realities of being out there on ships and collecting it [data], and then all the steps that the data are put through before you end up with this nice summary table.' Because most groundfish scientists had spent at least some time at sea, this position tended to separate them from the mathematical modellers, although at least one interviewee had crossed this divide in his career.

Stock assessment was a public service that did little to help the careers of research scientists because they were evaluated on the basis of refereed publications. Stock 'assessors' and 'scientific-oriented' scientists are situated in different networks. They participate in different discourses, and have different orientations and loyalties in the world. Assessments were also time-consuming. It was therefore annoying to some that modellers would request 'their' data and rush to

publish before they could do so. Individual characteristics also played a part. In one instance, the option of co-operative arrangements did not arise because of scientists' reactions to a key participant. Several interviewees noted that he was unpopular and behaved in an arrogant way, which countered his acknowledged ability. An extreme example would be '[n]o one in Newfoundland wanted to work with him ... because he had the personality of a pig.' Such reactions might have had something to do with the general tension of the work place because this individual was able to maintain co-publishing arrangements over a substantial time with other scientists.

### *Creating Consensus*

We are especially interested in the processes of power that influence what claims by scientists get counted as knowledge, given that consensus is rare and that it requires much effort to create. For the 1980s, it is now clear that dissent about the validity of the cod assessments appeared within the DFO. Almost all scientists recognized that their data were imprecise and that in the early years their computing technology was slow and rudimentary. Nevertheless, challenges were strongly resisted.

Whereas some scientists we interviewed accepted that decisions by consensus actually occurred, others were more critical. Thus, we heard that 'the consensus can sometimes be the strongest person's opinion to end up on the paper and doesn't necessarily represent all of the uncertainties that were expressed at the meeting.' Not surprisingly, this person advocated putting multiple interpretations of data into reports when they were present in debate. A manager acknowledged that the review process was 'pretty brutal' because work was torn apart. There was much dispute. A third respondent noted that there was little room for dissent. 'In the end it is consensus. That's it.' This person felt that the DFO wanted a 'monolithic' report. If so, the reason might be that it was useful to refer to a consensus among scientists as a rationalization of any action taken by managers. From a Foucauldian (1989) perspective, these expressions of concern and frustration are examples of resistance to power in a situation in which the institutionalised network of established science in DFO was struggling to transform statements from assumptions into truth (Latour 1987). This required action to ignore, discredit and even to repress dissenting actions and discourse.

One case is particularly instructive. As early as 1982, some scientists expressed reservations about the cod stock assessments approved at the Northwest Atlantic Fisheries Organization, but not to the satisfaction of the majority. 'You're outdone by numbers, numbers around the table' according to one opponent of the established view. He had offered analysis to suggest that overfishing was a serious problem. This scientist also claimed that the commercial data were unreliable and that picking a midpoint between those data and research vessel data when indices diverged was wrong. He felt that in the international forum of 1982-83, the scientists from various countries allowed their national interests or priorities to influence their interpretations. Reflecting on these meetings, another scientist who was present indicated that there was at least some support for this critique, but he observed that the reports never reflected this discussion and called that omission



'really bizarre.' Yet where, management depends on scientifically backed 'truths,' ignoring dissent can be an effective power strategy when the dissenters are part of the management bureaucracy and have no independent avenues of expression.

Although the attribution of rejection based on perceived national interests might appear convincing, this argument cannot be applied to a 1986 presentation of the critique to Canadian researchers only. Once again, the criticism was rejected. Following its release under freedom of information, the paper (Winters 1986) that challenged stock assessment advice became controversial, especially when Hutchings *et al.* (1997a) objected to how the DFO responded to it and what this response implied for how science should be organized.<sup>6</sup> Clearly, there was little support from Canadian scientists for an argument that suggested their judgement and the management decisions based on it were flawed. Another scientist we interviewed claimed that 'it was discussed and rejected by everyone' on scientific grounds because the scientists assembled were a-political. He noted that the critics (Hutchings *et al.*) were not present and did not approach those who knew what took place. Our respondent reiterated that they all felt Winters was wrong, even if later it became evident he might have had the better argument. That is why Winters' point of view did not appear in the assessment document. He failed to convince anyone. 'It wasn't because of management interference ... it was because we squashed it – the peer review scientists in the room, the thirty-five of us.' That is, the institutional network of DFO scientists effectively discredited the dissenter and had the resources to silence him.

