Is scientific inquiry incompatible with government information control?

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Abstract: Government-administered science in Canada, and its potential for bureaucratic and political interference, merits examination in the wake of the biological and socioeconomic catastrophes associated with recent fishery collapses. We cite specific research on Atlantic cod (*Gadus morhua*) and Pacific salmon (*Oncorhynchus* spp.) habitat to illustrate how nonscience influences can interfere with the dissemination of scientific information and the conduct of science in the Canadian Department of Fisheries and Oceans. The present framework for linking fisheries science with fisheries management has permitted, intentionally or unintentionally, a suppression of scientific uncertainty and a failure to document comprehensively legitimate differences in scientific opinion. We suggest that the conservation of natural resources is not facilitated by science integrated within a political body. The formation of a politically independent organization of fisheries scientists, or some such reorganization of the link between scientific research and the management of natural resources, merits serious and open debate.

Résumé : La science administrée par le gouvernement au Canada, et sa vulnérabilité à l'intervention bureaucratique et politique, doit faire l'objet d'un examen à la suite des catastrophes biologiques et socio-économiques liées à l'effondrement récent des pêches. Nous citons de la recherche spécifique sur la morue franche (*Gadus morhua*) ainsi que l'habitat du saumon du Pacifique (*Oncorhynchus* spp.) pour démontrer comment des interventions non scientifiques peuvent gêner la diffusion de l'information scientifique et la conduite de la science au ministère des Pêches et des Océans du Canada. Le cadre de travail actuel de liaison entre les sciences halieutiques et la gestion des pêches a permis, intentionnellement ou non, que l'on fasse fi de l'incertitude scientifique et que l'on néglige de documenter de manière complète et détaillée les différences d'opinion scientifique légitimes. Nous estimons que la conservation des ressources naturelles n'est pas facilitée par l'intégration de la science dans un organisme politique. La création d'un organisme politiquement indépendant de scientifiques des pêches ou une quelconque réorganisation en ce sens du lien entre la recherche scientifique et la gestion des ressources naturelles devrait faire l'objet d'un débat sérieux et ouvert.
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Introduction

Integration of fisheries science within government is a relatively recent phenomenon in Canada. Prior to 1972, most research had been conducted under the auspices of the politically independent Fisheries Research Board of Canada (FRB). The FRB was dissolved in 1979 with the creation of the Department of Fisheries and Oceans (DFO), within which the full integration of fisheries science within a political body was complete.

Concomitant with the creation of the DFO were two events whose unfolding would later result in intense public scrutiny of the Department. One of these was the extension of Canada's fisheries jurisdiction to 200 miles in 1977. This effectively placed the responsibility for managing eastern Canada's groundfish fisheries with the federal government. Fourteen years later, commercial exploitation of Newfoundland's northern cod, *Gadus morhua*, ended because of a nearly 100-fold reduction in spawner biomass of what once was the largest cod fishery in the world (McGrath 1911). The second event occurred on the Pacific coast. This was the reduction in water flows of British Columbia's Nechako River, a large tributary of the Fraser River, effected by the Aluminum Company of Canada (ALCAN) in the late 1970s. Concern for the negative effects that reduced flows might have on salmon productivity prompted legal actions against ALCAN by the DFO in the early and mid-1980s before a much-criticized (British Columbia Utilites Commission (BCUC) 1994) out-of-court settlement on flow regimes was reached in 1987.

Recent declines in Atlantic cod (Hutchings and Myers 1994) and Pacific salmon, *Oncorhynchus* spp. (Walters 1995), coupled with clearly negative socioeconomic consequences, warrant examination of the means by which fisheries science is conducted, interpreted, and publicly presented by the DFO. Following a brief overview of the distinction between fisheries science and fisheries management, we cite specific examples of bureaucratic influence on government science related to northern cod and to the Nechako River settlement. Regarding Atlantic cod, we document nonscientific influences on the dissemination of information pertaining to its biology, ecology, and abundance that have deleteriously affected strategies designed to sustain the stocks in the past, have hindered scientific efforts to understand fully the causes of the present collapses, and, without change, are likely to limit the effectiveness of strategies to prevent fishery collapses in the future. Failure to document and (or) failure to acknowledge fully scientific uncertainty and variability in scientific opinion on the status of cod stocks were features common to the attainment of the Nechako Settlement Agreement. We contend that political and bureaucratic interference in government fisheries science

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compromises the DFO's efforts to sustain fish stocks and, thereby, the socioeconomic well-being of fishing people and fishing communities. There is an urgent need for public scrutiny of the influence of senior-level bureaucrats in the management of Canada's natural resources.

There is much variability in fisheries science and in stock assessment documents within and among the different regional branches of the DFO. Thus, our comments and criticisms of the dissemination of the results of fisheries research may not be wholly applicable to all regions and (or) to the stock and habitat assessments of all species. We also want to be very clear that our criticisms are directed at the present *system* of government-administered science and its facilitation of bureaucratic/political interference, *not* at individuals.

Communication between fisheries science and fisheries management

Fisheries science conducted within the DFO is an exercise distinct from fisheries management. Fisheries science develops information on the biology and population dynamics of exploited fishes, upon which rational management can be based. Such information can include data on age structure, movement and migration, vertical and horizontal patterns of abundance (i.e., distribution), interrelationships with other species, morphological, behavioural, and genetic distinctiveness, life history strategies, and associations between physical environmental factors (e.g., temperature, water flow, salinity) and biological variables (e.g., growth rate, survival). Fisheries science is the responsibility and primary activity of a professional cadre of research scientists. As in any branch of science, there are uncertainties and different interpretations associated with the quality of fisheries data, analysis, and interpretation. This situation leads to the formulation and testing, and reformulation and retesting, of alternative hypotheses to explain processes, patterns, and variability. This is a good thing; this is how science works. For commercially exploited fishes, particularly important hypotheses and debates are about interannual variability in recruitment, spatial and temporal differences in fish location, and spatiotemporal changes in fish mortality and stock structure.

Fisheries management consists of establishing and implementing regulations intended to maintain fish abundance at or above conservation targets. To control fishing mortality, these regulations can take the form of catch quotas (e.g., Total Allowable Catches (TACs), Individual Transferable Quotas), effort restrictions (e.g., minimum mesh sizes, maximum vessel sizes, season limits), and other measures to limit exploitation rates directly. Indirect protection of fish occurs through regulations that identify limits to which fish habitat can be manipulated (e.g., establishment of minimum water flow regimes, limitation of stream channelization, elimination of the influx of chemical pollutants).

Uncertainties in data and the existence of alternative hypotheses are often not evident in the science documents upon which management bases (in consultation with industry) its decisions. For example, rather than including confidence intervals on abundance metrics or demonstrating how one's perception of stock abundance is dependent on the validity of different sets of assumptions, stock assessment documents have often provided only a single perception of the health of a stock, and little or no indication either of the variability associated with the various parameters used in abundance models or of the robustness of these models to such variability (e.g., Baird et al. 1992).

