

Reply: Scientific inquiry and fish stock assessment in the Canadian Department of Fisheries and Oceans¹

and

Reply: The interplay of policy, politics, and science²

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The collapse of Atlantic cod was the unifying theme of Memorial University's EcoResearch project "Sustainability in a cold ocean coastal environment." The project entailed a broad-scale examination of the history, context, and consequences of that collapse. One objective was to analyze the role of science in fishery management decisions. The research question that became the title of our paper (Hutchings et al. 1997) was developed within that framework. We concluded that an environment in which information can be controlled, for whatever reason, is not one in which the scientific process can exercise its full potential. Our attention was drawn to the institutional structure that results in such an environment. After identifying what we perceived to be undesirable consequences of having science fully integrated within government, we posed the question, how should the structure be changed?

Doubleday et al.'s (1997) comment is a welcome contribution to this examination. They make the valid point that the present stock assessment review process is more open to non-DFO employees than in the past. However, they charge that by misinterpreting and selectively quoting from Department of Fisheries and Oceans (DFO) documents, Hutchings et al. (1997) "give a false impression of stock assessments of Northern cod in the 1980s and 1990s." Because of our comparatively minor treatment of northern cod stock assessments, Doubleday et al.'s point of departure is surprising and draws attention away from the main points of our perspective. We examined neither the structure of stock assessment models nor their ability to estimate stock size and fishing mortality, ren-

dering superfluous the discussion of the Alverson and Harris reports by Doubleday et al.

Doubleday et al. (1997) describe the stock assessment review process presently used by the DFO. We agree that the recently modified system has many desirable aspects. For example, the presence of fishers and non-DFO scientists at assessment meetings has been viewed in a positive light by all parties. However, the integration of outsiders in the assessment process is not nearly as complete as Doubleday et al. imply. First, assessment documents are not written in a form readily accessible to non-scientists. Second, in Atlantic Canada, involvement of non-DFO scientists can range from a full review of assessment documents received prior to assessment meetings (a good practice) to a simple response to general open invitations to attend meetings and provide comment (a considerably less effective practice). However, involvement by non-DFO employees in actual stock assessment data analysis, i.e., running and tuning Virtual Population Analyses (VPAs), is rare, if it occurs at all. Thus, in practice, the production of, and the ability to comprehensively review, stock assessment documents remains a DFO "in house" affair in many important respects.

Doubleday et al. (1997) appear to have misunderstood our discussion of scientific concerns regarding northern cod in 1986. We focused on the convergence of conclusions among DFO and non-DFO scientists regarding both the overestimation of northern cod biomass and the underestimation of fishing mortality on this stock. We cited Winters (1986) because of the many uncertainties that he identified. Although scientific uncertainty with respect to stock size and fishing mortality prompted several recommendations by CAFSAC at the time, it was not until 1989 that these uncertainties were reflected by somewhat reduced catch quotas. Doubleday et al. (1997) do not explain why these significant recommendations were not enacted within the time frame specified by CAFSAC.

Regarding the projected increases in northern cod spawning stock biomass between 1992 and 1994 (Doubleday et al. 1997), these were based on numbers-at-age estimates from VPA. Uncertainties in these numerical estimates were not quantified. In addition, the projections did not account for the well-known "retrospective" problem associated with VPA numerical estimates (Smith and Gavaris 1993; Myers and Cadigan

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² Reply to comment by M.C. Healey 1997. *Can. J. Fish. Aquat. Sci.* **54**: 1427–1429.

1995), namely that statistical biases in the estimation procedure cause abundances of current cohorts to be consistently overestimated. Regarding their Fig. 1, again we note the absence of simple uncertainty measures, e.g., standard errors, an omission apparently consistent with the reporting of point estimates in stock assessments in the 1980s (Doubleday et al. 1997).