Also reflecting on the same event, a senior scientist and manager recollected that it was 'more a lark than a piece of science. It was a shoddy piece of work filled with holes...' This person argued that he (Winters) did have 'some good ideas. It had some fresh perspective. However, the mistakes should have been fixed in order for the rest to be considered seriously.' He added that scientists were aware that recommendations were too high in 1986-87, but evidence (probably the unusually high estimate from the research vessel survey in 1986) also suggested that the stock was increasing and thus that they had time to fix their assessment tools. The only issue was that the rate of growth was slower than expected. An alternative justification of inaction mentioned by another scientist was that no particular percentage reduction in biomass estimates could be justified and so words of caution were used that didn't have a lot of impact in terms of decisions that were ultimately made.

Having started groundfish research at DFO shortly after the 1987 debate, one scientist stated that he had tried to introduce some critical comments. He raised the problem of bias in the commercial catch rates, but 'it didn't fly at all.' This person claimed that other scientists knew he was right, but the person in charge decided to use these data anyway. He thought this might be because the data were needed simply to make the model run. Yet, he stated that the chair honestly believed he was correct. This position was also supported by industry, including the skippers, whose catch rates remained high until there was almost no fish left. Whatever the motivation, consensus had to be manufactured and the consensus that was reported was clearly influenced by the institutional positions of the claimants. Those with more power could write the texts of truth. What

counted in the end as reason and consensus was determined by who controlled the writing of advice and, indirectly, by the section of industry that had the attention of these key DFO members.

### *Alternative Rationalities and Self-Censoring*

Most scientists appeared genuinely concerned with protecting fish stocks from excessive harvesting. However, this did not prevent them from dismissing problems with data that were clearly challenging for the assessments. Evidence for this statement includes the reaction to in-house dissent (as mentioned above), the problem of ignoring under-reported catches, and the practice of providing advice on the status of fish stocks by selecting the mid-point of estimates from research vessels and vessel log records.

Sinclair's observations of the assessment process provide grounds to conclude that scientists were aware that regulations were frequently broken by practices that included dumping of unwanted fish, under-reported landings, mis-reporting of where catches occurred, and an unknown incidence of foreign over-fishing. After hours of listening to reviews of various stocks without mention of these issues, he inquired why the assessment advice did not include even a modest adjustment for various forms of overfishing. The response, which was not challenged by anyone, was that they recognized the problem, but it did not fit their model and the percentage adjustment would, in any case, be a guess. Yet, the consequence was underestimation of fishing mortality by anything from five to twenty-five percent each year. Even the lowest estimate of overfishing would have produced a serious overestimate of the stock biomass after several years. Here we have an example of evidence ignored, apparently because it did not fit with the kind of factors that marine biologists learn to take into account.

Scientists nevertheless took many decisions about quota levels based on what they knew to be unreliable information. Similar to the experience of European Union scientists (Wilson and Hegland 2005), this was a disconcerting experience for them. Basically, the information from commercial vessel catches and research vessel surveys often diverged and sometimes showed opposite trends. Although 1989 was the final year in which commercial data were used in the northern cod assessment, they were part of the process in area 3Ps (off southern Newfoundland) as late as 1992. The CAFSAC approach was to offer biomass and total allowable catch estimates that were at the midpoint between the two indices. In effect, this was a political decision that offered no judgement as to the relative values of the two estimates. It was halfway between one that favoured short-term advantage for harvesters and fish-dependent communities, and one that favoured protection of the stock. A precautionary approach would suggest more emphasis on the lower estimate. Although we have no clear evidence on motivation, we suspect that the mid-point choice was an example of self-censoring because most scientists would have been more comfortable with lower estimates. When Sinclair raised this issue, the only response was that the practice was well established.

Self-censoring, even if reluctantly done, was evident in other circumstances. Thus, Sinclair recalls at least one occasion when a committee member argued strongly that there was no basis to recommend any catch level, but the com-

mittee was informed that they had no choice but to come up with an opinion. More clearly self-censoring was the decision made at committee level simply to rule out presenting a particular position because it would not be politically acceptable to higher level managers and the minister. An interview with a scientist-manager provides similar evidence in that this person was keen to introduce a rough measure to take into account the increased efficiency of the fleet in catching cod, but he found it difficult to argue without conclusive numbers. 'I kept getting slapped down because ... I would say, "Let's build in a ten percent factor".' He was annoyed that Memorial University biologists Keats and Green, who were supported by the Newfoundland Inshore Fisheries Association, could build in rough estimates, but DFO scientists could not – due to the reaction of the industry.