It is also important to note that science documents have not been developed in close linkage with analyses of policy options. Hence, they do not contain direct assessments of the various options identified by managers and industry. The discordance is intentional, as noted by a former DFO Assistant Deputy Minister for Science: "It is the role of the Minister and not of public servants to make policy decisions affecting the fishery" (Morrissey 1993, p. 4). This direct consequence of the Act that established the DFO has led to the assumption that science can be used in policy assessment without the necessity of having scientific questions stated explicitly in terms of precise and particular policy alternatives.

Collapse of Atlantic cod in eastern Canada

Assessment of stock status

Assessment documents on Atlantic groundfish stocks seldom reflect the full range of opinions that often exist among scientists regarding the "health" of fish stocks. Although dissenting opinions can be expressed at stock assessment meetings, it is the "scientific consensus" on things such as present and past levels of fishing mortality and stock biomass that is paramount in the final stock assessment document. An example is presented below. By not acknowledging formally the existence of differing opinions on the health of a fish stock, and by not reporting and quantifying sources of variability and their effects on the robustness of abundance models, the transfer of scientific information to fishery managers bears little resemblance to the means by which scientific information is communicated, debated, and accepted by scientists in general.

Prior to 1993, scientific advice on eastern Canadian groundfish stocks was provided by the Canadian Atlantic Fisheries Scientific Advisory Committee (CAFSAC). Every year, data and stock assessment analyses pertaining to commercially exploited fishes were presented as CAFSAC Working Papers by DFO scientists to other DFO scientists in CAFSAC Subcommittees (e.g., the CAFSAC Groundfish Subcommittee dealt with Atlantic cod stock assessments). Working Papers accepted as CAFSAC Research Documents provided the supporting documentation and analysis for stock assessment. Subcommittee reports summarizing stock status, detailing specific advice, and recommending additional research constituted the primary source of information for the CAFSAC Steering Committee. Subcommittee reports were not available to the public. Following its vetting of these reports, the Steering Committee prepared Advisory Documents which formed the primary source of scientific information for the Atlantic Groundfish Advisory Committee (AGAC), the committee of industry and government (federal and provincial) representatives that provided recommendations to the senior managers and planners within the management structure of the DFO.

In principal, the flow of scientific information through these committees and subcommittees, from "raw" data to consensual interpretation, has a certain logic. It is clear why such a process is appealing to bureaucratic institutions whose leadership

requires that "specialist" information (e.g., scientific data and interpretation) be pared down to simple essentials before it can be used in reaching management decisions. But there clearly are risks associated with paring away too much information. For example, in 1990, CAFSAC (1990) identified three exploitation options for the 1991 TAC for northern cod: (*i*) 100 000 t, the TAC corresponding to the conservation-based $F_{0.1}$ target fishing mortality of $F = 0.20$ (under the $F_{0.1}$ strategy, the target instantaneous rate of fishing mortality, F, corresponds to the point on the dome-shaped relationship between yield per recruit and F at which the slope is 10% the slope at the origin; at $F = 0.20$, 18% of the harvestable biomass is removed by fishing), (*ii*) 150 000 t, the TAC corresponding to the "50 percent" rule, i.e., set next year's TAC halfway between the TAC corresponding to $F_{0.1}$ and this year's TAC, and (*iii*) 170 000 t, the TAC corresponding to $F = 0.40$, i.e., double the $F_{0.1}$ fishing mortality. However, CAFSAC (1990, p. 114) advised that there was no need to reduce fishing mortality to the $F_{0.1}$ target in 1991. Because of this recommendation, the $F_{0.1}$ TAC was not considered by AGAC (AGAC 1990). Rather, the exploitation options for 1991 debated by AGAC were 150 000, 170 000, and 200 000 t (i.e., status quo). These management options were presented to AGAC with little information as to the probability that the stock would grow under each option or the risk that each option posed in regard to the sustainability of the stock (CAFSAC 1990). The Minister of Fisheries and Oceans subsequently set the 1991 TAC at 190 000 t. The moratorium on the collapsed northern cod stock was announced the following year.

The primary objectives of those involved in stock assessment are to determine the range of possible states of nature and to evaluate the consequences of alternative management actions; the job of fishery managers is to choose the management action. Failure to transmit scientific uncertainty to decision makers, and an apparent inability and (or) unwillingness of decision makers to acknowledge uncertainty and to make decisions in the face of it, has been discussed in detail by Hilborn et al. (1993).

Scientific concerns regarding stock status of northern cod in 1986

One example of how difficult it has been for government to deal with scientific uncertainty is the means by which concerns regarding the legitimacy of the June 1986 Stock Assessment of northern cod were addressed. In October 1986, a meeting was called at the DFO, Newfoundland Region (St. John's), to consider reasons for the apparent discrepancy in the availability of cod to the inshore and offshore fisheries (the inshore fishing sector had been arguing that their catches were low because of overexploitation by offshore trawlers) and to address uncertainties associated with the most recent abundance estimates of northern cod. A list of questions was compiled and forwarded to CAFSAC: these questions were addressed at a meeting on 14 November 1986.

Among the CAFSAC Working Papers presented at the November meeting was one authored by Dr. George Winters (Head of the Pelagic Fish, Shellfish, and Marine Mammals Division, Newfoundland Region) entitled "Aide-memoire on 2J3KL assessment: *non gratum anus rodentum*?" Winters' main conclusions were that the size of the cod stock had been overestimated since 1977 (leading to the result that fishing mortalities were in excess of those reported in the 1986 stock assessment) and that this overestimation was caused by excessive reliance on abundance indices derived from commercial trawler catch rate data and by violation of assumptions of the multiplicative model used in the assessment procedure. Winters' unpublished paper is important because it represents one of the few in which disagreements based on assessment methodology related to northern cod stock assessments are documented. The manner with which the DFO dealt with the concerns expressed by Winters, and by non-DFO scientists (e.g., Keats et al. 1986), concerning the health of the northern cod stock serves as an excellent example of the potential incompatibility of government bureaucracy and normal scientific inquiry.

To sum the main points of Winters' short document (Winters 1986, pp. 1–2, italics added):

> "The impetus for my effort arises out of the uneasiness which I experience when I peruse the STACFIS [Standing Committee on Fishery Science] report on 2J3KL [northern] cod. Not only is its train of thought inconsistent but quite often there are no passengers on it. For example, it concludes that, while trends in commercial catch rates may be good indicators of stock abundance, the magnitude of such changes with time may not be reliable, due, in part, to violations in certain assumptions of the data treatment model (the Gavaris Multiplicative Model). It further concludes that the results of cohort analyses using [autumn research] survey data should also be viewed with caution because of environmental effects, *yet no analysis was performed to show that such effects were present in the data available*. Both indices were, however, used to estimate F_T [terminal fishing mortality] with widely divergent conclusions as to stock status and STACFIS assumed that the true status was at some intermediate (and arbitrary) point. There was no rigorous consideration of the two other alternatives that (*i*) estimates of F_T by the Commercial C/R [catch rate] and the survey index were biassed in the same direction or that (*ii*) only one abundance proxy was biassed and that its estimate of F_T should be discarded unless the direction and magnitude of such a bias could be evaluated."