Concerning the reprimand of a DFO scientist, this was justified by Doubleday et al. (1997) by reference to DFO's Media Relations Guidelines. Such reprimands may be acceptable in a tightly managed bureaucracy, but from a scientific perspective, we believe it is unwarranted for a DFO scientist to be disciplined for publicly stating the conclusions of peer-reviewed published research, irrespective of the reason for which a scientist is interviewed by the media.

Doubleday et al. (1997) do not address our criticism that the 1995 Newfoundland and Gulf of St. Lawrence groundfish stock status reports (SSRs) inadequately reflected the breadth of scientific interpretation of available data. Despite the existence of at least six relevant papers, evidence that fishing was the primary cause of the decline of northern cod was excluded from the 1995 Newfoundland SSR, with the single reference to fishing appearing under the heading "Ecological Perspectives." Further to our point that SSRs that discuss stock collapses should "identify all relevant scientific documents upon which this research is based" (Hutchings et al. 1997, p. 1202), we note that the scientific information in the Newfoundland SSR quoted by Doubleday et al. (1997) is not supported by any of the four documents cited at the end of the SSR's overview. That SSRs are public documents does not negate Departmental responsibility to fully document interpretations of scientific data, especially when one considers that SSRs comprise the primary scientific source for the Fisheries Resource Conservation Council, the non-scientific body that now makes Atlantic groundfishery recommendations to the Minister. All SSRs must be approved by senior Departmental officials before public release, one of several important points in our perspective not addressed by Doubleday et al. (1997). The existence of such an information filter, irrespective of the frequency and the strength with which it is applied, is incompatible with Doubleday et al.'s (1997) laudable objective of increasing the openness of stock assessment reviews.

Regarding the 1995 Gulf of St. Lawrence groundfish SSR, we neither implied nor concluded that "seals are being blamed for the decline of southern Gulf cod" (Doubleday et al. 1997). Our point was that scientific conclusions regarding the influence of seals and the physical environment on the decline of a cod stock were removed from the SSR, contrary to the advice of scientists who conducted the stock assessments (Hutchings et al. 1997). Occurring *after* stock assessment peer review, we argued that this type of intervention was inappropriate, counter to scientific understanding of biological phenomena, and inconsistent with the objective of full public disclosure of scientific interpretations of stock assessment and research survey data.

In a separate comment, Healey (1997a) argues that our analysis is seriously flawed and that we lack objectivity, but he provides no justification for these opinions. He also notes that science is only one of several factors upon which management and policy decisions are based. This obvious statement

strengthens rather than diminishes our assertion that the scientific information presented to the Minister should be divorced from nonscience influences and that the scientific basis and ramifications of all management decisions should be made explicit and public. We concur wholeheartedly with Healey's (1997a) praise of the high quality of DFO science. However, we disagree that "science has relatively little to offer" in those situations for which there is high uncertainty. On the contrary, it is on such occasions that science has potentially the most to offer. Risk analysis, for example, permits many uncertainties to be quantified, thereby providing resource managers with a means of assessing the potential biological consequences associated with various management options.

Healey (1997a) agrees with us that "ministers and senior bureaucrats sometimes make decisions that are not very credible scientifically...and...sometimes seek to interfere with the free pursuit of scientific understanding by Departmental scientists," but he disagrees that greater independence of science from government would reduce the incidence of such decisions and interference. First, we note that with the exception of the first assumption mentioned in his discussion of scientific independence, the assumptions are Healey's (1997a), not ours. Second, his arguments here against the formation of an independent scientific body appear to be inconsistent with his recommendation elsewhere for "an independent auditor for the state of our [Pacific salmon] fishery resources...capable of reviewing the technical information on the state of fish stocks and making public comment on the resource" (Healey 1997b).

Our perspective ended with the suggestion that a reorganization of the link between scientific research and management of natural resources merited serious and open debate. The comments by Doubleday et al. (1997) and Healey (1997a) represent preliminary forays into such a discussion, and as such we welcome them.

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