Finally, a well-placed manager, who did not have a fisheries science background, argued that in the DFO, 'the bureaucratic process is completely outweighed by the political power process', and that this was based on the priority of operations or action over policy. Senior scientists, he continued, become political, trying to anticipate an answer that would be acceptable in order 'to protect the system.' Although the motivation is unclear, this self-censoring was common. We now move from control of science by scientists to direct control by management.



### *Managers Control Science*

As problems in the regulation of the modern fishery emerged, fisheries managers engaged in a defensive reaction to which many scientists contributed, as mentioned above. We claim that managers acted primarily to protect themselves rather than to promote effective intervention. Fisheries managers occupy a delicate position between various groups: the governing executive to which they are formally subordinate; scientists and any lower level managers whom they formally command within the limits of their positions; and external groups such as unions, processors, provincial governments, environmental groups, academics, mass media and members of affected general populations. Their actions are open to serious scrutiny. It is not surprising with this constellation of circumstances that most managers would adopt a defensive posture.

Having managed on the grounds of a particular rationality – that the cod population is growing and that fishing is sufficiently controlled to ensure continued growth – managers seldom accepted challenges to that rationality when first put forward. Indeed, such claims appear often to have been ridiculed or not taken seriously. In so far as scientists differed on important aspects of stock assessments, this was often covered up in reports or alluded to in obscure and delicate language, perhaps because the short-term interest of managers was to present an image of confidence in the science and the data that underpinned advice, which they were required to supply. The Winters example is but the most glaring. In general, the administrative structure that kept scientific debate removed from public attention contributed to this outcome.

At various stages, part of the protective strategy of scientists, and particularly of science managers, was to resist opponents by producing alternative rationality claims. We do not suggest that any opposition should be accepted uncritically. However, it is probably not coincidental that the argument favoured by

managers to account for the radical decline of the cod biomass was to suggest that natural factors, exceptionally cold water and related changes in recruitment and migration patterns, were responsible. This was strongly advanced and new funds to support cod science in the early 1990s were designed to defend that thesis. This rationality had the advantage of removing responsibility from the managers. However, it went against the powerful statistical evidence marshalled by opponents in favour of an over-fishing argument. This over-fishing should be understood not simply as a problem of fishers catching more than their quotas, but more significantly because of persistent misjudgement of the size of the stock on which quotas were based. The bitterness of the dispute is understandable when we realize how closely it is associated with responsibility for an outcome harmful to the stocks and to people who relied upon or valued those components of the environment.

During the period covered by our research, managers attempted to prevent any public challenge to the official rationality. Again, this is not surprising, but it has had serious consequences. Suppression (not too strong a word) of dissenting views by scientist or managers, especially any public statements that went against department positions, was one critical tactic. Advice might be ignored as in the case of Winters. Suppression goes beyond that by trying to prevent statements being made at all. A number of examples may be given. In 1987, concern about low catches of cod in coastal waters of the Newfoundland region led to a Task Force chaired by a distinguished scientist, D.L. Alverson, and charged with evaluating what was happening. One interviewee, who should have been a prime witness, claimed that he was prevented from meeting the Task Force because it was expected that he would present a controversial and critical opinion. He considered this to have been 'a local decision made given the political sensitivity of it [that is, declining inshore cod landings]'.  


In the mid-1990s, a well published scientist-manager reported that he was reprimanded for publishing, in conjunction with outside authors, a paper that did not correspond with the official department position as represented by a public statement from the assistant deputy minister for science in the DFO. 'I got a lot of shit over that one.' This included a letter from the assistant deputy minister, in which the scientist was accused of having embarrassed the department. Our respondent felt that constraints on science were '... a fact of life ... To do research on DFO's fisheries policies and being a player in the department, you are just bound for trouble.'

We also discovered an interesting incident of political interference with the content of a document. At the crux of the crisis in Atlantic groundfish, the chair of the groundfish committee met with the deputy minister of the DFO just before a presentation to the fishing industry. The committee intended to compare the various stocks and argue they were all in a poor state. However, minister Crosby was about to announce closure of the northern cod fishery and argue that it was different from the others. The science presentation then had to be altered. All reference to similarity was removed and no comparative overview was presented. Our interviewee suggested that Mr. Crosby could not get enough money from Treasury Board to compensate for a full regional closure.