Winters' analysis went on to address these biases and to document the strong influence that a given abundance metric can have on estimates of fishing mortality. Winters also examined briefly possible reasons for the recent failures in the inshore fishery. Contrary to the consensus expressed by Lear et al. (1986) that had concluded that cold water temperatures were responsible for low inshore catches in 1985, Winters documented a statistically significant negative association between inshore catch and offshore exploitation rate, concluding that (Winters 1986, p. 8) "the decline in the inshore catches since 1982 has been due to the increase in the offshore exploitation rate" (Winters 1986, pp. 8–9):

> "There is sufficient evidence to suggest that the assessment of 2J3KL Cod agreed upon by STACFIS in June 1986 presents an overestimate of stock abundance and this bias has been a consistent pattern at least as far back as the late 1970's. This error appears to be largely a result of a heavy dependence on commercial catch-rates as the major calibration source for cohort analysis and the biases introduced into such catch-rates by the violations in the assumptions of the Multiplicative Model used to standardize such data. Given this conclusion and considering the results from the

> Canadian surveys, the recent failures in the inshore fishery can be explained by accrued over-fishing of this stock as a result of persistently inappropriate advice on TAC levels."

CAFSAC's response and research recommendations

The first two questions posed to CAFSAC in October 1986 appear to address Winters' concerns (CAFSAC 1986): Question 1: "What is the current [1986] status of the Div. 2J3KL cod stock (4+ biomass and spawning biomass) in relation to its size in 1977?" Question 2: "What have catches been since 1977 (including estimated foreign activity) in relation to the $F_{0.1}$ objective?"

CAFSAC's response to Question 1 is peculiar. It begins by detailing uncertainties associated with abundance data estimated from research surveys and commercial trawler catch rates. After noting that the survey and commercial catch rate data gave divergent trends in abundance (the latter showing an increase in abundance since 1983, the former showing a decline), and that a revised weighting procedure produced a trend consistent with that based on the survey data, CAFSAC agreed with Winters (1986) that "these differences in the indices of stock abundance indicate...uncertainty with respect to the current estimates of stock size" (CAFSAC 1986, p. 4). However, CAFSAC went on to state that "the level of uncertainty in these estimates is not unusual in comparison with the assessment of other cod stocks in the Northwest Atlantic" (CAFSAC 1986, p. 4). This odd and clearly unscientific means of addressing acknowledged uncertainties in stock abundance is followed by implicit dismissal as CAFSAC concluded its response to Question 1 by restating the increases in stock abundance between 1977 and 1987 predicted by the June 1986 Stock Assessment.

CAFSAC's response to Question 2 included the statement that "annual population sizes since 1977 have been overestimated in previous assessments....$F_{0.1}$ catches based on the revised estimates of annual population size are considerably lower [30% to 70% lower; Fig. 8 in CAFSAC 1986] than the $F_{0.1}$ catches actually advised." Thus, it was recognized that the abundance of northern cod from 1977 to 1985 was probably considerably less than had been previously believed and that fishing mortalities had been approximately double the intended, and presumed sustainable, target levels.

Others were also questioning the stock status of northern cod in 1986 (see Steele et al. 1992). CAFSAC's conclusions regarding (*i*) uncertainties associated with abundance estimates, (*ii*) overestimation of stock abundance, and (*iii*) underestimation of fishing mortality were also reached by the authors of the first independent, albeit industry-sponsored, review of northern cod assessments (Keats et al. 1986). Researchers at Memorial University of Newfoundland concluded that (Keats et al. 1986, p. 23)

> "There is no evidence that the stock has continued to increase since 1981–82. Problems with catch rate and survey abundance indices suggest that we have no real idea of what is the true value of F_t [i.e., F_T], and therefore no idea of the true pattern of biomass change in recent years. Catch rate indices contain an unknown degree of bias. Survey abundance indices suggest a decrease in abundance of fully recruited cod in 2J+3K since 1981–82. Inshore landings have gone down since 1982. Unfortunately, as with offshore catch rates, which tell us very little, we also have no measure of inshore effort. Gill net landings have declined dramatically. Cod trap landings have not gone up since 1977. Where are the fish? Offshore? Where is the evidence? These observations suggest that the fishery has not continued to recover from the influence of foreign overfishing offshore, and that the northern cod in 3KL is currently being fished at a rate much higher than that which would allow the stock to increase. At the very least, this suggestion should be given much more serious consideration than it evidently has been given by NAFO [Northwest Atlantic Fishery Organisation], CAFSAC and AGAC."

The main conclusion of this review was that cod abundance was significantly lower than indicated by DFO assessments because of consistent underestimation of F, the same view expressed by CAFSAC (1986). The chair of CAFSAC's Steering Committee from 1989 to 1993 observed that the Keats et al. (1986) review was "not taken very seriously...by DFO Science [in 1986]...because the analysis was somewhat naive. It was easy for us to discount it" (see Finlayson 1994, p. 48). Based on interviews with DFO scientists and nonscience DFO employees, Finlayson (1994, p. 38) concluded:

> "The response from DFO was to dismiss the Keats Report as superficial. It was researched and written in only four weeks and, DFO claimed, axiomatically biased, pseudoscience written to support the political agenda of the Newfoundland Inshore Fisheries Association [who sponsored the review]."

The scientific validity of these responses to the review by Keats et al. (1986) must be assessed in light of the observation that CAFSAC had reached the same conclusions as the independent review at about the same time and using the same data. Furthermore, many of the concerns expressed by Keats et al. (1986) regarding the lack of data on the effects of fishing on cod (e.g., data on effort, size- and age-selectivity of gear) were also identified by CAFSAC in the recommendations that comprised their response to Question 18 which asked, "What specific research can be undertaken before the June 1987 assessment to better allow precise answers to questions of the type posed above?" In addition to physical and biological oceanographic analyses, CAFSAC suggested that studies be undertaken to (*i*) quantify effort and catch rates in the inshore and offshore fisheries, (*ii*) assess the validity of the assumptions of the multiplicative model (*iii*) identify potential biases in the research survey data, (*iv*) quantify discarding rates and include them in the assessment process, and (*v*) determine the fishing selectivity of each major gear type. CAFSAC's 1986 recommendations that had yet to be addressed by 1995 included (*i*) a comprehensive analysis of the spatial structure of cod as reflected by survey data, (*ii*) the documentation of and errors associated with catch discards from all gear types and an evaluation of the effects of discarding on abundance estimates, and (*iii*) the determination of age- and size-specific vulnerabilities of cod to different fishing gear.

Failure to enact CAFSAC's recommendations

Why were the most important of CAFSAC's recommendations — those dealing with the effects of fishing — not enacted by the DFO prior to the June 1987 assessment? Why did the DFO not publicly acknowledge that the concerns raised by Keats et al. (1986) were the same as those that had been identified by CAFSAC and that recommendations had been formulated to address them? CAFSAC's Advisory Document would

have lent credence to the arguments of those who had identified sources of uncertainty in the means by which the stock status of northern cod was being assessed and had argued that the abundance of northern cod was less than the estimates being provided by the DFO.

One factor that may have loomed large in a failure to act was the extremely high abundance of northern cod estimated from the research surveys conducted during the autumn of 1986. This anomalously high estimate had the effect of bringing the survey abundance estimates in agreement with those estimated from the commercial trawler catch rate data (see Baird et al. 1991; Hilborn and Walters 1992). The illusion that the stock was in terrific shape may have led DFO officials to conclude that there was no immediate need to investigate the questions posed by CAFSAC. But, rather than treating the 1986 survey-based estimate with the scientific suspicion it deserved (given the abundance data of previous surveys, it was impossible for the stock to have grown so much in a single year), the estimate was included in the 1987 assessment (Baird and Bishop 1987). This may explain why the DFO did not act upon CAFSAC's recommendations and why it publicly criticized the conclusions of Keats et al. (1986). For, if the 1986 survey abundance was even close to being accurate, the stock was not in the midst of decline, as suggested by Winters (1986) and by Keats et al. (1986), but was growing as argued in the 1986 stock assessment (Baird and Bishop 1986).

Would the uncertainties regarding the status of northern cod in 1986 have been summarily dismissed by a scientific establishment with no political or governmental affiliation? It is difficult to imagine that scientists concerned with the status of northern cod would not have persisted in their efforts to quantify these uncertainties and to evaluate their influence on stock abundance and corresponding TACs (e.g., a group of independent scientists who reviewed the northern cod stock assessments in 1987 concluded that the data were consistent with a twofold range in F (Alverson et al. 1987)).

One can conclude that constraints imposed by the DFO stifled efforts to undertake, or to discuss publicly, such analyses of scientific uncertainty. These constraints took various forms. Prominent among them was the withholding of research survey data from DFO scientists who did not normally participate in the annual stock assessments (see Finlayson 1994). Scientists were also explicitly ordered then, as they are today, not to discuss "politically sensitive" matters (e.g., the status of fish stocks currently under moratoria) with the public, irrespective of the scientific basis, and publication status, of the scientist's concerns. These constraints are on record in the form of memoranda between DFO officials and DFO scientists (e.g., memorandum dated 6 June 1995).[1]

Bias in stock status reports and manipulation of public perception

Bias is evident in some Stock Status Reports published by the DFO. These documents, written for public distribution, summarize contemporary scientific knowledge of the fish stocks managed by the DFO. They are also meant to reflect the Department's "scientific consensus" regarding the status of the various stocks.

The 1995 Groundfish Stock Status Report for Newfoundland Region begins with a seven-page Overview of the groundfish fisheries. Much of this Overview focuses on the causes of the recent stock collapses (the remainder provides brief descriptions of the fisheries for groundfish, pelagic fish, and invertebrates, harp seal abundance estimates, and trends in water and air temperatures). In this regard, one would reasonably expect a balanced treatment of the scientific research that has investigated the reasons for the stock collapses and to identify all relevant scientific documents upon which this research is based.

The 1995 Stock Status Report for Newfoundland groundfish does neither. The potential influence of fishing on the groundfish collapse receives almost no mention in the Stock Status Report's Overview, being limited to the remark that "while some strongly advocate that the decline can be fully explained by fishing, others believe there was an important environmental component that must not be ignored" (DFO 1995*a*, p. 5). Roughly half of the Overview is then devoted to arguments in support of some form of environmental or ecosystem change as an important cause of the stock declines. Virtually none of the evidence that fishing was the primary cause of the stock declines is presented in the document. This explicit bias is even more apparent when one is aware that work documenting overfishing of northern cod was excluded from the 1995 Stock Status Report despite the fact that such research had either been published (e.g., Hutchings and Myers 1994), presented at pre-1995 assessment meetings (e.g., Myers and Cadigan 1995*a*, 1995*b*), or presented at the April 1995 stock assessment meeting in St. John's (e.g., Myers et al. 1996*a*, 1996*b*, 1997*a*; see Shelton 1996).

Thus, the DFO's 1995 Stock Status Report for Groundfish in the Newfoundland Region inaccurately reflected the breadth of research that had been conducted on the causes of the groundfish collapses. Clearly, then, it did not represent a consensus among scientists who were conducting research in the area, although it may have represented a consensus of sorts among those who contributed to the final document.

A second example of selective exclusion of scientific information is the 1995 Stock Status Report on Gulf of St. Lawrence groundfish (DFO 1995*b*). The commercial fisheries for Gulf cod were closed in September 1993. The scientific document upon which the status of southern Gulf cod was based (Sinclair et al. 1995) was written after assessment meetings held in March 1995. This document noted that the 1993 closure of this fishery was attributable to the low abundance of 6- to 8-year-old cod — the age classes that usually comprise the bulk of the commercial catch. Sinclair et al. (1995) examined the degree to which the high mortality experienced by the 1985, 1986, and 1987 year classes of cod was due to fishing, seal predation, and environmental conditions. They concluded that "the most likely cause is increased exploitation in the late 1980's and early 1990's" (Sinclair et al. 1995, p. 25) and provided evidence in support of this conclusion. With respect to seals, "based upon current information [on survey-based trends in cod mortality], it is unlikely that Grey seal predation was the main cause of this trend in mortality" (Sinclair et al. 1995, p. 25). Regarding environmental causes of increased cod mortality, based on temporal trends in "the most severe

[1] Complete set of data (memoranda) available for a minimal charge from the Depository of Unpublished Data, CISTI, National Research Council of Canada, Bldg. M-55, Montreal Road, Ottawa, ON K1A 0R6, Canada.

environmental indicator in the southern Gulf of St. Lawrence," Sinclair et al. (1995, p. 25) concluded that "it is also unlikely that unfavorable environmental conditions are primarily responsible for this pattern [i.e., increased mortality of 1985–87 year classes]."

Thus, the conclusion reached during the 1995 assessment of southern Gulf cod was that the high mortality experienced by the year classes of cod that should have supported the fishery in the early 1990s was due to high fishing mortality. Observed trends in grey seal abundance and physical oceanographic conditions were apparently inconsistent with hypotheses that attributed increased cod mortality to either of these factors. The following statement was part of the original draft of the Stock Status Report submitted for approval to DFO officials in Ottawa and was to have appeared on p. 15 as part of the Regional Overview of fish stocks (memorandum dated 15 June 1995):

> "It is unlikely that seal predation or environmental conditions are responsible for these trends in total mortality [of the 1985–87 year classes of cod]."

The Assistant Deputy Minister (DFO Science) asked, "Is there evidence to back up this statement and is it consistent with last year's statement on grey seals?" (memorandum dated 15 June 1995). The Regional response to this query from Ottawa was to remove the statement from the Stock Status Report, contrary to scientific advice that the statement had empirical support and that it should remain (memorandum dated 15 June 1995).

Fishing people and the Canadian public have been ill-served by the bias that can exist in Stock Status Reports. The public has every right to expect such reports to represent the current state of knowledge of the groundfish stocks and the factors that influence fish abundance. Because of the authoritative profile that the Stock Status Reports assume, the public might well believe that these documents represent a consensus among fisheries scientists. Instead, as is clear in the aforementioned 1995 Stock Status Reports on Atlantic groundfish, recent key reviews have been biased against research that has identified overfishing as the primary cause of the present stock collapses.