In the period shortly after the moratoria, a scientist stated that the regional director of science came to his office just before a public talk and asked him not to be critical of the department. He also described the process of reviewing one of his papers. After multiple reviews, the director eventually approved its submission to a journal, but, without the author's knowledge or permission, he faxed a copy to Ottawa. Within days, the author had to discuss the paper with the Assistant Deputy Minister of Science. This was a draft at the stage of journal submission and the author felt that the degree of ministerial involvement was 'disconcerting'. A second incident around publication, which is central to the development of science, comes courtesy of an experienced scientist who stated that the science director refused permission to submit a paper for external review until the author presented the director with a panel of reviewers who all supported publication. The scientist put the DFO official in a position where he could no longer pretend to reject the paper on scientific grounds. These decisions, the scientist claimed, involved people higher up the management chain than the science director.

A scientist no longer connected to the department at the time of the interview claimed that he never felt any pressure to produce a certain result, but he did get himself 'in a lot of trouble' for speaking out against quotas that were too high in 1990-91. There was no general blanket policy on speaking without clearance; 'there was a policy that would come on, sort of on and off like a light switch, and for various times. Certainly, I was muzzled many times during my actual career there. I would be told specifically I was not allowed to speak to anybody, either other inquiring academics or the press in particular, about any issue.'

Another related managerial control strategy was to permit only designated persons to speak with representatives of the mass media. This might be defended, as it was in several of our interviews, on the grounds that only properly knowledgeable persons should present the information. However, one interviewee found it 'a little bit odd' that he was never put forward as the DFO spokesperson for the stocks on which he was the main authority. He did not object to having an official spokesperson, but would have liked to comment himself on occasion.

When there was finally no choice but to acknowledge that cod and many other stocks were seriously depleted, the politicians tried to blame the department's scientists. According to an interviewee who was in a position to observe what happened, by the late 1980s, John Crosbie had the decisive voice, even when Tom Siddon was Minister. The real advice becomes what the lobbyists are saying to the political staff in Tom Siddon's office, and Siddon is told, '[d]on't you dare move until John Crosbie agrees with things'. More to the point, this person was present when Crosbie agreed with advice to let the scientists take the blame for the stock problems. Whatever the mechanism and the motivation, the suggestion that ministers simply acted on the scientific advice (without attention to other inputs) was hurtful and annoying to many scientists.

In the face of collapse, part of the management regime was dismantled. The Fisheries Resource Conservation Council replaced CAFSAC in 1993. Far fewer scientists were then involved at the final stage of advice, and discussions among scientists prior to providing their recommendations were limited to those at work in the local regions. Several people felt this hindered the final quality of the sci-

entific reports. The Council contained representatives from various sections of the industry and several academics so that these people were formally involved in the production of advice, rather than as recipients of an unchangeable allocation document. This change received wide support.

We do not want to leave the impression that all people interviewed experienced the suppression of their opinions or even felt that it was a serious problem. A manager agreed that some people were reprimanded for 'making comments on ministerial decisions,' but not if they stuck to non-management matters and avoided speculation. A stock assessment scientist claimed that it was 'horse shit' that scientists were reprimanded and afraid to speak out. However, he also accepted the view that no one should speak against the official consensus, once it has been established. Similarly, referring to the mid-1990s, an outside member of the Conservation Board felt there was no pressure from DFO to make decisions a certain way, but in public he would always give the official position, even when he disagreed with it. Another managerial respondent stated that external pressures had no impact on decisions: 'I'm very unequivocal about this. I basically see that it's had no influence.' Finally, another manager claimed that such pressures did not influence the scientific advice and that differences of opinion were encouraged.

## Conclusion

Many expected control over both people and nature through managed exploitation of resources after the extension of jurisdiction that most coastal states implemented in the 1970s. Our story describes how difficult it is to get control and to stabilise it, even when dissent is smothered for several years. Furthermore, we see that the lack of consensus in science makes truth and reason blurred and disputed. When the information system fails, the network is difficult to stabilise and this evident weakening and then collapse of the cybernetic management regime was met with opposing reactions. Thus, the modern Leviathan could not be built, and new management strategies had to be developed.

There is ample evidence from the 1980s and early 1990s that DFO's harvesting regulations and even the content of scientific advice were influenced more by the power of participants than the quality of their arguments. Winters' position was later acknowledged to be largely correct by several of his opponents. Consistent with Flyvbjerg's (1998) analysis, power based on position is more important than reasoned argument in the production of 'truth' in state bureaucracies. The power of the managers was evidently based on location in the command hierarchy of the department. However, managers were certainly constrained by the highest levels of the department, including the minister, and would often anticipate what was acceptable. In order to promote and protect their ideas and their careers, managers engaged in strategies that included partial control of science through funding decisions, attempting to ensure that public statements supported department positions, and ensuring that managers dominated the final stages of advice.

Scientists helped in that most were wedded to a particular vision of stock growth and trusted commercial data sources so that they dismissed challenges

without careful investigation. Aware of how various types of advice would be received, many scientists took that into account in presenting recommendations that they believed would be accepted. At times, personality differences interfered with the judgement of arguments. Finally, as some scientists clearly understood, it is misleading to use the term consensus to describe the position that appeared in statements of advice because it assumes that opposing positions have been reconciled in discussion. This does not adequately describe what took place.

Many scientists and managers, faced with mounting evidence of collapse, were reluctant to accept that they had been mistaken in their faith in their models and associated regulations. Hence, they adopted the natural causes theory to explain what happened rather than the overfishing account, which some, even within the DFO, put forward. Outside the DFO, overfishing seems to have been accepted more easily, except by the trawler companies. Inside the department, natural causes were favoured by those whose credibility was most challenged by the alternative; and this group had the power of position to make their view stand as the departmental position until, some years later, it was modified into either acceptance that both factors were responsible or that no one could be sure what had happened.

We do not wish to claim that any group's power was actually or, even in principle, complete. Our evidence also includes numerous examples of resistance to the strategies of control. Certain people insisted on speaking and writing according to their interpretations of evidence regardless of how their views might be received. One scientist did simply stop challenging and focussed on other matters, but others continued or left the department for academic appointments. Thus, we do not claim that power was or could be absolute.

What are the implications of this analysis? Is sustainable fisheries management only a dream, destined to remain a war among different groups trying to control or struggle against the networks that are emerging? Perhaps so, but it might be possible to achieve greater transparency and make it easier to introduce effective precautionary practices. Hutchings *et al.* (1997) looked for the answer in returning to an older form of organization in which science was conducted in an organization separate from the ministry, but problems of managerial control and self-censoring would likely continue. Moreover, given the reward system of science, it is unclear, in an independent setting, who would actually be prepared to do the mundane stock assessment work that does not lead to high prestige publications.

More critical than the location of the science is insistence on the openness and transparency that is required to challenge the basis of decisions. Whatever is inconvenient, such as the uncertainty about data and the questionable assumptions of models, should not be hidden in the management process. Science should not be privileged in the sense that its results or claims are thought to be based on facts that are open to only one interpretation and that in principle scientists with some effort can know anything. One problem is that too many scientists themselves believe this. To reorganise the system so that a more critical discourse can take place may improve the quality of the information on which managers act. Lack of consensus can be an argument for a precautionary approach, not against it. Moreover, later works about fisheries governance, such as Jentoft (2007) and

Kooiman *et al.* (2005), claim that interactivity between the *governing system* and the *system-to-be-governed* is necessary to achieve ecological and social sustainability. On the other hand, as other articles in this issue claim, the process of cyborgisation/cyborgization has contributed to a system shift, linking the governing system and the system to be governed closer together, by building governance mechanisms into the fishing organisations in micro level. According to Johnsen *et al.* (this issue) fishing boats become more and more cybernetically organised, and their management activities become a part of the activity on board. If that is the case, we must assure that cybernetic mechanisms, scientific rationality and reason are not allowed to rule alone.

We need clear recognition of the ethical or value judgements that must be made in decisions about killing fish. This relates to application or suspension of the precautionary principle, for example, and to indicators of biological, ecological, and social limits. Managers and the public must be clear about what values are prioritized when various decisions are taken. A sustainable fishery is a value. It may clash with other values. Yet, there are ways to bring it closer; the first steps are to be aware of how power works and what needs to be challenged.

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## Notes

- 1 These observations are based on Sinclair's memory of the meetings and are open to the possibility of error that all oral history involves. To protect confidentiality no individual is identified.
- 2 The process implies that scientific discourses act on their objects. In ANT terms this is called 'Performativity.' The concept of performativity is originally related to language pragmatics and the works of John L. Austin, who criticises the idea that language is basically representative. Performativity has become a central concept in ANT. For more detail, see Callon (2007).
- 3 For a general overview, see Irwin and Michael (2003) and Resnick (2000).
- 4 For a thorough review of this process from the perspective of science as a social construction, see Finlayson's (1994) controversial book.
- 5 For a compelling analysis of these problems in assessments of North Sea cod, see Nielsen (2003).
- 6 Since this incident is described in detail in the literature, we shall only add here some further telling information on how scientists responded to it. For further reaction to the article, see Doubleday *et al.* (1997) and Healey (1997) with a response from Hutchings *et al.* (1997).

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