Use of the "scientific consensus" of stock status reports to influence fisheries scientists

An indirect but equally serious effect of bias in Stock Status Reports has been on the research conducted by fisheries scientists. One example serves to illustrate this point. In response to questions asked by a journalist, a DFO research scientist in Newfoundland was quoted as saying "what happened to the [East Coast] fish stocks had nothing to do with the environment, nothing to do with seals. It is simply overfishing" (Globe & Mail, 25 August 1995). These comments were consistent with much of the research that had been conducted and published in peer-reviewed scientific journals. This includes articles on the collapse of northern cod (e.g., Hutchings and Myers 1994, 1995; Myers and Cadigan 1995*a*, 1995*b*; Hutchings 1996; Myers et al. 1996*a*, 1996*b*, 1997*a*; Walters and Maguire 1996; Walters and Pearse 1996; Sinclair and Murawski 1997; R.W. Doyle et al., unpublished data) and on other North Atlantic cod stocks (Myers et al. 1996*b*; Cook et al. 1997; Sinclair and Murawski 1997). It also agrees with the conclusions reached at the March 1995 assessment meetings on southern Gulf cod (Sinclair et al. 1995). However, as a consequence of this quote, the DFO scientist in question received an official reprimand for making statements that were not consistent with the content of the 1995 Stock Status Report for Newfoundland Groundfish (DFO 1995*a*) (memorandum dated 7 September 1995):

> "Your comments, as presented by the media, did not give a balanced perspective on the issue of the status of the cod stocks and were inconsistent with the June 1995 Newfoundland Stock Status Report. [We] have cautioned you regarding statements which do not take into account peer-reviewed scientific information. Your...disregard for both departmental policy on communication with the media and the professional opinions of your colleagues warrant the disciplinary action of a written reprimand. In the future, you are expected to respect both the system of primary spokespersons and peer conclusions on matters within your area of expertise."

By making public statements consistent with scientific publications (anonymously peer-reviewed) to which he also contributed, a fisheries scientist was officially reprimanded and experienced the job insecurity and psychological stress that accompany such reprimands. Bureaucratic action of this kind cannot help but have a stifling effect on proper conduct of science in the organization. Freedom to raise scientific debate should be integral to the conservation and management of Canada's natural resources.

Legitimization of bureaucracy: the portrayal of "science" as science

Inclusion of fisheries science within a political body can permit analyses presented by that body to be portrayed as being based on science, thereby legitimizing government policy and Departmental objectives. One example of such legitimization occurred in July 1992 when the DFO announced that a moratorium would be imposed on commercial fishing for northern cod. The moratorium was expected to last 2 years (DFO 1992). This 2-year time frame provided the temporal basis for the income support package (Northern Cod Adjustment and Recovery Program) that the Canadian Government offered fishing people affected by the fishery closure. What was the scientific basis of this 2-year recovery period?

The 2-year time frame for the recovery of northern cod was apparently based on a graph in a government press release (DFO 1992; reproduced as Fig. 10 in Lear and Parsons 1993). The graph in the press release provided two projected increases in northern cod spawner biomass between 1992 and 1994. The projections indicated that, in the absence of fishing, northern cod spawner biomass would increase *more than sixfold* between 1992 and 1994 to as much as 600 000 t — a spawner biomass that had not existed since 1972. These projections appear to have been made by (1) multiplying the 1992 numbers-at-age vectors estimated from two Virtual Population Analyses (VPAs) (ADAPT and Laurec–Shepherd) by a mean weight-at-age vector and (2) assuming that the instantaneous rate of total mortality on northern cod during that period would be caused by natural factors alone (i.e., $Z = M = 0.2$).

These spawner biomass projections portray data as having a scientific basis despite being subjected to none of the criteria that are part of a complete scientific analysis (e.g., anonymous peer review). For example, the scientific literature contains

estimates of instantaneous rates of population increase (r) considerably lower than those required by the DFO (1992; Table 1). The values of r associated with the DFO's projected population increases range from 0.80 to 1.10, corresponding to annual rates of increase in spawner biomass at low density of 126 and 200%, respectively. In contrast, estimates of r in the scientific literature for northern cod range from 0.09 to 0.17, corresponding to annual rates of increase of 9 and 19%, respectively. Moreover, only 3 (15%) of the 20 cod stocks throughout the North Atlantic are predicted to have values of r in excess of 0.80. In each of these 3 stocks, age at maturity is 2 to 3 years rather than the 6–7 years for northern cod (Myers et al. 1997*b*). Thus, the DFO estimates differ markedly from studies published in the scientific literature based on data available in 1992.

The DFO labelled its two projections of spawner biomass as "high" and "low." This suggests a statistically based confidence interval, i.e., a reflection of the error associated with a mean projection. Instead, the "high" predicted spawner biomass referred to the ADAPT-based projection, while the "low" estimate was that based on the Laurec–Shepherd method. This can only be determined from the original stock assessment document in which the VPA estimates were calculated (Baird et al. 1992, Table 46). Thus, statistical confidence intervals, a normal calculation in the scientific literature, were not part of the DFO's predicted 2-year moratorium period. Finally, the DFO's spawner biomass predictions were based on the assumption that the most recent VPA estimates of age-specific abundance were without error. This assumption has been demonstrated repeatedly to be without justification and to produce grossly inaccurate estimates of future stock biomass (Walters and Maguire 1996; Walters and Pearse 1996).

Summary

The perceived need for scientific consensus and an "official" position has seriously limited the effectiveness of government-based research to contribute effectively towards an understanding of the collapse of Atlantic cod. Nonscience influences on fisheries research incompatible with normal scientific inquiry included (*i*) government denunciation of independent work, (*ii*) misrepresentation of alternative hypotheses, (*iii*) interference in scientific conclusions, (*iv*) disciplining of scientists who communicated publicly the results of peer-reviewed research, and (*v*) misrepresentation of the scientific basis of public reports and government statements.

Pacific salmon production in British Columbia: the Kemano Completion Project

Prologue

Inappropriate government influence on fisheries science in Canada is also evident on the Pacific coast. A well-documented example is the September 1987 Nechako Settlement Agreement between the DFO, the Province of British Columbia, and ALCAN. The central focus was the rate of water discharge from an ALCAN dam. The water temperature and flow conditions provided by the discharge were biologically important to salmonid production in the Nechako River, B.C. The Nechako River, a major tributary of the Fraser River, had a mean annual discharge (MAD) of 201.9 $m^3 \cdot s^{-1}$ (Jaremovic and Rowland 1988) and supported several salmonid fish including sockeye, *Oncorhynchus nerka*, and chinook salmon, *Oncorhynchus tshawytscha*.

In the early 1950s, the B.C. Government gave permission to ALCAN to construct an aluminum smelter in Kitimat, B.C. They built a water storage and hydro-generating dam to supply electric power. The dam greatly reduced water flows in the Nechako River. Between the late 1950s and 1978, operation of the dam reduced annual discharges by 40–50%. Low reservoir inflows coupled with increased demand for electricity in 1979 resulted in the release by ALCAN of even lower water flows (November 1979 discharge was approximately 10% of natural flows; BCUC 1994).

In 1980, the Attorney General of Canada obtained, on behalf of the DFO, a temporary injunction from the B.C. Supreme Court that required ALCAN to release flows that averaged about 33% of natural flows (60.9 $m^3 \cdot s^{-1}$; DFO 1986). In mid 1985, ALCAN petitioned the B.C. Supreme Court for a permanent resolution to the jurisdictional issues regarding discharge rates in the Nechako River. A trial was set for the end of March 1987.

In preparation for the trial, the DFO wanted a definitive position on the discharge necessary to protect salmonid populations in the Nechako system. A briefing document provided to the Minister of Fisheries and Oceans in preparation for a 24 February 1986 meeting in Vancouver suggested two alternative flow regimes: a "base" flow regime of 62.8 $m^3 \cdot s^{-1}$ (DFO 1986) and a "preferred" flow regime of 124.0 $m^3 \cdot s^{-1}$ that made use of surplus water in wet years (BCUC 1994). It was the view of several DFO scientists who had, since 1983, been assessing the influence of river discharge on salmonid survival and production in the Nechako River that flows in the range of the preferred flow and higher were required to maintain salmonid production at conservation targets (BCUC 1994). Following the 24 February 1986 meeting, the Minister indicated that he "was not satisfied with the pleading [i.e., the preferred flows]." This was partially because of the uncertain fishery benefits provided by the additional flows and the natural hydrological variability in ALCAN's water supply. The Minister requested that the pleading be made "more reasonable" (memorandum dated 25 February 1986). Following a recommendation made by the Deputy Minister (memorandum dated 27 February 1986), the DFO revised its pleading to indicate that it sought a requirement for ALCAN to adopt the base flow, the lower of the flow regimes considered by the DFO. By comparison, ALCAN's pleading flows before the Supreme Court (MAD of 19.6 $m^3 \cdot s^{-1}$; DFO 1986) represented approximately one third the 1980 "injunction" rate recommended by DFO.

In March 1987, the B.C. Government pressed for an out-of-court settlement (BCUC 1994). The Minister of Fisheries and Oceans agreed. Following a series of proposals and counter-proposals between the DFO and ALCAN, negotiations culminated in a 4-day meeting between representatives of the DFO, ALCAN, and the B.C. Government. The Terms of Reference for this Nechako River Working Group (NRWG) were, "To develop a program of measures and plan of implementation which will provide an acceptable level of certainty for the conservation and protection of the chinook fisheries resource

Table 1. Predicted rates of population increase (*r*) for northern cod, based on data available in 1992; this table contrasts official predictions by the DFO (1992) with those published or submitted to the scientific literature.

Type of analysis	Population growth (*r*)	Reference
ADAPT VPA	0.80–0.88	DFO 1992
Laurec–Shepherd VPA	0.98–1.10	DFO 1992
Age-specific survival and fecundity	0.13–0.17	Hutchings and Myers 1994
Stock–recruitment relationship	0.17	Myers et al. 1997*b*
Stock–recruitment relationship and population growth analysis	0.09–0.13	Walters and Maguire 1996

of the Nechako River" (BCUC 1994). However, the discharge rates being considered by the NRWG differed considerably from those previously pleaded by the DFO to the Supreme Court. In fact, the NRWG was instructed to "take the Alcan fish and other use flows as a given" (BCUC 1994, p. 28). One interpretation of this instruction is that the DFO was asking if there was an acceptable way of adopting ALCAN's low discharge rates (approximately 10% of historical flows and about one third of the DFO's pleading rate). The NRWG identified a hierarchical series of mitigative measures (ranging from habitat improvements to the construction of a hatchery) intended to ensure an escapement of 3100 chinook spawners per year (i.e., the 1980–1986 average escapement, identified by the NRWG as a conservation target) (BCUC 1994). The result of the deliberations by the NRWG was the Nechako Settlement Agreement, announced by the DFO in September 1987, in which a long-term flow regime (26.5 $m^3 \cdot s^{-1}$), slightly greater than ALCAN's pleading flows, was accepted and a series of measures intended to mitigate unintended effects of low flows on salmonid production presented.

Bureaucratic interference in the interpretation and dissemination of scientific research

Following the imposition of the injunction flow in 1980, ALCAN undertook an environmental impact assessment of the Kemano Completion Project (KCP). This expansion project would permit ALCAN to utilize all of the water it was entitled to under its original contract with the B.C. Government. In August 1983, a team of DFO biologists and scientists — the Kemano Task Force — was formed "to co-ordinate and conduct the technical review of ALCAN's proposal from a biological perspective, assist in developing the departmental position on the proposed development and provide technical assistance at public hearings" (Mundie 1994, p. 4; Schouwenburg 1994). The Task Force concluded in March 1985 that ALCAN's proposed flow (ALCAN's eventual pleading flow) would adversely affect chinook salmon in the Nechako River (Schouwenburg 1994). The Task Force Report also made it clear that "there were few areas of agreement between the positions taken by ALCAN's consultants and the Task Force regarding the extent and nature of the impacts on the fisheries resource arising from the project" (Schouwenburg 1994, p. 4).

In preparation for the DFO's Supreme Court case against ALCAN, a workshop was held in mid-November 1985 at which the potential biological consequences of ALCAN's proposed flow regime were discussed (BCUC 1994). The proceedings of this workshop marked "the first time that scientists began to entertain a suspicion that the Department might not be solely concerned with providing flows that would protect the salmon" (Mundie 1994, p. 7). One of the scientists' concerns was the proceedings' failure to document adequately the differences in opinion that existed between scientists and managers. As noted by one scientist (memorandum dated 28 November 1985, italics added):

> "I am greatly disturbed generally by the failure of the [workshop's] notes to accurately record the polarization of opinions which occurred between the scientists and the habitat managers...the scientists were using as a starting point the amount of water required for the needs of the fish while the managers were starting from the point of optimizing conditions for the fish with water left over after water was first used for hydroelectric generation."

In mid-February 1986, just prior to the Minister's aforementioned 24 February meeting in Vancouver, three of the scientists who had been members of the Kemano Task Force expressed their discontent with the Technical Report being prepared to brief the Minister, noting the "substantial differences that exist between the views expressed in the report...and those offered by us" (memorandum dated 13 February 1986). In their opinion, "the gap between scientists and the operational staff in this matter is one that [can] not be bridged by minor adjustments in the report, but only by a major re-aligning of approach, i.e. away from compromise with industry and towards defining safe conditions for the fish" (memorandum dated 13 February 1986). It could be argued that the opinions of the scientists simply reflected one end of a continuum of opinions that ranged from having a wholly "natural system" to making every attempt to accommodate industry, recalling that habitat managers do have to deal with industry on a regular basis. It could also be argued that it was at this point in time that the selective use of available scientific information began, perhaps reflecting an editorial role that management staff had on the preparation of the Minister's briefing document.

The influence of the Kemano Task Force was further eroded at the 24 February 1986 briefing of the Minister at which no DFO research scientist was present (memorandum dated 25 February 1986). Recall that the two potential pleading discharge rates being considered by the DFO were the base flow of 62.8 $m^3 \cdot s^{-1}$ and the preferred flow regime of 124.0 $m^3 \cdot s^{-1}$ (BCUC 1994). Following the meeting, a senior ministerial bureaucrat concluded, "we [i.e., those who prepared the report for the Minister] were being unreasonable in our demands to take all the excess water [in wet years] to obtain questionable benefits in fisheries production" (memorandum dated 25 February 1986). The pleading was revised and the Minister adopted the base flow regime as the DFO's

pleading flow in the file submitted to the B.C. Supreme Court on 28 February 1986 (BCUC 1994).

The selective inclusion of, or emphasis placed on, the scientific input that supported the Minister's decision to set the pleading flows at the base flow rate was noted by those who had participated in the Kemano Task Force (Mundie 1994). The 1986 briefing report failed to document scientific uncertainty that the base flows would achieve the conservation goal of salmon production. Rather, the document indicated certainty that the base flow was the minimum acceptable flow, and that somewhat uncertain benefits might result from higher (i.e., the preferred) flows. In response to a query by one of the Task Force scientists as to the origin of the final pleading flows, the senior DFO manager responsible for the Nechako River Project wrote, "If you are referring to the current pleading flows I am sure you are aware that those flows represent the Minister's opinion not any decision of our team" (memorandum dated 4 March 1987).

Between March 1986 and March 1987, the DFO prepared expert reports and expert witness statements for the trial (BCUC 1994). But despite being asked to provide scientific input to the court case in the form of witness statements, science was not to be the first priority of DFO research scientists. Rather, their first priority was that they support the Minister's position. This is perhaps not surprising given the need of lawyers to marshall all available evidence to support the Minister's decision. However, this highlights the dilemma between the scientific ethos of basing opinion on empirical "facts" and the need of a government department to support its Minister. Regarding his role as a potential expert witness, one of the scientists of the Kemano Task Force asked, "How can a scientist who opposed the recommended flows both before and during the time they were being drawn up speak for them?" (memorandum dated 18 July 1986). Referring to bureaucratic interference on the contents of expert witness statements, this scientist noted (memorandum dated 18 July 1986):

> "At a meeting in Vancouver on April 28 the Director General instructed staff to support the Minister's position while adhering to the scientific advice. As my statement shows, I find it impossible to do both...[It was] pointed out to me that those technical staff who do not support the Minister 'must take their game and play elsewhere'. This underlines the seriousness of my predicament and makes me feel intimidated."

Following the request by the B.C. Government for an out-of-court settlement, there was no more need for expert witness statements. That much of the scientific evidence and expertise available to the Department was ignored is evidenced by the NRWG terms of reference in 1987, and by the decision to exclude most of the individuals intimately involved with the project from the Working Group.

Evaluations by scientists

Since the announcement of the Agreement, the mitigative measures described therein have been denounced by the independent Commission, chaired by Dr. Peter Larkin, that undertook the Kemano Completion Project Review in 1994 (BCUC 1994, pp. 156–157):

> "The various proposed instream remedial measures are for the most part untried and in the view of the Commission will not be effective as mitigation. The proposed measures for protection of resident trout...are at least as unlikely to be effective as those for chinook salmon as measures of mitigation. The concept of off-site mitigation...is unacceptable because it does nothing to replace the habitat that is lost."

The arguments by DFO scientists for high water discharge rates appear to have been well founded. They were supported at the time by a case history study by Mundie and Bell-Irving (1986) that documented reductions in the salmon stocks in 26 of 29 rivers in which flows had been regulated. And more recently, Bradford (1994) has suggested that there is evidence of a negative association between flow and chinook salmon production in the Nechako River. His work also questions the capability of the current flow regime to sustain the chinook population at the 3100 spawner target identified in the Settlement Agreement.

Summary

The selective use of scientific information in the events leading to, and in the provisions contained within, the Nechako Settlement Agreement of September 1987 is a poignant example of how government bureaucrats can, and do, interfere with science. Decisions on flow regimes were made and were termed scientifically defensible despite a broad range of scientific opinion to the contrary. These problems are well summarized by one of the biologists originally involved in the assessment of the KCP in a brief prepared for the 1994 Kemano Completion Project Review (Schouwenburg 1994, p. 12):

> "The facts of the matter are that [the Minister] and his government chose...to adopt a position of virtual surrender to both ALCAN and the province. They did not...admit that this was what had been done and chose instead to embark on a program of disinformation. First they had to disavow the existence of credible information contrary to ALCAN's view of the impacts of the project. This was accomplished through [1] the suppression of information such as that contained in the Kemano Task Force Report, [2] the intimidation and 'gagging' of employees familiar with the project evaluations done by DFO with respect to the Task Force and the court case to the point that they fear for their jobs, and, [3] the misrepresentations of the terms of reference given DFO representatives to the [NRWG] in recommending the Nechako Settlement Agreement."

Restructuring the linkage between science and management of fisheries

One factor common to the examples given above, intentional or otherwise, and to greater or lesser degrees, is a suppression of scientific uncertainty. We would argue that bureaucratic intervention has deleteriously influenced the ability of scientists to contribute effectively to fisheries management. Viability of fish stocks, sustainability of employment in fisheries, and persistence of coastal fishing communities would appear to be poorly served by the present institution in which fisheries science is inextricably linked to, and affected by, a political bureaucracy. It is difficult to imagine how wise policies for dealing with uncertainty can be devised in the present administrative atmosphere.

We suggest that the science and stock assessments conducted for government be provided by a publicly funded, but

politically independent, institution (somewhat analogous to the Canadian judiciary). The former Canadian fisheries research boards (e.g., Biological Board of Canada (1912–1937), FRB (1937–1979)) may provide an appropriate model of an organization of fisheries scientists dedicated to undertaking research on the population biology of commercially exploited fishes.

The precursor to the Biological Board was the Board of Management of the Marine Biological Station of Canada. Established in 1898, the Board consisted of one representative of the Canadian Department of Fisheries and eight professors from each of eight universities whose faculty collaborated with Board researchers in fisheries research (Johnstone 1977). The objectives of this Board, and of its successors, were twofold: (*i*) to undertake independent scientific research on aquatic organisms in marine and inland waters and (*ii*) to provide solutions to practical and applied problems in the commercial fisheries.

In 1912, protests by the Board and its researchers against government control of the Board's research budget resulted in the formation of the more independent Biological Board of Canada (Johnstone 1977). Oversimplifying the situation, but identifying one of the scientists' seeds of discontent, A.G. Huntsman recalled that (Huntsman 1943, italics added)

> "A clerk's veto of a request for purchase of scientific literature because the latter was in foreign languages caused an explosion and resulted in the creation in 1912 by Act of Parliament of the Biological Board of Canada, an *independent* body under the Minister to have charge of all [Canada's] biological stations."

The scientific value of the Biological Board and its researchers was rated highly by scientific peers. For example, University of Toronto zoologist J.R. Dymond noted in 1939 that, "no other factor has had a greater influence on the development of zoology in Canada than the organization of the Biological Board" (Dymond 1939, p. 49). The Biological Board consisted of 12 members: two representatives of the Canadian Department of Fisheries, two representing the fishing industry, and eight nominated by as many Canadian universities. Dymond noted that whereas the Biological Board stressed representation of universities, the 1937 Act that created the FRB permitted the selection of scientists "peculiarly suitable for the Board's work, the institution to which they may happen to be attached being a secondary matter" (Dymond 1939, p. 51). The FRB in the late 1930s consisted of nine scientists, four representatives of the fishing industry, and two Department of Fisheries representatives; its permanent scientific staff consisted of approximately 35 individuals (Dymond 1939).

The formation of the International Commission for the Northwest Atlantic Fisheries (ICNAF) in 1949 stimulated an increase in the effort expended by the FRB on matters related to stock assessments; fish behaviour and fishing methodology were apparently studied seriously for the first time (Johnstone 1977). The scientific excellence of the FRB in the 1950s and 1960s is evident from a sampling of its members: F.E.J. Fry, J.L. Hart, F.R. Hayes, W.S. Hoar, D.R. Idler, P. Larkin, W.E. Ricker, and W. Templeman. However, with a restructuring of the Department of the Environment in the early 1970s, increasing government infringement on the operations of the FRB culminated in 1972 when the Board lost its independent status, reporting thereafter to an Assistant Deputy Minister for Marine and Fisheries (previously the FRB had reported directly to a Minister). F.R. Hayes, Chair of the FRB from 1964 to 1969, argued that the primary reason for government's desire for increased influence of fisheries research was that "the government simply cannot contemplate the control of policy and funds by any but its own employees" (Hayes 1973, p. 31). The FRB was dissolved by an Act of Parliament in 1979 with the consolidation of fisheries research in the newly created Department of Fisheries and Oceans.

Proposed solutions

Recognizing the need for recuperative measures, what would be the attributes of a new natural resource science establishment that could operate unimpeded by bureaucratic and political influence? This new organization would provide scientific information on issues such as abundance and stock status of exploited fish stocks, effect of habitat alteration on fish production, and deleterious consequences of interactions between wild and cultured fishes to the Minister of Fisheries and Oceans but would not be a part of that Department. The organization would meet annually to prepare assessment documents and stock status reports in much the same way that DFO biologists and scientists do today. But critical differences would include the following, using stock assessments as an example.

(*i*) Assessment documents would be explicit concerning scientific disagreements about stock status. This could take the form of the presentation of different sets of assumptions concerning the stock and the abundance and fishing mortality corresponding to each set. The review process would produce decision tables showing the range of ecological consequences (alternative hypotheses about response) for a strategic range of alternative strategies (Hilborn et al. 1993). The presentation would examine possible outcomes for a range of strategies, ordered from very cautious to very optimistic. The review process would also include highly qualified ecologists and population biologists regardless of the "fisheries application" of their research.

(*ii*) The variability associated with model parameters and variables used in each analysis would be quantified and made explicit in the document.

(*iii*) All scientific information on stock abundances would be released to the public at the same time that it was presented to the DFO. This point is extremely important; by making all scientific information available to the public, the public could then evaluate for themselves the Minister's management decisions based on the scientific data that had been received. The public cannot at present make such an evaluation, making it possible for politicians and fishery managers to disregard scientific advice on occasion and to blame unintended consequences of such management decisions on the quality of the scientific information given to them on other occasions (Finlayson (1994) discusses and gives examples of such instances in more detail). Such a model of interactions between scientists and managers/politicians would do considerably more in providing an objective scientific basis to fishery management than does the present structure of the DFO (see also Walters 1995). It is not clear how such a reorganization would

Table 2. Arbitrary probabilities of occurrence of six undesirable influences on the conduct and communication of fisheries science and stock assessments under different institutional arrangements.

	Assessments/science/fisheries policy conducted by government department (status quo)	Strengthened external review of assessments and policy options	Contested assessments by interested groups	Alternative assessments by different national or international organizations (e.g., ICES, NAFO)	Privately funded, contracted assessments/science	Publicly funded, politically independent science/policy analysis organization
Failure to transmit scientific uncertainty to decision makers	Medium/high	Low/medium	Low	Low	Low/medium	Low
Inability or unwillingness of decision makers to acknowledge or address scientific uncertainty	Medium/high	Medium	Medium	Low/medium	Medium/high	Low/medium
Government restriction of public access to assessment/science data	Medium/high	Medium/high	Low/medium	Medium/high	Medium/high	Low
Government interference in public dissemination of scientific research	Medium/high	Medium/high	Medium/high	Medium/high	Medium/high	Low
Government influence on direction of scientific research	Medium/high	NA	NA	NA	Medium/high	Low
Industry interference in public dissemination of scientific research	Low	Low	Low/medium	Low	Medium	Low

be initiated, but it would be unlikely to occur without a comprehensive public examination (e.g., Report by the Auditor General for Canada) of the present structure of the DFO and of the factors that compromise its mandate for the conservation of fish.

Being the most preferable change in the status quo, the formation of a publicly funded body of scientific inquiry, completely independent of political influence, is one of a range of modifications to the present system that could be implemented. A representative, but by no means exhaustive, set of options is offered in Table 2. These include (*i*) contested stock or habitat assessments for which different parties, such as commercial and recreational fishers, industry representatives (e.g., ALCAN), and conservation groups, participate in the assessment process, have full access to the raw data, and analyze and table alternative assessments for decision makers if a consensus is not reached, (*ii*) alternative assessments conducted by different national laboratories or international organizations, and (*iii*) privatized research in which independent research organizations are contracted to undertake specific assessment tasks or narrowly defined research projects. To each of these options, we have assigned arbitrary, but perhaps not unreasonable, probabilities of occurrence of six institutional factors considered undesirable in fisheries science and fisheries management. Such a table may provide a useful starting point for discussion (see also Hilborn et al. 1993). Detailing the means by which each of these institutional structures would deal with the problems identified would be an appropriate subject of a separate paper.

Summary

We have given two examples of how nonscience influences can interfere with scientific information and the undertaking and conduct of fisheries research in the Canadian Department of Fisheries and Oceans. The present framework for linking science with management can, and has, lead to abuses that threaten the ability of scientists to understand fully the causes of fish declines, to identify means of preventing fishery collapses from recurring, to incorporate scientific advice in management decisions, and to communicate research in a timely fashion to as wide an audience as possible. The existing framework of government-sponsored fisheries science needs to be replaced. It has failed to ensure viable fish resources and thereby sustain the fishing people and fishing communities upon which successful fisheries management depends. The economic and societal cost of this failure to Canada has been enormous.

The present work invites a variety of questions about why the federal government, or at least the part represented by the DFO, has acted in the manners described above. Why were senior DFO officials not interested in ensuring wide reporting of differences of opinion about assessment matters? Why did they repress challenges to their practices, given that the biological and socioeconomic consequences would be extremely serious if DFO assessments were indeed optimistic? To what degree were decisions based on individual differences in personality, or perhaps the perceived need to balance scientific concerns with the sociopolitical constraints imposed on the decision-making process by a government bureaucracy?

There is a clear and immediate need for Canadians to examine

very seriously the role of bureaucrats and politicians in the management of Canada's natural resources. The present framework of government departments such as the DFO is based on the belief that the conservation of natural resources is best ensured by science integrated within a political body. Recent history would suggest otherwise The formation of a politically independent organization of fisheries scientists, or some such reorganization of the link between scientific research and the management of natural resources, is a timely idea that merits immediate, serious, and open debate.

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